DISSERTATION

Titel der Dissertation

What is there to be realist about?

The Epistemic Roles of Theoretical Entity Concepts

Verfasser

M.Sc. Miles MacLeod

angestrebter akademischer Grad

Doktor der Philosophie (Dr.phil.)

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Contents

I. Introduction

The topic of this dissertation suggests that this is yet another wading into the ongoing realism debate on the reality of theoretical entities. That is true, but only enough to wet the toes so to speak. What I say will have pertinence to a small aspect of it and only right at the end of the dissertation; namely that part of the debate which connects with the topics of incommensurability and the pessimistic meta-induction argument. Together these are considered to undermine the claim that scientific theories have been and are converging on the truth. Certainly they force a more critical view on the aspects of contemporary theories we can be realist about and those we cannot, and indeed of the whole direction of the realism debate in general. My claim is that what we can be realist about is much vaguer and more uncertain than we think, when the history of science and the essential dependency of scientific concepts on dynamic research processes is taken into account. Trying to assess realism on the basis of their supposed fixed meanings hides the dynamic role theoretical entity concepts themselves have played as epistemic units of research.

What I'm interested in is what I consider a rather neglected aspect particularly in philosophical circles of our understanding of scientific practice. Indeed the question of what the postulation of theoretical entity concepts has contributed to scientific research historically, and why in fact they have been postulated, are actually I believe prior questions to addressing philosophical problems such as reference-fixing, referential continuity and even realism, questions which have traditionally been approached first from a theoretical standpoint, and only later have drawn on the evidence of practice in the endeavour to justify or discount essential philosophical claims. This is unfortunate however for other reasons. Theoretical entity concepts are among the most interesting and most important concepts in modern science, which are witness to the very ability of science it seems to reach into the unobservable world and structure it in ways that have in many cases been hugely successful in producing successful theory and experimentation in spite of what might call our lack of epistemic access to this world. Something certainly interesting and potentially unique is going on with the use of these concepts through the way they drive scientific understanding and scientific research. In most cases they are postulated as central elements for the understanding of certain phenomena, and around which research processes are built and organised. Yet apart from some good recent philosophical and historical work, what we know about theoretical entity concepts and their contributions is rather limited, usually restricted strictly to the understanding of them as bundles of assertions about the world and the narrow concerns of the realism debate.[1] But theoretical entity concepts are more than this in the rough and tumble context of research and play many other roles, that go beyond just representing a thing and this is little researched, leaving a huge hole in our understanding which I'll claim renders debates like the realist debate somewhat abstract and misdirected.

As such the principal foci of this dissertation can be expressed as the roles of theoretical entity concepts in the history of science and the referential practices of scientists with respect to them. The former in fact will provide the detail for putting forward an account of, and theory of, the latter, but in itself makes a number of philosophical and historiographical points about the way we understand the role of concepts in the context of historical research processes. This extends to how we should understand what such concepts are. In this respect this dissertation has something novel to say, postulating that there has been a failure in our accounts of theoretical entity concepts so far to consider their essential functions as epistemic devices or tools in scientific practice. Concepts in general have epistemic ramifications. They are not used by scientists just as representations of the world, or to store data about the world, nor are they simply the outputs of research processes. They have important roles in directing those vary processes acting as resources and constraints for understanding, investigation, conceptualisation or theory-making, experimentation and explanation: all basic elements of scientific activity. If we wish good historical accounts of concepts in scientific practice then these accounts should respect and track the integrated relations between research processes and the concepts they rely upon.

Some concepts have been more important in this regard in general, and unobservable or theoretical entities have a strong history of being central to research processes. Histories of such concepts can be constructed around their epistemic value to particular research processes, and even told from this perspective. What such histories reveal is the methodological relationship of their use in practice between their role framing research processes and investigation, and their oft openness and flexibility of description for the researchers involved. Indeed part of their epistemic value can be derived from the dispositions

[1] See for instance Theodore Arabatzis, *Representing Electrons* (Chicago, University of Chicago Press, 2006); *Biographies of Scientific Objects*, ed. Lorraine Daston (The University of Chicago Press, Chicago and London, 2000); Hans-Jörg Rheinberger, *Towards a History of Epistemic Things: Synthesising Proteins in the Test Tube* (Stanford University Press, Stanford, 1997).

of scientists historically to accept re-descriptions of such concepts (or more properly, of their underlying entities) in order to adapt them to the emergence of new phenomena and new investigative paths which are the commonplace of continuous evolving research.

At this point reference enters the picture. By concerning myself essentially with referential 'practices' I'm taking a step back from the philosophical principles established somewhat normatively by philosophers on largely logico-semantic bases to point out what counts as reference based on linguistic (and sometimes metaphysical) considerations, and thus what counts as instances of referential continuity in the history of science, but not to scientists' own practices of retaining concepts or terms and abandoning them, and how they structure their beliefs in this regard. This gives us a more naturalistic standpoint to assess theories of reference and the realist debate, and it reveals that we have neglected an important aspect of scientific practice pertinent to them: that is, these *epistemic roles* of theoretical entity concepts in the production of theory, explanation, investigation, conceptualisation and experiment. The very functionality of these concepts in this respect has referential import as I'll try to show. Such a position is placed in opposition to those that tend to understand reference as a language-world relation bound up with the descriptions of entities and their ontological assertions.

Although it certainly needs to be spelled out why, this promises some benefit to the realist position and indeed something of an escape from what might be considered a largely abstract philosophical debate, without real grounds in research practice. As it turns out, it frees up referential continuity from issues of conceptual meaning, which have been the traditional mode of understanding scientific concepts in philosophical circles. Theoretical entity concepts have a history of being either open-ended or incomplete in their descriptions, or the subject of re-description and reinterpretation. Yet that this is often the case should not necessarily lead us to think that the reference of scientific terms are shifting ceaselessly about, such that the electron today shares no common reference with the electron of 100 years ago. It is part of the methodology applied to the use of such terms that these concepts are inextricable from the research processes they help develop and that in turn shape the representation of the concepts themselves. As such the electron concept of today and yesteryear are historically related through these processes which they have had a continuous role in framing. This is despite otherwise quite radical shifts in its meaning. Something deeper is going on in the way scientists use these concepts epistemically and refer to them despite representational change.

All this starts with the observation of scientific practice itself, and something that seems from a philosophical perspective, especially in the context of various pronouncements on the way reference works, to require explanation. Indeed my starting point is to draw out the problematic elements that result from the traditional philosophical approaches to reference and conceptual continuity, and the treatment of concepts generally, which have proceeded more from the analysis of language rather than actual practices themselves. In fact we can identify certain underlying presuppositions which tend to co-occur behind this treatment particularly of theoretical entity concepts:

1. These concepts are elements of claims about the world as representations.
2. They are essentially linguistic structures analysable chiefly within the philosophy of language as ring-fenced containers (defined by their content).
3. They are to be considered as outputs of research distinct from the actual processes of research.
4. They develop, and are compared at different times, in terms of their content.

As a result traditional theories of reference focus on the meaning of concepts, treating them as ring-fenced containers, leaving certain referential practices of scientists inexplicable at best, or irrational at worst. Incommensurability is an ever-present danger. Yet the assumption is that despite what philosophy tells us should count to fix reference, science has its own various criteria for which they have their motivations, which have in turn emerged historically from their need for methodologies with which to meet the epistemic goals of their research and further them. That is the starting assumption at least of what I have to say about these referential practices and also the continuity of theoretical entity concepts.

Four aspects of these referential practices stand out;

1. Scientists have in many cases persisted in using a concept, and maintaining a belief that they have been continually redescribing the same thing, even though such redescriptions have involved significant and dramatic shifts in the representations of the entity involved. (e.g. various subatomic particles, the gene concept)
2. Significant aspects of an entity's representation may remain unsettled or unexplained. These may pertain to significant ontological questions about the entity, such as what

natural kinds it belongs to, how it is internally structured, what might be said to be its 'nature' or constitution (which might require theories of interpretation), or how the collections of properties or causal powers assigned to an entity might be explained where there is a want of explanation (with models of internal structure or models of behaviour – see continental-drift theory). In which case it might be said that there is no real clear picture of what the entity is, or what provides its causal powers, beyond the bundle of properties associated with it and the implications of those powers. Certainly there is nothing that might be classed as an essential description of the object that might be an answer to the question 'what physical properties of X uniquely determine its extension?'(the initial gene concept for instance, the early virus). It simply might be left open for lack of information. Yet lacking these kinds of descriptions does not seem to stop scientists believing their entity concepts refer to something in the world.

3. In other instances there might be disagreements over such descriptions or interpretations, but still significant agreement that there is a thing being referred to independent of these and despite this debate (atomic structure debates, models of the electron). Thus a kind of principle of charity operates. Scientists implicitly agree that they are talking about the same thing yet disagree over significant aspects of it.
4. In some cases scientists think that, where there has been an acknowledged conceptual shift and an entity replaced by a new one in their explanations of a certain phenomena, they regard the old concept as simply referring to the new, as though they were all along referring to the new even while describing aspects of the old. (e.g. the aether and the EM field). In other cases they don't (e.g. phlogiston and oxygen).
5. Theoretical entity concepts seem to share the same reference ascribed by scientists even across disciplinary boundaries and the different uses and descriptions of the entity given by those various disciplines (e.g. the chemists' and the physicists' electron of the early 20th century, and historically before the 60s, genes across molecular and classical biology)

This seems to indicate that for scientists the representations or characterisations of their entities haven't necessarily been fundamental to the entity concept. For them a continuity of reference, a belief that they are describing and re-describing the same thing, comes through other less obvious means. An entity concept can be substantially open-ended; flexible in the understanding one can have of it and open to interpretation and reinterpretation. But why? Why should this be the case when it has seemed to many philosophers (from Frege onwards) that changing descriptions should rationally lead one to give up on any belief that the same thing is being described? Well the answer has to take account of the fact that other aspects of entity concepts other than descriptions play an important role in referential practice. To preserve substantial space for description is in fact a distinct and essential part of the methodology of science when dealing with complex often inaccessible systems, which can place its roots in quite minimalistic assumptions about how the world is, but leave the more substantial details to investigation or correction. At the same time these concepts have guided and shaped the scientific process, by *framing* objects of investigation, and providing objects for the investigation and explanation of other phenomena. Such concepts have served as inherent parts of developing research processes and played roles as resources and constraints for those processes. Perceived in this way a philosophical methodology that simply extracts such concepts out of research processes analysing them distinct from it, misses this important epistemic dimension of their use, and their association for with the goals and objective of ongoing process of research, and hence misses the fact that scientists have aligned themselves to such concepts on the basis of their epistemic value to developing and evolving this research. These concepts in certain aspects provide structure to essentially dynamic processes and thus methodologically anticipate development and accommodate re-description. The question for us is what these aspects, that we've labelled these epistemic functions or roles, are and how scientists use these to ground world-language relations such that they could be said to sustain reference. Whatever the answer is it must relate to the dynamics and demands of research, not language. The promise is an account of reference faithful to practice which at the same time accounts for the essential fluidity and flexibility of these theoretical entity concepts, and their space for development.

I.1 Dissertation Structure

The structure of this dissertation is as follows:

Chapter 1 argues for the necessity and value of an epistemic account of these scientific concepts in order to evaluate their historical role in scientific practice. This is not to invent a completely novel perspective on theoretical entity concepts. There exist strands of philosophical and historiographical literature that have for one reason or another treated concepts in this way, particularly in the case of theoretical entities. These serve as useful starting points for developing this approach, and provide justification for it (other than for the sole purpose of understanding referential practices).

Chapter 2 attempts then a clarification of what could be meant by the epistemic roles or values of theoretical entity concepts, and indeed historically how we might categorise them. It provides a loose framework for the rest of the dissertation, particularly a basis of analysis for examining the roles of such concepts historically, and understanding how individual cases have contributed centrally to their processes of research. I suggest in fact that we can picture these roles together as well represented by describing theoretical entity concepts as *epistemic frames of research and investigation*.

Chapter 3 applies the theory of chapter 2 in respect of the classical gene concept up until roughly 1953, one I think of the clearest examples of how such concepts have functioned epistemically in a research process, in the attempt to understand heredity and development. What is particularly instructive is the historical illustration the gene concept gives of how scientists incorporated openness and flexibility of the concept into their use of it, to create an epistemic tool to guide their investigation, theorising and experimentation in circumstances where little was known or understood about the biological and chemical processes at hand. I'll argue specifically that describing the gene concept as akin to an epistemic frame of investigation helps us understand its central contribution to this research. Further this use of the concept was supported in this instance by what I will call an *open-concept* realist attitude on the part of many scientists involved, which justified their reliance on the concept to frame their activities on the basis of a belief in the existence of a gene entity.

Chapter 4 provides the second case study, which is that of the neutron concept from the 1900's up until 1939. We will see that like the gene we can describe its contribution to the study and examination of nuclear forces and nuclear structure in terms of certain epistemic roles, which guided experimental investigation and theoretical development. Like the gene fundamental or central aspects of its description or representation were not settled, but the subject of debate and reformulation in the course of this research. In this case I will track more precisely, through a more detailed historical narrative, how the development of the neutron concept and research into the nucleus for researchers was intertwined through the agency of the neutron's epistemic roles. Treating it simply ontologically, as a collection of representations, ignores this underlying dimension of participation and continuity.

Having considered these case studies and the historical importance of epistemic values to the roles of these unobservable or theoretical concepts in historical practice, the last two chapters deal with the philosophical implications. Chapter 5 argues that what emerges out of these case studies and other examples are practices of conceptual continuity and referential continuity that seem to rely fundamentally on the epistemic roles these concepts play in their research contexts. The key point is it seems is that researchers centralise these concepts around the elements that help structure and progress their research, and which are to an extent constitutive of this research, being basic necessary aspects through which they can frame their understanding of the phenomena and their approach to it. As such conceptual continuity and reference is governed in practice by these epistemic roles and their essential properties, upon which research depends.

Lastly chapter 6 considers the philosophical debate over the reference of theoretical entities, and argues that theories so far developed do not really start from an understanding of referential practices or the part played by their embedded nature in ongoing research processes. The norm is generally to start from considerations of language and try to work that into a theory of reference that fits practice, rather than starting from practice and seeing what motivates it directly. We will consider then the question whether or not we can from these theoretical positions derive a rational basis, or more precisely rationale, for this practice of treating such concepts as epistemic frames that could be said to ground language-world relations, and thus compose a *theory of reference* for historical theoretical entity concepts in the research context. If this carries through then we have some way of understanding the continuity of theoretical entity concepts despite meaning change in line with the often realist interpretations scientists themselves have. We can understand continuity as part of an investigative process into the same underlying entity, whether that entity turns out to exist or not. As such we have not only the basis for an objection to anti-realist arguments that begin from the supposed discontinuity of the reference of these concepts but also for the recognition that the whole realist debate has proceeded from a noticeably abstract position, in which terms and concepts have been generally separated from their research contexts. At the same time however we have to accept that what we can be realist about needs to fit with this more investigative and developmental functions of these concepts and their open-ended character, giving us a picture of what we can know that is much more vague and uncertain.

I.2 But what are theoretical entities?

As mentioned above my main concern is not with scientific concepts in general but with historical and philosophical accounts of theoretical entity concepts. These of course have a history of being among the most interesting of the family of scientific concepts to philosophers, particularly in their incarnation as theoretical terms, but also some of the most vexing. They represent that most exciting and powerful capability of research to reach supposedly into the deeper elements of the world, through reasoning rather than pure technology, exposing not just the world's wonder and complexity, but also the power it seems of human abilities. But they also represent philosophically some of the most frustrating and least understood elements scientific ontologies. The task in this section is to deal with the difficult issue of what these are and what the motivation could be for choosing to group these concepts and their underlying entities in this way. I'm hoping not to get engaged in a dispute about how exactly to define these, because that's probably impossible in any precise way, but I do want to point out some important characteristics of theoretical entities which will help with our analyses later on, and so we can be relatively clear about what class of concepts I'm generalising over.

I.2.1 Defining Theoretical Entities

In general the concept 'theoretical entity' has a rather poor and unclear history in philosophy of science. Often a paper talks about 'theoretical entities' casually listing examples like 'atom', 'electron,' 'gene' and 'field', leaving it unclear as to the extension of the term, but relying on some idea of the postulation by theory of some *thing* that is unobservable. This leaves open questions about whether something like a process is included or a force (as opposed to a field) or any of the large class of states and properties, as well as kinds and categorical terms like Newtonian System. Together these things cover what might be called the class of 'theoretical terms'. It's fairly clear that philosophers don't mean by theoretical entity just anything from the class of theoretical terms (terms without observable reference). They have something more refined in mind. But it is difficult to specify this.

Realists often implicitly do so to an extent at least by discussing theoretical entities in terms of an analogy with observable things to give sense to the claim that an unobservable thing can be real. The observable world is of course our immediate grounding and source of information for what it is to be 'real'. Realists argue that for something unobservable to be real it must be in the same sense for which observable things are. When we assert an electron is real we mean that in some sense it is like an observable material object such as a chair. Likewise an unobservable process is real in the same sense that an observable one is. If you're sceptical then about considering a process as a real thing, then of course you won't find the unobservable versions any more real. Thus the real world can be a guide to what you consider can be real at the unobservable level. This is an appealingly intuitive approach to giving sense to the reality of unobservable or theoretical entities, and thus giving meaning to the claim that unobservable postulates represent real things. After all the idea that a thing is in some sense composed of its properties, and properties themselves if they co-occur can identify a thing, comes from our familiarity with the observable world. Unfortunately it only works so far. One may debate whether there really is any sense in which a field (force field) compares with something observable. There is a huge difficulty equating subatomic particles with real objects in any straightforward way. Standard notions about material substances break down at the microscopic level.

Happily my concerns don't require me engaging in a long-winded discussion of this. My approach doesn't require me to give strong ontological detail of what my class should or shouldn't include. After all such ontological questions are to some extent the providence of empirical research, the kind of questions I claim are framed and investigated with respect to established concepts. I am operating primarily to group these concepts via elements typically important to their historical epistemic roles in research processes.

I.2.2 Some ideas about Theoretical Entities

So what I'm going to do here is specify some aspects of what I mean by theoretical entities. The first remark is that I've chosen to stick with the nomenclature of 'theoretical' entities. Arabatzis chooses the term 'hidden entities' for his recent discussions of these objects.[2] He tells us the reasons are so as to avoid raising the vastly problematic unobservable/observable distinction with notion of an 'unobservable entity', and also to avoid giving the impression that that hidden entities don't transcend the theoretical frameworks in which they are embedded. I think this is perfectly reasonable. For me though it's important to emphasise the

[2] Theodore Arabatzis, 'Hidden entities and experimental practice: towards a two-way traffic between history and philosophy of science' (unpublished).

actual ‘theoretical’ nature of such objects which translates to a lack of knowledge and uncertainty regarding the entity, and the unobservable world in general which it attempts by its supposition to give some structure to. It informs the very methodology by which they have been historically used and which I will outline in the coming chapters, as elements which are accessible only through the medium of theoretical assumptions and as such are treated open-endedly and flexibly in terms of their descriptions, but provide researchers with a resource to begin to structure the world and upon which theoretical positions can begin to be constructed. As such I don’t think that these entities never transcend their theoretical frameworks. Conversely, their principle role is often their contribution to theoretical frameworks, and the way in which they contribute is partially transcendent itself. Theoretical in this sense refers to their function rather than their nature. For these reasons I think ‘theoretical’ is reasonable way of describing these entities.

Here then is a list of some otherwise important aspect of theoretical entities and entity concepts as they have historically arisen which help give sense I suppose as well to the notion of ‘entity’, and which have certainly been important to their use.

1. They are *causal agents*. They act through their properties causally on other bodies, systems, processes etc and thus can provide causal explanations.
2. They are or were once *unobservabl*e (in which case they are historically studied during the period before they became observable) or only *indirectly observable*.
3. They (the elements of the terms in question) occupy generally *a spatial and temporal location*. Although this may not be well defined in terms of precise values (this may be impossible as in quantum theory), it is clear that they exist ‘in the world’, as part of the world and occupy some space-time region.
4. They seat certain properties. They serve to combine or link various properties simultaneously, and in ontologically neutral terms are packets of properties. They are not themselves properties or quantities.
5. They are natural kinds.

The aim with these criteria is to capture the sense of entities as physical stuffs or objects which are investigable through their position in or occupation of space-time and which can influence the world around it causally, while at the same time have the metaphysical basis to support property ascriptions and support generalisations regarding entity-members. It is deliberately general enough, that it captures such things (which may or may not still be unobservable) as genes, classical fields (EM – field, gravitational field) if they are considered unobservable, maybe quantum fields (which are controversially physical), subatomic particles, viruses, atoms, unobservable planets, black-holes, the aether, phlogiston, caloric, Wegnerian continents, diseases, systems with causal agency (various biological systems for instance) and events. It might include processes, at least those with a sense of location in the world and causal agency (thus not 'evolution' but maybe 'chemical reactions'). On the other hand it probably doesn't include things like forces (with no real sense of space-time occupation: fields provide this sense), mathematical abstractions without physical interpretation such as the quantum mechanical wave function ψ, and the whole class of terms that apply descriptively to classes of different objects but aren't objects themselves, such as 'conservative systems', or Newtonian systems, or reversible processes. These rely on there already being something about which you are speaking which counts as a causal agent entity. They are more like properties.

Although this will be brought in more detail in the next chapter, let me give some insight into the criterion chosen. Each in some way supports the epistemic use of these concepts. This is elaborated in much more detail in later chapters. For instance what I have to say about epistemic function is often linked to the formation and postulation of entity concepts as *causal agents*. This is expressed through explanatory functions, but also theory formation, the manner in which they are detected, investigated and identified experimentally, and the broader manipulability of entities in experimental contexts of which Hacking discusses.[3] There is thus seems to be a basic methodological link across the sciences between the postulation of theoretical entities and the presumption of the causal structure of reality which operates beneath the observable level. Referential practices are often directed at loosely and broadly marking out the causal relations and causal structure with entity postulations that are relatively open to investigation and description. Broad causal relations provide some of the tools to establish these broad referential links (see chapter 6).

Likewise the other properties all share epistemic virtue, which I should give at least a brief outline of here. The ability to collocate spatially or temporally properties is fundamental to the use of many entity concepts 'conceptually', and has been in many relied upon to fashion an entity concept to fit its explanatory purposes (see chapter 2). As I'll suggest in chapter 1 one

[3] Ian Hacking, *Representing and Intervening,* (Cambridge, Cambridge University Press, 1983).

way of considering what 'natural kinds' are is in epistemic terms, is as group over which scientists can generalise inductively. 'Natural' need not be spelt out ontologically, which has proved difficult to do, but rather in terms of the belief of scientists about the relations between members which gives them a basis to rely on the concept in this way.

Spatial-temporal locality which I would also call 'thing-likeness' has played a role in the way scientists have pursued their further investigation of the world. The assumption that the object in question can be found in some locality and is bounded by the world in some way (distinct from the background) prompts investigation into how to distinguish it, find it, identify it or locate it. These concepts structure investigation of phenomena by providing a conceptual basis for it, relative to which the world can be examined.

The category of 'unobservable', and also for that matter 'theoretical', have a difficult history in philosophical discussion and are not terms that can be used or should be used with any kind of strict precision.[4] There is no strict line separating unobservable from observable objects because it depends what you consider to constitute instances of observation. Many physicists for instance take a particle track in a cloud chamber to be an observation event of that particle, and thus consider the particles 'observable', even though of course no one actually has a direct visual image of the particle. Likewise planets around distant stars are often said to have been 'observed' when its star is seen to shift. While of course the common usage of a term need not coincide with what is meant philosophically (the sense in both case seems more like 'detected'), it is clear in this case that there is an issue here of where to draw the line and whether or not a line can be drawn. In fact in the case of 'observation' various epistemic categories like 'observed', 'detected', 'measured', 'deduced', 'induced' or 'abduced', get run together and as such observation or degree of observability is a matter of the directness of epistemic access one has to the object in question. The indirectness of the cloud chamber example should not thus automatically mean that this does not count as an observation, only that it should to be a lower degree of observation than say witnessing an event in the street.

However strict unobservability is not the basis I'm working with here. It's more important that epistemic access to the underlying entity is mediated by theoretical elements, such that

[4] See particularly Achinstein; Peter Achinstein, *Concepts of Science* (Baltimore, John Hopkins Press, 1968), pp157-167.

the information about it is in some way dependent on factors that are open to change in the course of research, and such that information about the underlying entity is limited by the constraints of what can be derived through these elements, often perceived as generating only limited or incomplete accounts of it. This is what necessitates the kind of methodological response to the use and management of theoretical entity concepts that I'll try to detail in this dissertation. The way in which they can be investigated further themselves relies on the mediation of theory, which is less the case with more observable objects which have the mechanism of perception through which to gain information about them, and verify statements with respect to them more directly. It still is however a matter of degree depending on where these entities lie on theoretical scale, and although I don't explicitly argue this in this dissertation, it is probably the case that the extent to which this methodology is relied on depends on the extent to which these terms are only indirectly 'accessible'.

Of course my interest in this dissertation is saying something historical and philosophical about theoretical entity concepts, which have long been an interest of both philosophers and historians precisely because of their unobservability. In any case none of these properties are meant to be strict criteria for demarcating or identifying this set of concepts. They are properties that in my research seem pertinent to the epistemic use of this class, and thus serve to group them epistemically such that we can say something general about them and form an historical perspective with respect to them. Each property applies to a greater or lesser extent. What matters for us fundamentally is the epistemic use of these concepts, and how this use takes advantage of these properties, or is shaped by them to the extent that they hold. What we find is that these properties do matter in this respect and even for discussions about reference.

I.3 Final Notes

This said, I don't wish to go overboard about what I can achieve or argue for in this dissertation. I'm happy if I can offer a novel way of assessing the contribution of theoretical entity concepts historically, and an alternative account of referential practices. The real aim is to motivate these with a degree of historical evidence, rather than provide necessarily watertight argumentation. Most of the hard work properly refining the concepts involved for instance and giving more full blooded accounts of the historical cases in accord with these forms part hopefully, if any of this sounds at least promising, of future further work.

Chapter 1: Epistemic Roles – A History of Considerations

A principal focus of this dissertation is the referential practices of scientists with respect to theoretical entity terms in the context of research processes. Reference however is a philosophical concept, not a scientific one. There is no scientific theory of reference per se, although individual scientists may have something to say about it, in a very generic sense at least. As a result referential practices have to be teased out, by looking at the broader methodological strategies and positions they engage with, and more precisely, with the way such terms are in fact used by the scientists that use them. This extends beyond what the scientist might say about definitions and meaning. Scientists themselves present their terms and concepts in different contexts in different ways. The research context is a different environment from that of a textbook for instance, and the notion of concept relied upon is different in each, as we'll see. As Achinstein puts it with respect to the scientific meaning of concepts, what the scientist says about them may be precious little and often misleading, and "one will often need to dig beneath the surface…."[5] Such a philosophical methodology extends to the study of referential practices with respect theoretical entities. We need to examine historically the uses of these concepts, and only by having an account of the methodological practices that have been associated with such entity concepts or terms first, can we begin to see what these practices of reference might be. I will argue that for theoretical entity concepts relations of reference are associated quite strongly in the context of ongoing processes of research, with their *epistemic functions or epistemic roles.* This awaits however chapter 5!

But reference is only an aspect of the broader historical and philosophical concerns raised by a consideration of the epistemic roles of theoretical entity concepts in practice and scientific concepts in general. What I want to assert in this chapter (and the next) is the relevance and importance of have epistemic account of scientific concepts in general as ways of piecing together and understanding their role and importance in scientific practice. This kind of project tries to build up a general picture of the kinds of epistemic roles or functions that concepts have provided in the context of research processes. It has long been recognized for instance that theoretical terms have played methodological and epistemic roles in theory construction. They are more than just ontological posits or discoveries. No one need be reminded of course that the Vienna circle had a strong interest in the methodological function

[5] Peter Achinstein, *Concepts of Science* (Baltimore, John Hopkins Press, 1968), p.109.

of such terms. The attention to such functions has come up at various times in one form or another. Most recently Hacking has pushed the notion of an entity as a tool in experimental circumstances.[6] In the history of science there is now a greater perception of the importance of having some account of the role of scientific object in scientific practice. There are developing accounts of what entities in this respect have contributed to science. We find in the historical literature the perception of entities as active agents in their own histories, a perspective that stems partially at least from their central methodological and epistemic functions in regulating and directing the activities of scientists. All these I will discuss as motivations for what I have to say on the topic.

However while these various projects have explored the methodological function of theoretical entities, none have done so as a subject in itself, rather than working towards some other goal. Hacking for instance wished to argue for the primacy of experiment and the reality of 'experimental entities'. The logical positivists and empiricists were interested primarily in the relation between unobservable and observables and ultimately the meaning of theoretical terms. Historians have pursued scientific objects for historiographical reasons, principally for developing new ways of writing the history of science that take account of the primacy and centrality of their role and certainly of their active contributions. Historians have yet tried to give a structured picture of what these values are, if there is some general picture to be had across the historical examples. I think in fact some of these epistemic roles and values underlie the very basis of writing histories in the way, from the entity's perspective so to speak.

However the more significant hole lies in the philosophical discussion of entity concepts and in fact scientific concepts in general. Generally the consideration of theoretical entity concepts has been driven by the realism debate and its goals. There has thus been much attention towards the truth value or otherwise of theories involving entity concepts, somewhat ignoring the life or role of entities beyond acting as ontological posits about nature. To an extent we have Hacking's experimental realism to thank for attempting an argument for realism based on the notion of an entity as a tool rather than as a theoretical assertion. After all theories rarely directly posit the existence of things as the final line of a calculation. They use entity concepts and their properties to construct theories and experimentation towards the goals of

[6] Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge, Cambridge University Press, 1983).

their research. Entity concepts are bound up in the mechanics of active ongoing research as resources and constraints for them. This is the very basic but formative insight of this entire dissertation.

Most philosophical problems which rely on the nature of science (descriptions of its methodology for instance) must of course at first have a reasonable account of scientific practice, and this account needs to be historically aware of the dynamic and evolving character practice. Such a statement is as close to fundamental as any is. In any case it is a goal of philosophy of science in itself to fathom this practice and reveal the methodologies that have historically driven it. I claim that these historical and methodological goals necessitate a consideration in practice of the epistemic roles of entity concepts and in fact scientific concepts in general, which have shaped research processes. Modern scientific practice has made methodologically central the resources and constraints that its concepts provide. Concepts and conceptual frameworks have provided the means of research and investigation, not just the end products of it. Realisations such as these point the way to deeper revelations about just what such entity concepts do for science, where there is an unobservability or lack of direct visibility, of the causes involved. These reveal at least for these concepts I believe a set of broadly shared epistemic values and an underlying methodology with which scientists use them to approach the unobservable and hypothetical, including importantly, the open-endedness with which they have typically treated them.

The purpose of this chapter is principally then to motivate the analysis of scientific concepts, and theoretical entity concepts in particular, in terms of their epistemic contribution to research processes. The way I will do this is by drawing from the historical and philosophical literature that has itself pursued this topic, if indirectly. The insights from this literature is almost invariably useful for what I have to say. Indeed these can be drawn together, not only to justify or motivate a general program of such research, but also to provide the framework for it…i.e. the set of important concerns and issues that it aims to deal with and should help elucidate.

This chapter then has the following structure. In the first section we give meaning, briefly at least, to the notion of ‘epistemic role’. The second argues specifically for an *epistemics* of concepts, particular theoretical entity concepts to understand their role in research processes by drawing together historical streams of thought that have considered the epistemic value of entity concepts. The third draws these historical discussions together and tries to formulate in general way the considerations that should guide such investigations towards providing historical account of particular concepts and conceptual frameworks. In other words we seek out the language appropriate to interpreting their history in terms of their epistemic roles and evaluating their participation in the development of research processes.

1.1 Epistemic Roles: The Basic Idea

How do we make sense of the idea that concepts, especially theoretical entity concepts, have provided epistemic roles for researchers? Although I will develop these ideas more in chapter 2 let's start at least with a basic consideration of what they might be. What I mean to describe by the epistemic function or epistemic role of a concept (entity, property, relation, kind or theoretical term in general) in a research process, is a role that concept plays in achieving or furthering the epistemic objectives or aims of that process. Here epistemic objectives are thus defined within the context of the aims of particular process of research, but could be construed generally as whatever would contribute directly or indirectly to the improvement of knowledge or understanding about the natural world. This includes the development of new theories (conceptual frameworks), experimental set-ups for probing established phenomena and the production of relevant new phenomena, new explanations and predictions, and also judgments of belief, in the sense that terms and concept may have a function testing theories. It is essentially then its productive function which makes it 'epistemic' in so far as it helps discover new facts about the world and/or provide new understanding. It is also a confirmatory one in the sense of helping confirm postulates. This doesn't mean that the concept must be productive in the sense of directly giving new predictions, or that it be straightforwardly constructive by bringing with it building blocks for theory or experiment. Simply by constraining or influencing the direction research can take a concept can exercise an epistemic function.

What I'm aiming to give then with this conception of an 'epistemic role' is some structure to the belief that historically theoretical entity concepts, or scientific concepts in general, have participated in the development and pursuit of a process of research. This said, my idea of an 'epistemic role' is not meant to capture all the ways in which concepts have interacted in this way, but rather those roles that have worked at a basic or 'outer-level' if you like to help constitute research. These I believe as we'll see are the roles that are important for understanding conceptual change and the historical continuity of theoretical entity concepts. As such the following properties will characterise what I mean by an epistemic role. These are:

- Defined relative to the goals of an accompanying *research process*.
- Not of themselves determinative of the conclusions scientists reach in complex circumstances or what they find.
- Subject to interpretation in practice (but in this way still act epistemically as a frame for interpretation).
- Formative of research: to a degree independent of the development of research as they are part of the very structure by which that research is run.

A research process then is defined by its epistemic objectives and by virtue of these the epistemic roles of a concept can be historically identified where they exist. I don't think one could define the epistemic role of a concept free of the particular research in which it is being utilized because by definition an epistemic role is directed at a purpose and these purposes themselves are defined in some context, which is the context in which it's being used. These objectives countenance generally the achievement of some form of understanding of a phenomena or resolution of a particular theoretical problem or indeed a family of problems. Genetics for instance worked towards the understanding and prediction of the phenomena of heredity and development. Nuclear physics worked towards the understanding of the structure of the nucleus and the operation of nuclear forces. One can be more specific than this, but only in so far as it's necessary to identify the relevant research context for which a concept was postulated. Research processes evolve their specific goals within these frameworks, but usually these motivations stay relatively permanent. Of course if the goals are achieved then these processes cease to be operational. The concept of course might carry over into new processes and thus acquire new goals. Note by raising this idea of a research process, we do not at the same put *a priori* requirements on the levels of nature that scientists must work at to achieve these goals or understanding. This is itself historically an open question for scientists. If they need for instance to delve to lower physical levels to understand a particular phenomena or resolve a particular problem then that will happen in the course of this research, as it has in many cases such as genetics and nuclear physics. Further it's a mistake to think that a concept's advent can in any way be identified with the resolution of those goals. That just hasn't happened historically. There are always issues that remain, always more problems to solve. As I'll emphasise later particularly in the case of theoretical entity concepts, conceptual construction gives generally piecemeal solutions to particular questions that research has thrown up. In many cases the concept itself is productive of new directions to that research or in fact new problems. A reason to keep the idea of a research process general, is not just that historically we can identify them as such, but also that its stops us confusing this process with the concepts employed by it and gives us a relatively stable framework for the assessment of those concepts. What are the important questions or avenues of investigation, and the conceptual frameworks for that investigation, considered part of achieving these epistemic goals, are shifting all the time, but shifting in way that is generally historically separable from the actual goals and motivations of that research that give those shifts, as alternative ways to reach these goals, motivation themselves. I think this claim fairly reflects what is going at least in the case studies that follow.

The latter three points capture the idea that epistemic roles should acts as means for a research process, in a sense being constitutive or formative of it, but not determinative of the particular paths it must take or conclusions it must reach. Put in this way, these roles allow the reliance on a concept to be expressed in different ways as a research process evolves, and new information and new phenomena is encountered, through their changing interpretations, but also through the changing background theory which itself is part of how such epistemic roles might be applied and understood. Indeed interpreting roles as well see is itself an active part of what drives a research process, in which case these roles play a significant investigative role framing the direction it takes and relevant issues for it. At the same time what arises from employing these roles, is not determined by the roles themselves. If a concept is relied upon to generate theoretical positions then the way this might be done depends on the problems that might need to be solved, the other theoretical tools available and so on. Even in the same context they can be jointly relied on but to reach different conclusions, just as a physical principle might be at base of different physical theories of the same phenomena. They underdetermine in other words the results of using them to generate theoretical positions for instance, but also how they can be used in experimental contexts. This allows again of course for the changing environment of a research process in which new information is arising all the time, and theoretical responses often proceed in a piecemeal and partial fashion, relying on varying and alternative approaches to work towards better positions in the face of complexity and uncertainty. These roles provide a means for doing research, not what that research might find or what it might conclude. In any case these features seem to me to be the essence of what we should mean by an epistemic role, as a tool of research, and I believe as such it captures an important aspect of the historical reliance on concepts, particularly theoretical entity concepts.

Let me single out five epistemic roles which help I believe at least express in a more structured way the epistemic contributions historically of theoretical entity concepts in the context of ongoing research processes; namely explanatory, inferential, conceptual, experimental and investigative. These aren't meant to be exclusive or exhaustive, they are meant more or less as a basic framework for talking about and systematising the epistemic import of these concepts. In fact they overlap each other. Inferential uses are often explanatory uses; experimental uses are often investigative. But they each select some salient and identifiable aspect of their use. Nor can we ignore the fact that these roles themselves require generally an amount of interpretation using background theoretical or experimental elements to make them practical in a research environment, as we've already suggested. But we'll note the important fact that generally these roles rest upon certain property ascriptions in the case of theoretical entity concepts, which are necessary to, or even constitutive of, those roles. We'll see for instance that the unit structure of the gene, was fundamental to many of its conceptual and explanatory uses, and thus even today because its epistemic centrality to constructing theories with the gene and thinking about genetic material, part of the understanding of what a gene is. It is to extent constitutive of theory-building and conceptualization with the gene, and without which its epistemic value to research would have been arguably non-existent.

Explanatory roles probably don't need much additional information. They countenance the particular phenomena that a concept is associated with as an explanatory element. This is often of course how concepts get constructed, with particular explanatory goals in mind. The concept of the continent was invoked for instance to explain geological phenomena in the earth's crust. The virus was invoked to account for infectious agents so small they could pass through bacterial filters. Inferential roles reflect the participation of concepts in forms of reasoning and argumentation such as induction. Concepts provide the basis for instances of inductive generalization, often as an element over which generalization can take place (think of a kind term for instance like 'lead' or 'virus'). Conceptual refers to conceptual uses such as providing a resource or constraint for theory construction or the construction of conceptual frameworks. Of course it is often said that some kind of premise or supposition has 'conceptual value' for scientists by providing them a basis of understanding. This understanding is of course itself relied on in the development of theory and conceptual frameworks. Experimental refers to their use in constructing and designing experiments, and as a tool in experiments (see the section on Hacking). Investigative refers to the role concepts have through their postulation raising issues and problems that require further investigation and resolution, such as the inner structure of an entity, and means for managing that investigation which rely generally on the application of these other roles, but have their own particular goals and purposes.

1.2 Why Epistemic Roles? The Historical and Contemporary Lessons

We have here then at least the basic outline of what it means in my terms to assess the role of concept with respect to a research process epistemically. The question is why should we do this and what is the value or necessity of doing so? I think this question has to some extent been answered historically or is being answered in contemporary historical and philosophical literature which sees the value of this approach to understanding scientific practices. Thus I propose we look to these historical and developing strains of thought first and thus develop some context and motivation for it.

1.2.1 The Logical Positivist Debates and the Meaning of Theoretical Terms

The debates arising from the Vienna Circle provided the first philosophical discourse where the question of the role of concepts – in their incarnation as theoretical terms – came under close attention. As is well known one of the principal subjects of a debate that started early in the century and particularly during the Vienna Circle, and after it into the 40s and 50s, was the meaning of theoretical terms.[7] Such terms were defined generally by their lack of correspondence to something observable. Carnap for instance famously split the language of

[7] While it is not my aim here to give an exegesis on the Vienna Circle in this section it is important to point out that work of the last 20 years or so has taught us that our understanding of their work and their debates, and the sheer diversity of opinions and ideas that reigned, makes the traditional attempt to fit the Vienna Circle into the straitjacket of the 'received view' untenable. See Friedrich Stadler, *The Vienna Circle* (Springer, 2001). Here what I say will show the actual relevance of the Vienna Circle thinking on theoretical terms to our study and thus challenge the opinion that the Vienna Circle scholars were divorced from actual methodology and practice. In fact they had a very good sense in some respects of how science worked with theoretical concepts. This has been unfortunately somewhat ignored in the post-positivistic era.

science into the language of observable and the language of theoretical terms.[8] The logical positivist criterion of meaning, which attributed the source of meaning alone to testable observation statements, raised the issue of what the meaning of theoretical terms could be, given they lacked any direct observability. The classic Carnapian position was that such terms could only be partially interpreted through the observation statements to which they could be in some way deductively tied. But the question was, given this partiality of meaning, what were theoretical terms doing in theories, if it wasn't to represent something that actually existed, and whether they were actually necessary at all. This led somewhat naturally to investigation of the methodological roles of these terms. In general Carnap conceived that it was a general method of science to employ terms without complete interpretation or definition, where this incompleteness itself had methodological value in terms of an ability to attach new correspondence rules to terms as required.

This kind of thinking occurred in the context of the challenge first set by Russell as to whether or not theoretical terms could be eliminated from theories and replaced purely by an observation language. This was referred to as the 'Theoreticians Dilemma' by Hempel.[9] It prompted an effort to show by philosophers inside and outside the circle that such reductions were not in fact possible because of the essential roles such terms played in theories and the use of theories. The observations they made in this regard more or less pertained to what I would call the epistemic function of such terms and more particularly entity concepts. It is possible to construct a sophisticated list of epistemic uses of theoretical entities from these writings.

Lewis White Beck in his article entitled 'Constructions and Inferred Entities''[10], declares that the inferred entity concept serves three purposes in science "(i) It 'summarises' the observed facts. (ii) It is an ideal object for search, i.e. it provides a goal for the improvement of observations so that what was formerly unobserved may, if real, become an object of observation. (iii) It is a basis of predictions of further facts and 'explains' them." The first role was also described under the heading of the 'intervening variable' posed by MacCorquodale

[8] Rudolf Carnap, "The Methodological Character of Theoretical Concepts" in eds. H. Feigl and M. Scriven, *Minnesota Studies in the Philosophy of Science* 1 (Minneapolis, University of Minnesota Press, 1956), pp. 38–76.
[9] Hempel, C. G., "The Theoretician's Dilemma", in eds. H. Feigl, M. Scriven & G. Maxwell, *Minnesota Studies in the Philosophy of Science* 2 (Minneapolis, University of Minnesota Press, 1958).
[10] Lewis White Beck, 'Constructions and Inferred Entities', *Philosophy of Science* (January 1950), 17, pp. 74; p77

and Meehl[11] in the context of psychology. For Beck and others a term with only such a function was essentially eliminable, because the concept simply had no application other than its summary of the data. There wasn't anything else that could be done with it, such as inferring other properties or the like. It simply acted as a convenient shorthand. These particular concepts aside there can be no doubt that to some extent theoretical terms have acted as summaries of observed facts, in the sense that they have conveniently packed information that can be extracted (deduced) from them, in accord with background principles, at any time needed. Concepts that represent putatively real objects may derive part of their explanatory value from their ability to unify disparate facts under the same principle. The third are inferential functions which we'll discuss further below. The second is most interesting because it reveals some awareness even in the context of a more or less philosophical debate over meaning of the role of theoretical terms at a higher methodological level providing scientists with direction to their research and a conceptual structure for it. The example White Beck gives is of a chemical compound that does not have the features we expect of it. Naturally we try to resolve this problem with the hypothesis of an impurity, which can then itself be examined. This is all done relative to the postulation of the entity itself and only because an entity had been postulated. Although not developed by White Beck beyond his need to reply to logical positivist assertion, what he anticipated was the idea that an entity concept entity itself can act as an epistemic frame of investigation.

Feigl, again in challenge to the 'Theoreticans Dilemma' and the implications of anti-realism, gives a more sophisticated list. Similarly to the intuitions of White Beck with regard his summarization function, Feigl includes 'formal simplification'.[12] By this he means the ability of a concept to reduce the number of laws by acting for instance as a putative common cause for otherwise complexly correlated events. Secondly Feigl singles out what he calls the 'explanatory power and heuristic value' of hypothetical constructions, by which he means more or less their 'unifying power'. "A large manifold of originally separate and apparently heterogeneous facts are all brought together."[13] This is similar of course to the first. But Feigl like White Beck also points out that such hypotheses are also fruitful in "suggesting further avenues of research". Feigl contends something of a conceptual benefit which is often the basis of the value of many causal agent entity concepts. Hypothetical constructs are justified

[11] K. MacCorquodale and P.E. Meehl , 'On a distinction between hypothetical constructs and intervening variables', *Psychological Review* (1948), 55, pp. 95-107
[12] Herbert Feigl, 'Existential hypotheses: realistic versus phenomenal interpretations', *Philosophy of Science* (January 1950) 17, pp. 35 – 62
[13] *Ibid*, p39.

frequently by the 'spatial-continuity they afford', which enable a measure of nomological or causal coherence. What Feigl means by this more or less is that with entity distributions you can formulate causal laws, which would be impossible if you allowed yourself only to plot relations between phenomenal patterns. Feigl's essential point is that hypothetical constructions enable or facilitate reasoning and simplification that wouldn't be achievable otherwise. These in my terms point to the strong *conceptual values* of the kind of physical entity concepts that Feigl is referring to. If we were to restrict ourselves only to everyday concepts in our theories, then we would have only complex and unwieldy phenomenal laws with generally an historical character e.g. if a magnet is brought near a compass at time *t* then the compass needle will move at a time subsequent. These laws would be discontinuous and numerous. With hypothetical construction we have proved capable of replacing these with general systems of laws predicated on the causal relations of entity concepts. Feigl thus pushed the systematization and simplicity value of entity concepts, themselves largely what I would call conceptual values, but also the explanatory power and intelligibility from the derivation of a 'nomologically coherent formulation.'[14]

Feigl gives a last but higher epistemological purpose to 'hypothetical constructions'; to provide a causal relation between observer and observed. The laws induced by virtue of reasoning about the unobservable world provide an independent basis upon which to correct, check or question the data or observations we make. We can in other words design causal theories of perception to help process and interpret observational material. Feigl calls this the 'Copernican Turn in Science', meaning the point at which a framework was set up in science which no longer treated observations as the primary and irrefutable, but in need of some kind of theoretical framework for their interpretation and relevance. One would think Feigl is heading towards some kind of 'theory laden view' of observation but he doesn't quite go that far. It is clear nonetheless that he thinks that hypothetical construction is itself needed in some capacity to make decisions about the value and reliability of evidence. Again I take these statements to be comments on the conceptual value of entity concepts for interpreting the world.

There are in fact for him higher epistemological ramifications of this fact; that of constructing a naturalized epistemology. 'The knowing organism must find a place in the world it knows.' This must be facilitated necessarily by postulation about the unobservable world, since these

[14] *Ibid*, p40.

relations are not themselves observable. If this is the case then of course concepts representing unobservables provide a deep fundamental role to the very possibilities of knowledge. That is at least Feigl's grand perspective on the epistemic role of theoretical terms, which support for him the very basic elements of scientific methodology. Feigl was thus willing to pursue quite strongly the idea of theoretical terms and entities as performing significant epistemic roles. We don't however need to go quite so far as this to identify the importance of theoretical entity concepts in historical practice (see chapter 2).

At the same time as the theoretical uses of entity concepts were being explored and categorized in this way by those associated with the Vienna Circle, if only loosely, there was a stream of thought strongly questioning the reduction of the meaning of such concepts to sets of observation statements or otherwise. In strong opposition to the Theoretician's Dilemma philosophers argued that there was an aspect of theoretical terms beyond definition, which served nonetheless an essential, what I would call, epistemic function. This was more or less the 'openness' or better put 'open-endedness' of theoretical concepts. Carnap identified it in his 'Testability and Meaning,' by which he meant the partial interpretation of such concepts and hence the ability to add more correspondence rules as required to strengthen the concept without ever completing it.[15] As such this 'openness' of interpretation was construed to reflect the property of novel application which theoretical terms seemed to possess and was thus an element of their methodological value.

Others had a different understanding of what the 'openness' or indefinable element of theoretical terms consisted in and what function it performed, construing it more as an aspect of meaning. A.C. Benjamin argues for instance that the meaning of terms in general is left deliberately vague rather than partial. When a term is employed it is only loosely defined by the case to which it has been applied in the past, but with 'reference to the possibility of cases slightly at variance with the past.'[16] As Benjamin puts it 'every symbol is defined with reference not only to past cases but to future cases as well.' While this might seem like rather odd expression, what kind of sense can we give to the idea of future cases, the point is that a symbol as he calls it, is deliberately and methodologically taken to be indefinite and not pinned down by the cases it has been so far applied to. If it were of course completely defined by its past cases, it would have no possible future applications and thus be only an efficiency

[15] "Testability and meaning", *Philosophy of Science* (1936), 3; and (1937), 4.
[16] A. Cornerlius Benjamin, "Science and Vagueness," *Philosophy of Science* (1937),4 , pp 422 -431; p425.

of expression. It is this potential application that provides the fruitfulness of a term and in fact explains our capability of relying on them for predictions.

Benjamin was challenging with this the positivist ‘theory of constructs’ or the attempt to argue that theoretical terms can both be constructs (reducible to present applications) and predictive at the same time. He doesn’t so clearly specify however what he thinks is the aspect of entity concepts responsible for this future application. But whatever it is, it is something to do with the meaning of the concepts. “…by ‘molecules’ we must mean something more than [the observation sentences] ‘p and q and r and s’ since we do make genuine predictions from this notion. Therefore the conjunctive sentence cannot exhaust the content of the notion ‘molecule’, and we are committed to an element of vagueness.”[17] It is manifested then in the element of vagueness of the meaning of the term. What this might consist of in terms of the structure of theoretical concepts, such as their properties and relations is not addressed. Benjamin makes an association between meaning and use, and takes the future application of terms to indicate nonetheless a surplus of meaning that is by nature of its inability to be precisely expressed in terms of actual applications, to be vague.

Lewis White Beck also takes the openness of theoretical terms to be fundamental to them, in terms of their ability to serve as a basis of prediction.[18] To do this a concept must refer ‘at least hypothetically’ to components which can be ‘symptomised by data not yet obtained, or at least by data not actually used in establishing the symptom relations and definition of the entity.’ So an entity must be more than its positivistic definition. Unlike Benjamin, White Beck has a more structural idea of the source of this future reference, and that is the possession by theoretical terms of properties that are mere constructs or ‘accidental properties’. These are properties not essential to the term, i.e. not part of its definition, but features of the concept nonetheless which can be used to give new predictions. If all its properties were essential then it would merely be a construct, and completely reducible to observation statements. So White Beck chooses to argue that a theoretical term to be capable of being used for predictions which are more than ‘tautologous consequences of definition’ must be more than this positivistic definition. He refers to this additional meaning as a ‘surplus of existential meaning’ because to use these additional properties over and above those operational defined summaries there must be an assumption the term corresponds to

[17] *Ibid*, p429.
[18] Lewis White Beck, ‘Constructions and Inferred Entities’, *Philosophy of Science* (January 1950), 17, pp. 74.

something really existing. This I think is an important point as indeed a realist attitude is as we will see later is necessary to the rationality of many of the uses made of theoretical terms. For White Beck reality props up or justifies a use of properties of a term seemingly unconnected with its observation statements but present in the concept nonetheless. He gives the example of the postulation of molecules to explain Boyle's and Charles' laws. This does not of itself require the imputation of mass to the molecules, only momentum, but of course mass is nonetheless imputed to molecules by our introduction of the concept as a small quantity of matter. But from the imputation of mass numerous predictions can be made, including Graham's law. In my terms then, these accidental properties which attend a concept can prove to have significant epistemic value, and signal the role of a theoretical entity concept beyond that of simply organizing and summarizing data.

Lewis White Beck and Benjamin encompass here two aspects of the use of entity concepts that are I think extremely revealing. The first is the inherent openness of these concepts, which enable their future application and ability to develop the concept. They each locate this in different aspects of the meaning of the concepts, but both identify the fact that these concepts are not pinned down to a collected set of observation statements, and this is a methodological aspect of the use scientists make of these concepts which simply goes beyond limiting them to sets of observation statements. In the second instance the assumption of an entity brings with it conceptual value in the form of background assumptions about the entity (properties and relations assumed of it because of what it is assumed to be), which can be used in the application of the concept in new situations. These form an attempt by both philosophers to show that uses of these concepts can't be pinned down by a positivistic theory of meaning. What is interesting to us is that these uses are epistemic which work by the very failure of attempts to tie down these concepts to a specific theory of meaning i.e. these uses implicitly envisage the application of the concept to develop theory and make predictions in novel situations, that can't be captured this way.

The decisive response to the Theoretician's Dilemma is however awarded to Hempel, in his paper of that name, 'The Theoretician's Dilemma'[19]. Hempel poses that while it might be possible through the agency of Craigian sentences to replace theoretical terms with an observational theory alone with the same deductive systematization (i.e. gives the same

[19] Hempel, C. G., "The Theoretician's Dilemma", in eds. H. Feigl, M. Scriven & G. Maxwell, *Minnesota Studies in the Philosophy of Science* 2 (Minneapolis, University of Minnesota Press, 1958).

deductive outcomes) it will fail to provide the inductive systematization that is also an aspect of the use of theoretical terms. For example you might have a collection of observational properties that are deducible from the concept of phosphorous; white, garlic-odor, soluble in turpentine etc. Let's say you discover a substance and find it to have some of these. Then of course in certain circumstances you will infer that this substance is phosphorous. From this assumption prediction about the properties of the substance can be made i.e. those not yet tested for. This is an inductive use of a theory that is impossible with Craig's Method since it makes no empirical assertions. The sentences of a Craigian theory are simply logically true.

Hempel's paper was seen to put an end to the project through Craig's Method or Ramsey sentences of eliminating theoretical terms, which would have in turn strongly questioned any kind of realism about them.[20] Craig's Method was objected to by pointing to the fact that theoretical terms have more practical uses than it can account for. Hempel, like Carnap, also identifies the methodological value of partial interpretation, which he calls a 'heuristic aspect of scientific theorizing.' When scientists introduce theoretical entity terms they intend them to have an existence independent of their associated observation statements, but they do not intend that the symptoms ascribed to an entity by virtue are to give an exhaustive characterization of it. "The scientist does indeed wish to leave open the possibility of adding to his theory further statements involving his theoretical terms – statements which may yield new interpretative connections between theoretical and observational terms."[21] The value of this is that the concept through its open-endedness can be fruitful in suggesting further lines of research and may lead to additional connections with the data of experience. Hempel calls this a heuristic value without exactly specifying what this heuristic aspect consists in, but one would assume there must be more to the concept than just a set of known properties or else it would be hard to identify what about an entity concept could be driving this heuristic function.

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[20] Craig's theorem says that from a theory T which with a set of correspondence rules C implies a set of observation statements, an interpreted theory T' ≡ (T.C). It can be shown that the set of deductive connections among observation sentences given by T' is the same as the set of all the theorems of T' expressible in terms of observation sentences alone. They are functionally equivalent although the later is usually an infinite set. Ramsey sentences replace theoretical terms by existentially quantified variables, so that all extra-logical constants that occur in a Ramsay sentence are observational. As widely acknowledged by Hempel and others Ramsey sentences don't eliminate the reference to hypothetical entities, it can only be used to clarify the meaning of a theoretical entity postulation.

[21] Rudolf Carnap, "The Methodological Character of Theoretical Concepts" in eds. H. Feigl and M. Scriven, *Minnesota Studies in the Philosophy of Science* 1 (Minneapolis, University of Minnesota Press, 1956), pp. 38–76; p70.

There a few conclusions to be reached from this episode in the history of philosophy. When looking back at the response to the Theoretician's Dilemma, and the project of strong positivism (and instrumentalism) to eliminate theoretical terms, the outcome was more or less transformed to a study of the functions of those terms, for which it was realized that the could not be accounted for by deductive systematization or summarisation. In my terms it revealed certain other epistemic functions of terms other than these conceptual ones, and placed more emphasis on their use in actual scientific practice. These centered on the new application of terms and their predictive or inductive functions. Importantly the 'openness' of theoretical terms although framed within the language of 'partial interpretation' was centralized as a methodological choice by scientists as to how they treated their terms. Clearly it was obvious to these philosophers that scientists did not fix them to their observation statements but treated them as open to the accumulation of more observation statements as part of the process of research they were engaged in.

It is clear however that their analyses fall well short in a number of ways, and left some questions tantalizingly open. In the first place I have noted already that there is something left unexplored by Hempel's and Benjamin's reference to a 'heuristic' or vagueness with respect to the way in which openness is manifested and used. I think more can be said about this, at least in the case of theoretical entity terms or concepts, which pertains to aspects of their structure and general use. Secondly all the authors above employ something like a positivist or verificationist theory of meaning towards theoretical terms. No doubt this contributes to the lack of comment on what the heuristic value of entities could consist in, because the meaning of theoretical terms is only locatable in observation statements. It led to the concept of a theoretical term as 'partially interpreted', and thus its openness as consisting in an incompleteness of connections to the observable world. But it rules out that entities might have meaning over and above correspondence relations.

Paradoxically perhaps, focusing attention on the uses of such concepts cemented the idea that first an analysis of theoretical terms required a semantics of theoretical terms, and second that the meaning of concepts should be less tied to their use in observational situations and more tied to a more generic understand of what 'use' could be. That is an historical hypothesis at least. Feigl's and Achinstein's chief response in the 50s and 60s to the Vienna Circle has a large measure of this.[22] Feigl for instance argues that the use of such terms in scientific practice fundamentally demands a referential element, particularly in so far as many predictive and explanatory practices rely on making 'existential' judgments. In fact 'the customary and legitimate use of inductive probability presupposes 'the realistic frame'.'[23] By this he meant that the reliance on concepts pertaining to unobservable in induction demanded a semantics; the construction of an appropriate semantic metalanguage in which statements assessing the probability of existential statements of the reality of the corresponding entities could be formulated and thus could ground these inductive practices. Hence the 'realistic frame' towards entity concepts required at the least their potential reference before they could be used inductively. Such claims signal what I think is an important insight for us later on how this intersections with realist attitudes, and the way these attitudes govern these use of theoretical entity concepts.

Achinstein in his strong critique of various aspects of logical positivism makes the 'use' of concepts primary to their interpretation, and talks of the 'semantical aspect' of their use by which he means the properties used that are to a greater or lesser degree necessary to the classification of an object as a particular kind or thing. As such Achinstein claims, theoretical terms are introduced into science interpreted, and it is up to us to reconstruct through the use made of a term, what that interpretation is. Importantly however when a scientist introduces a term that purports to refer to something unobservable he or she will not interpret it simply by relating it to what can be observed. Rather he or she will rely on concepts that he or she thinks can be readily understood. This may extend and often does to citing or using properties that themselves are unobservable. Meaning is not constrained by observability or verifiability. Scientists rely rather on analogies, on metaphorical language, on background theory, on ascribing observable properties to unobservable things; on a collection of techniques to make their terms intelligible and their explanations viable. It is further hard to argue that scientists treat unobservable examples of properties as any different from their observable kinds, for example size or mass. In fact scientists rely on the link between on the basic lack of difference between a property instance that is observable and one that is unobservable to both provide intelligibility, and to transfer their uses. As molecules had properties that obeyed the laws of classical mechanics, they could be fruitfully employed in predictions and theory-making

[22] Herbert Feigl, 'Existential hypotheses: realistic versus phenomenal interpretations', *Philosophy of Science* (January 1950) 17, pp. 35 – 62 and Peter Achinstein, *Concepts of Science* (Baltimore, John Hopkins Press, 1968).
[23] *Ibid*, p57.

endeavours. It was in fact basic to their explanatory power in many instances. As such at the basis of their many epistemic values lies a degree of interpretation and meaningfulness of the concepts to scientists themselves, and it is artificial as per logical positivism to divorce the two.

In fact Achinstein himself is quite aware of something like the 'epistemic function' of a concept. In discussing the theoretical-nontheoretical distinction for terms, he considers more or less the question whether we can discriminate a theoretical from a non-theoretical term by the epistemic function it has in a theory. He makes the point which is essentially mine, albeit off-hand, that "to fully understand a scientific concept it may be important to consider the role or roles it plays in a theory…"[24] This extends for Achinstein to the role of a term in formulating the principles of the theory, how it provides conciseness or simplification, how it is used in proofs of theorems, and how it explains various phenomena.[25]

The logical positivist discussions and debates thus raised in the particular contexts of their disputes the subject of the methodology of theoretical terms. This resulted in a limited but novel exploration of the various epistemic roles such terms have had, and the claim that aspects of the meaning or use of such terms is not necessarily pinned down by definitions or observation statements and the like, but rather are 'open' in their description or meaning which facilitates their application in different contexts and their own further description. The discussion of these points was more or less confined by the whole question of 'interpretability' and the Theoreticians Dilemma, and didn't extend except in Feigl's case to broader questions about the roles of theoretical terms. But the realizations of this debate are nonetheless instructive and point towards deeper questions about the role of theoretical terms at the heart of ongoing processes of research. In summary theoretical terms have epistemic uses over and above deductive or systematization functions (and thus cannot be eliminated from scientific practice), and the openness of concepts seems to be an aspect of that use.

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[24] Peter Achinstein, *Concepts of Science* (Baltimore, John Hopkins Press, 1968), p186.

[25] Although he doesn't develop this point, the idea of the 'role in a theory' or role in scientific practice is for him important at least to make the philosophical point that it can't be used as a criterion to distinguish theoretical terms from non-theoretical ones, in the sense that the role of such terms may solely depend on principles of the theory, since there are simply too many different ways of defining this dependence and no particular justification for any particular one way. Achinstein is nevertheless not above the possibility that these features of a concept might be relevant to its understanding or interpretability.

1.2.2 Scientific Concepts, Conceptual Frameworks and Research Processes

In the last decade or so there has been an evolution in the thinking of various philosophers about what scientific concepts are, and what is or has been their role in scientific thinking and practice. This discussion is only recent, but is born of a dissatisfaction of the traditional manner of dealing with scientific concepts, which is to treat them more or less as sharply-bounded containers of meaning (properties, descriptions etc). We might call this the vessel-like or ring-fenced description of concepts. It is born of a tradition which has firstly emphasized descriptive theories of meaning for concepts and secondly treated concepts as largely the outcomes, or products of, research processes. That is, concepts are the recordings of scientific work and are reconstructed by philosophers to form coherent pictures about what scientists believed or believe, in a particular instance. Much philosophical work could be described in these terms. This ignores however the roles concepts play as the means by research processes are developed. Concepts I believe have dynamic roles in research processes, and not so easily extractable from research processes themselves. Certainly no full picture can be had of scientific concepts by ignoring these roles, and simply providing interpretations of them at particular points in time.

This understanding that concepts and conceptual frameworks have deeper roles in scientific practice is not a new one but has a longish history. One of the traditions of thought that replaced logical positivism is that of Hanson, Kuhn and others (although Carnap also had thoughts along this line) that there is a theory-ladenness with which scientists understand the natural world, frame questions about the natural world, and interpret observations. These questions about theory-ladenness of observation are somewhat already passed, but one conclusion of them is that the role theories play in science is not simply that of the end product of a constructive process, but also a framework through which the world is conceived, the significance of observations interpreted, problems formulated and adjudged for their importance and so on...

Hanson, in his text ‘Patterns of Discovery’ gives what is still one of the most persuasive formulations of this without falling into conceptual relativism.[26] What I want to emphasise here is his claim that theories are chosen to make observations or experimental results

[26] N.R. Hanson, *Patterns of discovery: an inquiry into the conceptual foundations of science* (Cambridge, Cambridge University Press, 1958).

intelligible, by fitting them, in his language, into an intelligible pattern of thought. The mechanism through which such patterns are chosen is more or less abduction or best explanation. Thus historically Kepler chose the ellipse to represent the motion of Mars because of his realization that this mathematical object made most intelligible the data on Mars' orbit. As he exclaims, "A theory is not pieced together from observed phenomena; it is rather what makes it possible to observe phenomena as being of a certain sort, as related to other phenomena. Theories put phenomena into systems."[27] Hanson's essential point, in objection to rather naïve philosophies of science, is that theories are not simply inductively read off the data, nor is science in search simply of deductive systems that reproduce the data. In fact data itself is always in need of interpretation from the outset. Theories, as conceptual frameworks, step in to provide a framework upon which to organize data and make it intelligible, but also more basically to do science, i.e. provide a basis upon which to interpret and to reason about observed facts.

The way in which a theoretical entity concept might participate in this is also noted by Hanson. "The formation of the neutrino concept provides a paradigm example of how observation and theory, physics and mathematics have been laced together in physical explanation."[28] The neutrino concept, postulated first in 1930 by Wolfgang Pauli, to preserve conservation laws in beta decay, can be understood as just such an elucidating thought-pattern. The neutrino concept made intelligible the otherwise baffling behaviour of the other particles in beta-decay. In the process of postulating subatomic particles Hanson tells us that there is a sense in which the concept being postulated acts as something of an open frame upon which to impute properties to satisfy the different explanatory demands for which it was postulated. Such a concept can be 'finely tuned' to fix the data in an intelligible conceptual pattern through the adjustment and attribution of properties to the entity. As he puts it the properties of particles are both 'discovered' and 'determined' by the physicist. For instance "the physicist fashions the electron concept so as to make possible inferences both to its particle and to its wave behaviour…" (123). As it goes, new properties may be worked into the concept of a particle to satisfy explanatory demands. This suggests something of fluidity and openness to the history of how particle concepts have been used, but also the fundamental epistemic value of its explanatory roles to this use.

[27] *Ibid*, p90.
[28] *Ibid*, p48.

As with the insights of logical positivism I think with Hanson there are germs of ideas that are actually suggestive of something historically important and in need of far more elaboration. The essence of Hanson's theory is more or less that theories or conceptual frameworks are starting points not simply the end points of grappling with the natural world; and that data is only interpretable through a theory. Though he doesn't explore this fully, being more focused on the introduction and creation of theories, he is suggestive of the fact that a conceptual framework provides the frame in which to approach new observations and develop a response to them. But he also addresses himself to the importance of individual concepts, such as the neutrino. Certain concepts play roles central roles in these frameworks and are the basis around which they are organized.

Of course some of this overlaps with Thomas Kuhn's conception of the function of paradigms, which are the broad conceptual frameworks that govern disciplines and are also constitutive of scientific activity. The paradigm is central. Kuhn's, and later Laudan's, commitment to the idea that paradigms or conceptual frameworks create more or less sequences of puzzles, as basic scientific activity. While too broad a claim in this respect, explanation or understanding cannot be so easily jettisoned, it does signal the important role of conceptual or theoretical frameworks for generating what count as important research questions. These in turn sustain processes of research. What is interesting or problematic to the research scientist is dependent on the conceptual framework he or she is relying upon. And much of science is solving problems that emerge from the application of theories, but recursively so, in the sense that established theoretical elements are relied upon at the same time to generate solutions i.e. they act as both resources and constraints. Kuhn is more or less driven by the theory-ladenness question with respect to the role of paradigms and his thesis about scientific revolutions regards the paradigm as some kind of more or less rigid constraint, but he is less concerned with its role as an actual tool with which it can guide its own development. Kuhn's paradigms are in fact too broad and encompassing for what we require to understand the function of most concepts in science, and his concept of puzzle solving as normal science too narrow, where the epistemic roles of concepts are more multifarious and a research process often investigative, open to new phenomena and discoveries as part of that process, rather than inwardly concerned with fixing up its own problems. In fact it is part of this dissertation to argue that Kuhn's account of science is too restricted because it takes a too restrictive view of what concepts are and neglects the epistemic roles of concepts in directing research processes 'openly' as it were. Kuhn sees discontinuity where there are in fact adaptive processes in operation that build processes for change and development into research process through the flexible and open use of concepts, through the shared epistemic roles of those concepts. This methodology allows scientists to explore complex systems from a vantage point that doesn't over-commit but acknowledges the contingencies of practice. Where thus in many instances Kuhn sees rather static frameworks, with scientists working to fix them up, we should see a more dynamic structure that guides, or sets a basis for, their own development. For this reason to talk of meaning shifts between theories, thus cutting up research processes into discrete elements, ignores in some cases the methodology of the use of concepts which in fact directs those research processes and frames those very shifts through these epistemic roles. This is at least the kind of model of science I believe more accurately captures its dynamics.

Putting these rather brief considerations aside until a later point, we have from Hanson and Kuhn some kind of appreciation of what I'm calling the 'conceptual value' and also explanatory value of concepts in research processes. Others have more precisely explored what roles concepts play and how they are used in practice. Thagard gives a list of what he thinks are principal uses of scientific concepts.[29] These include categorization, learning (an aid to learning things about the entity or kind), memory (an aid to memorizing things about the entity or kind), deductive inference, explanation, problem solving, generalization (learning new facts about the entity or kind from new examples), analogical inference (use the entity or kind to make similarity judgments about others), language comprehension and language production. The last two express the role of concepts in using and interpreting language[30]. Of these deductive inference, explanation, problem solving, generalization and analogical inference are clearly epistemic functions as I have defined them, and the rest only indirectly so, in so far as they as necessary to taking advantage of such uses in the first place. To these we would also add inductive inference, which proved very important in Hempel's destruction of the 'theoretician's dilemma,' and more generally prediction.

The point of Thagard's is that a single concept is a complex structure which instantiates a variety of uses. For instance if we consider the concept 'whale' we might extract the following information.

[29] Paul Thagard, *Conceptual Revolutions* (Princeton, Princeton University Press, 1992).
[30] *Ibid*, p22.

WHALE:
A kind of: cetacean, mammal, sea-creature.
Subkinds: humpback, blue, killer, sperm….
Parts: fins, blubber, bone, blowhole, tail

But also the concept should contain various rules for its use or application. Such as for instance, "if x is a whale then x swims." If as Thagard puts it we consider concepts as 'computational structures', then it makes sense to say that rules are part of concepts as well as that concepts are part of rules. "The presence of rules shows how concepts can be used in deduction, explanation and problem solving. It also shows how concepts can be intimately tied in with theories, since some rules express causal relations." The last can be elaborated by saying the concepts have also conceptual functions. "Concepts as complex structures can figure in generalization, analogy, and other forms of inference."[31] These rules are rules for applying a concept epistemically; i.e. in deduction, induction etc.

This I think is a good account of collection of values that compose the epistemic roles of scientific concepts. These are properly to be seen as part of the concept, which is not just a repository of factual information, but something broader. It is clear that Thagard's approach to concepts can be used a starting point, a basic structure, from which to approach the more specific question of the uses of scientific concepts, but Thagard gives unfortunately no real analysis of the role concepts play in research processes themselves still tending to treat conceptual frameworks again as static instances and outputs.

There has been thus some understanding of the epistemic roles of concepts, and thus their value as means to research and investigation. Let me be then here suggestive about what I think a theory of concepts in scientific practice, particularly in the research context, needs to take into account. This view, which will itself emerge from the following chapters, postulates that scientific concepts in actual practice cannot be reduced to mere vessels of meaning. The vessel conception, which is tied closely to the descriptive theory of meaning for concepts, treats concepts fundamentally as outputs in research processes or end products that are defined by the conceptual frameworks they find themselves in at a particular time or stage of research. In other words their abstract function is descriptive; or to acts as elements of representations of the world. However this is, I would assert, only one aspect of their function,

[31] *Ibid*, p30.

and taking this approach to them philosophically neglects their substantial other roles in scientific practice, which weighs against withdrawing them from ongoing research processes in this way as arguably philosophers have generally done. As such even the seemingly well acknowledged problem of incommensurability comes out looking like an artifact of a particular philosophical abstraction on the notion of a concept which fails to comprehend their very part enabling and structure the kinds of meaning and descriptive changes that are the essence of ongoing research.

That strong claim I leave for later chapters. The point here is to suggest that the tradition of picking out static instances of concepts and conceptual frameworks and analyzing them more or less independently of their context of construction and development ignores a significant aspect of their role, which helps account for the variation of those concepts without supposing that this means these simply become disconnected from one another. Seeing this requires a shift towards a more epistemic account of concepts that understands their intermediary role in guiding and facilitating ongoing research processes. The fact is that it is historically only rare that scientists have been in a position to claim a complete theory or the achievement of their research goals. Rather theories or conceptual frameworks are created with full knowledge and expectation that they are means to future research or to developing in a more sophisticated way with the problems at hands. They have for instance investigative functions, framing or revealing paths of research and problems that are guide this research onwards. As such, and this will be a focus of my arguments with respect to theoretical entity concepts, I suggest that scientific methodology anticipates its own development by building with conceptual frameworks conditions and resources for its development. In this manner it anticipates redescriptions and revisions as part of the necessary process of investigating complex and uncertain systems. Hence we have some methodological basis for the openness and flexibility of many concepts we witness in historical practice.

Aside from these claims however it is clear that there is a general consequence for how we approach scientific concepts. If we wish to give an account of the mechanism and methodology of historical research processes then we should account for how concepts have been used epistemically as bases upon which to structure and organise them. Historically research processes should not be extracted or separated from how we understand the concepts involved in them. We cannot simply treat concepts as the products of these processes rather than as part of attempts by scientists to frame their own approach to phenomena.

1.2.3 Lessons from the Natural Kinds Debate

One benefit of this approach to concepts is that it provides potential grounds for distinguishing concepts by their epistemic roles, when ontologically grouping these concepts, by come kind-constituting description for instance, has proved problematic. My pursuit of theoretical entity concepts in the later chapters can in fact be justified on this basis, grouping this class by their common epistemic roles, and not just ontological ones. As such we have grounds to generalize philosophically about this group. In this regard one type of concept itself concerned with generalizations has become the subject of a more epistemically attuned analysis, born of I would gather just such a frustration with ontological or metaphysical attempts to define them. I'm talking here about 'natural kind' concepts. In a recent paper Ian Hacking pronounced the concept otiose having outlived its philosophical usefulness, and having been exhausted by pointless philosophical attempts particularly in the 20th century to pin the concept down to a unitary metaphysics when in fact there is none nor should one be expected given the collection of varying objects or groups the term is applied to like water or electron or species.[32] However if one looks closely at Hacking's text its clear that what he really objects to it is more or less this essentialism about natural kinds. "It is the idea of a well-defined class of natural kinds that has self-destructed…." But I don't think this necessitates that the concept be abandoned, because it seems to me that the term 'natural kind' describes a particular attitude towards and use of particular concepts with which scientists feel they can rely on for making universal (nor near universal) generalisations.

As such recently philosophers of biology in particular have started to question the lack of an understanding of the epistemic values of natural kinds, at least before an ontological one is pursued. Both Brigandt and Reydon for instance point to the failings of the debate which has traditional put ontology first.[33] Mostly this has concerned itself with some attempt at essentialism. This tradition starts from the principle that there is some unique 'nature' to all proper natural kinds that fixes the extension of the kind. Everything belongs to a natural kind by virtue of its essence, and must possess this to be a member of a kind. On the Putnam story scientists search for these essences and when they have found them shift the 'name' of the

[32] Ian Hacking, 'Natural Kinds: Rosy Dawn, Scholastic Twilight', *Royal Institute of Philosophy Supplement* (2007) 82, pp. 203-239.
[33] Thomas Reydon, 'Natural kinds as tools for philosophers of science', *EPSA* 2007 (Conference Presentation, Madrid)

kind to that essence. The essence of water for instance is commonly taken as H_2O. Anything with that structure is thus of necessity water. But the problem with the restriction of kinds to essences is that the stronger you make this requirement the more it tends to exclude almost always concepts which would otherwise be regarded as fitting the idea of a natural kind – or a naturally determined grouping. On this approach then philosophers have to make distinctions between proper natural kinds and other kinds, usually favouring some kind of levels picture in which the only kinds that can be called 'natural' are those associated with the fundamental levels, physics and chemistry, where one can formulate sharp classification schemes that don't admit of exceptions. According to Churchland's well known criteria, a kind is only natural if it figures in "the most basic laws of an all embracing physics."[34] On this basis there could only ever be a small 'aristocratic elite' of kinds considered natural kinds, and as Churchland concedes, probably none at all.

This is of course an extreme position. But even with more moderate essentialist positions on natural kinds there can be genuine reference issues over how important or decisive essentialism is to the scientific attitude towards natural kinds. We can question for instance whether the kind term water in science is really necessarily pinned down to the structure H_2O or whether under certain circumstances scientists might be prepared to consider water, and its associated observable properties, to be described by other structures. It was in fact discovered that water contained not just H_2O but also molecules with heavy hydrogen. Instead of determining the later not to be water, LaPorte reports that scientists decided that there were two types of water not one, heavy and light, on the basis of the almost identical observable properties. [35] LaPorte asserts that it is really a contextual decision on the part of scientists what to do in these situations.[36] This problem of reference challenges at least the presumption that scientists are always essentialist about kinds, a point that will be emphasized in following chapters.

Indeed for me the main problem with trying to refine an essentialist approach, and by virtue decide what counts as a kind and what doesn't, is that it fails to identify or even consider that there exists a scientific *practice* in respect of such concepts that groups them together according to how they are used, not what they consist of. In fact what makes a kind 'natural'

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[34]Paul Churchland, "Conceptual Progress and Word/World Relations: In Search of the Essence of Natural Kinds," *Canadian Journal of Philosophy*, p16.
[35] Joseph LaPorte, *Natural Kinds and Conceptual Change* (New York, Cambridge University Press, 2004).
[36] *Ibid*

so to speak I would hesitate needn't be resolved on one ontological criterion alone, and in fact to give a metaphysical criterion itself prejudges what science itself might decide are the underlying conditions for natural kinds. As such it should be treated as an open question capable of different answers in different scientific fields.

While this is a fairly novel claim, it is clear that natural kind theorists have been trying to escape the metaphysical stranglehold of essentialism by pointing to more epistemic criteria upon which to understand natural kinds. Boyd for instance asserts that the philosophical task with respect to natural kinds is to evaluate how they support our inferential and explanatory decision-making, and the roles they play in this regard.

"It is a truism that the philosophical theory of natural kinds is about how the classificatory schemes come to contribute to the epistemic reliability of inductive and explanatory practices."[37]

At the same time for Boyd this is a question of practice that requires identifying how scientists fashion natural kind concepts to both balance the need for exceptions especially in the case of biological natural kinds and at the same time give them a concept they can generalize over and explain with. His homeostatic property cluster conception aims at showing how scientists do just this by treating natural kinds as property clusters, and not something pinned down to an essential group.[38]

If we want to avoid imposing metaphysical presuppositions about how we think nature is organized and thus what counts as natural kinds, and thus have useful a category of 'natural kind' which spreads across a wide variety of different scientists and represents something important about the connection between beliefs about the world and scientific methodology, then it is a prerequisite to have an account of how potential natural kind terms contribute to scientific practice, and on what basis they might be identifiable by their epistemic functions.

Consider for instance their explanatory functions. Natural kinds are conceived, the classifications developed, because the concept can be used for explanatory purposes where the

[37] Richard Boyd, 'Kinds, Complexity and Multiple Realizability', *Philosophical Studies* 95(1-2) (1999), p 67-98.
[38] Richard Boyd, "How to be a Moral Realist", in G. Sayre-McCord (Ed.), *Essays on Moral Realism*, Ithaca:Cornell University Press 1988, pp. 181-228; Richard Boyd, "What Realism Implies and What it Does Not", *Dialectica*, 43, 1989, pp.5-29; Richard Boyd, "Realism, Anti-Foundationalism, and the Enthusiasm for Natural Kinds", *Philosophical Studies*, 61, 1991, pp. 127-148.

features of some individual(s) needs explaining. Such explanations often take the form that individual 'a' is a member of the kind A. The category of 'virus' can be used to explain the action of some disease, and suggest the type of treatment that might work. To use one of LaPorte's examples, much is explained by an object being a polar bear. We can explain why the creature in question swims, lives in cold climates, why its raises cubs. These types of behavioural explanations are very characteristic of natural kinds. Natural kinds come with properties that apply generally to their members and make them appropriate hence to explain individual instances.

This link to explanatory function raises an important point that shows that we should take seriously the distinction between an epistemic account of natural kinds and an ontological one. Many kinds in science are *functional kinds*. They are defined by their physical function, not by their structure or intrinsic properties. This is expressed by a particular causal role, in the context of a particular system. These can include kinds like 'predator', or psychological categories like 'fear'. Generally they are multi-realisable: there is more than one structure that could reproduce their causal role in the particular system at hand. Reydon points particularly to modular reasoning in life sciences as a key example of the application of functional kinds in science, where the behaviour of a system is explained by citing generalizations about the functional kinds embedded in these systems.[39] Like natural kinds, functional kinds can be applied in new explanatory situations. Like natural kinds, functional kinds can be inductively generalised over. What applies to one functional kind can be assumed to apply to all, or to apply generally. By virtue predictions can be made. Further like natural kinds (and entities for that matter) they can serve as *explanans* as well as *explanandum*. Sometimes it becomes necessary to explain features of functional kinds, which involves opening what is otherwise a black-boxed system. Reydon's point is that for many purposes both functional kinds and natural kinds appear to have the same kind of epistemic roles, and aren't distinguishable by scientists, not at least until some attempt is made to penetrate to the inner structural level, and find out whether different structures are realizing the same causal function. Thus he suggests as I do that we should group natural kinds first by this epistemic role and reserve any hope for some homogenous or unique scientific ontology to investigation. But principally these epistemic roles must tell us what natural kinds are before these metaphysical questions can be investigated. In this regard I completely agree.

[39] Thomas Reydon, 'Natural kinds as tools for philosophers of science', *EPSA* 2007 (Conference Presentation, Madrid)

I think we can go further than this however, and ask why we should seek to answer this ontological question at all rather than leave it to scientific research processes. I would agree with Hacking in this respect that there seems little point in seeking after an ontological account of 'natural kinds'. They seem to me first and foremost elements of practice with epistemic roles that seek not to set down generally first principles on how these groups should cohere. At best we can have a pluralism of viewpoints on this, dependent on whatever emerges from scientific theory in this regard.

I would claim rather a more naturalistic option that we should accept that the reasons elements of kinds can be generalized over and explained with is an internalized part of practice, to be answered through the different explanatory frameworks scientists have and come up with. What groups these concepts we call natural kinds together then needs to be something epistemic, and thus we should investigate 'natural kinds' relative to the way in which these concepts are used in practice and are inherent parts of research processes. This seems to me by far the more interesting and relevant issue, and reserves to natural kinds a possibly fundamental role in practice. As such we examine how 'natural' is spelt out epistemically (not ontologically), and how it operates in research processes in terms of the uses and assumptions scientists make of these kinds. As such what physical basis underlies them and holds their members together can be interpreted as often part of the investigative roles these concepts have by shaping expectations of the natural world and providing actual paths of investigation.

I would postulate in this regard that scientists often garner their beliefs in the 'naturalness' of a kind, or its ability to support their inductive inferences and explanations, by virtue of their success in doing so. Reydon has recently suggested that what really counts is success in *many different generalizations*, not simply the ones a natural kind was initially generated to explain.[40] On this basis abductive inferences could be reasonably made that there exists underlying reasons to treat this group as a natural group and rely on it in making future generalisations. One suspects that this kind of reasoning does reflect a practice of natural kind concepts. This of course adds investigative functions to natural kind concepts, because these groupings may motivate and direct investigation into the underlying causal structures that maintain the group. But ultimately it doesn't matter to practice in many cases whether the

[40] Thomas Reydon, 'How special are the Life Sciences: A view from the natural kinds debate', *Proceeding of the first conference of the PSE (Philosophy of Science in a European Perspective),* (forthcoming, 2009).

kind is multiply realized or not as long as there remains a basis to rely on the concepts *epistemically* in this way. On the other hand a natural kind is often a concept which is associated with a field and its practices, for which there exists in the field no explanatory basis to reduce the kind, and no motivation to do so. The point is that the concept plays certain epistemic roles in the field and is as such indispensable to its research processes. For the field what underlies it is simply a secondary consideration, and doesn't necessarily count towards whether or not the concept is treated as a natural kind. The standard of success across different generalizations could be the basis of that.

With these ideas we have a way out of the rather paralyzing debate over reductionism and multiple-realisability. The spiral down to a strong essentialism with respect to natural kinds has largely caused this, by denying many scientific fields any claims to say they are with their natural kind concepts are investigating real objects of nature rather than artifacts of their classificatory systems. That runs simply against actual practice. In so far as reductionists aim to eliminate natural kinds that are irreducible from practice they miss the point that a reductionist explanation may not be necessary to support natural kinds in practice, and what natural kinds are in practice is driven by their epistemic roles. They are a basis of research processes. Investigative questions are formed, investigated and theorized about by virtue of them. On this basis I think we have against Hacking some ground to retain the concept of a 'natural kind' as an essential part of our understanding of scientific practice, particularly the categorization and grouping practices of scientists, although our consideration of these is principally epistemic and methodological rather than ontological. As objects of scientific practice and research processes, they play and have played significant epistemic roles. Natural kinds in this manner provide an example of the important of shifting our attention towards the epistemic value of certain concepts, affording us a better understanding of the way in which research operates through the epistemic categorization and identification of such concepts. The terms 'natural kind' covers an important and fundamental aspect of scientific practice which has yet itself to be fully investigated because of our distraction with ontological questions.

1.2.4 Lessons from Hacking's Experimentalism Realism

My last two considerations narrow down more precisely to theoretical entity concepts, away from theoretical terms or scientific concepts in general. One of the more significant motivations for considering the broader roles of theoretical entity concepts in scientific practice comes from Hacking in *Representing and Intervening,* who does just this.[41] Hacking asserts strongly the central role an entity can have in the experimental context as a tool for probing and producing new phenomena. He points to cases where entities or at least knowledge of their causal properties, have instructed scientists on the development of new instruments to better probe nature by making use of these entities to discover new facts and new phenomena. He documents for instance in the case of the Peggy II instrument where experimental physicists relied on detailed technical knowledge of the electron's causal properties to produce a machine capable of testing parity violation in weak neutral current interaction.[42] Here the 'tool' metaphor is most apt, because experimenters treat some unobservables like tools, using them to facilitate their further study of the world by producing new phenomena. Critical to Hacking's philosophical contentions about the importance of experiment, is that this all happens without theory, or at least only with basic 'home-truths', as he puts it, about the entities at hand.

Hacking justifies these claims by observations of the way in which experimenters use entity concepts. These are part of his more general belief that the experimental side of science has a much neglected 'life of its own'. His text directs itself to showing that the rather complex relation between theory and experiment can't be reduced to simple statements over the necessity of theory to experiment, or the classical ordering in which theoreticians hand experimenters theories to test. He points to examples where experimenters have worked largely free of overarching theoretical positions, manipulating their materials and exploring phenomena under different circumstances. Entities in this sense have an experimental life disconnected from their instantiation as a participant in theoretical development.

Of course Hacking is only intermediately interested in the value of entity concepts for experimental practice. His purpose is a theory-free entity realism. The manipulability of entities in the experimental setting and the ability of experimenters to use such entities to further probe the world and discover new phenomena rationally compel us to believe these entities exist. Indeed the realism of experimenters in dealing with the entities like the electron is explained this way as entirely rational. In fact as such for Hacking there should be no distinction in experimentation between the concept and the thing itself. It is not a matter of

[41] Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge, Cambridge University Press, 1983).
[42] Ian Hacking, 'Experimentation and Scientific Realism', *Philosophical Topics* 13 (1982), pp 154-172.

trying to justify the relation of a concept to the world. For experimenters the thing itself is what is being manipulated, not the concept. Most commentators disagree with Hacking's attempt here to have realism without ascribing best explanation arguments to the scientists themselves or a theoretical concept of the electron first.[43] It is theory that interprets the experimental setups as the manipulation of a particular entity. The theory thus comes first, not the entity.

Nevertheless regardless of Hacking's ultimate purpose with these observations or the failings of his realism argument, he has identified what I would consider an epistemic use of theoretical entity concepts as an experimental tool; namely a conceptual device used to structure experiments to test other phenomena. The Peggy example is a good example of just how the electron played such an epistemic role producing knowledge as a result. Importantly there is an 'open-endedness' to Hacking's conception of an experimental tool. Entity concepts can be used in experiments in ways which don't revolve around the question whether they have been adequately theoretically described. There is thus an epistemic life to certain concepts (namely those with causal properties) beyond theoretical debates about the underlying entity itself (such as the models that govern its behaviour or internal structure), and beyond ontological questions about what an entity is or how it should be defined. Considering the electron for instance:

"Various properties are confidently ascribed to electrons, but most of the confident properties are expressed in numerous different theories or models about which an experimenter can be largely agnostic. Even people in a team, who work on different parts of the same large experiment, may hold different mutually incompatible accounts."[44]

What then ties the concept together? What allows these researchers to remain agnostic?... the fact that there is sufficient understanding of certain causal properties of an entity to employ it practically in experimental contexts. "There is instead a family of low-level causal properties in terms of which gifted experimenters describe and deploy electrons to investigate something else…"[45] Whatever these are it seems clear from what Hacking says that this relation

[43] See for instance D.B. Resnik, "Hacking's Experimental Realism," *Canadian Journal of. Philosophy* (1994) 24, pp. 395-411; Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (The University of Chicago Press, Chicago and London, 2006).
[44] Ian Hacking, 'Experimentation and Scientific Realism', *Philosophical Topics* (1982), pp.154-72; p. 155.
[45] Ian Hacking, *Representing and intervening : introductory topics in the philosophy of natural science* (Cambridge, Cambridge University Press, 1983), p272.

between causal properties and productive situations gives the sense to experimenters that they are manipulating a real thing. This translates I think to the conceptual level (if again we reject Hacking's realist argumentation) as a belief that underlying the concept must be a reality. This holds the concept together. Thus this functional aspect of the electron is essential to the electron concept which Hacking argues is how experimenters see it, because this function ties these properties to a thing.

Hacking is thus onto something important when he gives the electron example to demonstrate that experimenters place significant stock in their ability to use an electron rather than to describe it or represent it within the latest theoretical framework, and work with the concept productively without this information. It suggests that their concept of an electron (lacking certain descriptive elements) depends on something functional, i.e. its conditions of deployment and use, in the sense that it becomes an established part of scientific belief. Hacking brings out into the open the importance of the epistemic function of entities in the experimental context, and notes how they can be used in certain circumstances open-endedly; in this case meaning free of certain deeper theoretical commitments.

I think Hacking's observations, and their obvious epistemic consideration of theoretical entities, can be generalized outside the experimental context. Doing this requires however that one of Hacking's other restrictions be vanquished; namely that the manipulation of entities be confined to experimental contexts and uses. As already pointed out it is difficult for Hacking to claim that there really can be a 'theory-free' manipulation of entities. Apart from the objection that theories and conceptual frameworks are needed to interpret experimental results I doubt that one really can isolate in practice a 'non-theoretical' component of an entity's representation which experimenters could be said to rely upon. Others such as Arabatzis and Resnik doubt this too.[46] I have expressed some sympathy with this when I've agreed with Hacking that there are cases in which there will be models and descriptions that attempt to account for the entity, its behaviour or apparent properties which needn't be accepted or rejected to proceed to use the entity in a substantial way. This is an important detail. But in general it cannot realistically be claimed that the limited descriptions of an entity (in terms of low-level causal properties for instance) can be isolated from the theory-making going on around the world of the experimenter. For one thing its extremely difficult to claim as

[46] D.B. Resnik, "Hacking's Experimental Realism," *Canadian Journal of. Philosophy* (1994) 24, pp. 395-411; Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (The University of Chicago Press, Chicago and London, 2006).

Hacking does that you could assemble some 'home truths' about electrons without bringing with is a host of theoretical principles. An entity's causal properties are in fact never refined in a theory free way. Such properties are always defined with respect to the background theory that gives them meaning. One can't for instance interpret 'charge' without the force law describing the interactions between charged bodies, to take a simple example. Experimental decisions are made with respect to a host of theoretical considerations, which includes theory about the entity itself. I think that almost goes without saying. Experimenters need laws and generalizations, and natural kind descriptions, to understand how the causal properties associated with theoretical entities behave. My point then is not to deny Hacking's claim that certain levels of theoretical description don't seem be necessary to the use of a theoretical entity epistemically, only that these causal properties or home truths Hacking talks of can be operated with or understood in a theory-free way. This distinction between what is needed and what isn't needs to be in general much more carefully circumscribed according to historical cases. Epistemic roles on my understanding often act as a basis or frame for interpretation, including how to manipulate an object experimentally.

In accepting partly Hacking's intuitions in the experimental case but dispelling the idea that this can take place in a 'theory-free' way, what I would pose is that Hacking's ideas of using a concept in the experimental context should be generalisable to other scientific contexts such as theory making. When Hacking talks about manipulating entities he's really talking about using the entity concept to manufacture new experimental setups in order to produce new phenomena. Why should we not talk then about scientists 'manipulating' a concept then in a theoretical setting for other purposes? An entity concept bound up with its theory-dependent causal properties has a theoretical life, and can be treated metaphorically at least in a tool-like manner to generate new explanations, predictions and construct theory. In such cases successful outcome in experimentation, are in no real different category from successes producing these, especially in the classical sense that experiment tests theory [47]. Thus as theoretical concepts, theoretical entity concepts generate through theory, knowledge claims, which while not always perhaps as direct or as convincing as the outcomes of experimental uses of an entity, count as epistemic uses nonetheless, with a similar tool-like metaphor

[47] Although explanatory success may be argued to be categorically different from predictive success. It certainly doesn't usually have much evidential value if a theory is constructed to explain something. But it can have something similar to predictive success, where it turns out to be able to explain something it wasn't designed to.

attached.[48] Theoretical entities can provide a resource to scientists in both the theoretical and experimental realm.

1.2.5 Scientific Objects and the History of Science

This brings us to the last consideration. As already mentioned my project is itself an historical and historiographical one. It aims to give on the one hand historical accounts of particular theoretical entity concepts but also prescribes the manner in which such historical accounts should be given in order to gain a more complete perspective on the basis upon which particular research processes have developed and the vital role such concepts have played therein. It's interdisciplinary in this respect. In fact historians of science have recently themselves established a historiographical discussion reassessing the historical importance of entity concepts to the historical analysis of episodes in science, although historians talk of 'scientific objects' rather than 'entities' and many don't carefully distinguish the concept of the thing from the thing itself or, have reasons for not doing so.[49] In any case this discussion about 'scientific objects' is itself motivated by an attention to the historical epistemic values of entity concepts in scientific practice given the broad assumption that epistemology is itself historical and its own construction and development part of any historical account. But of course the principal interest of historians is formulating histories, and given their often central importance to research processes an entity concept itself, by tracking its lifetime, can provide a vehicle for organising a history and thus provide a rather different perspective on scientific processes. Entities cross disciplines and research programs and other classical historical frames of reference. They certainly aren't constrained by theories or even their own representations (as we saw with Hacking). In Arabatzis' terms they have a trans-theoretical character. This sense that entities aren't pinned down themselves by any one frame of historical analysis and the sense of their independence has led historians to talk of writing 'biographies of entities', and thus treating them as the central point from which to view, refer and understand the science activities going on around the use of them and the study of them.[50]

[48] Note I don't for this reason distinguish theoretical from experimental entities. Experimental entities are theoretical entities in the sense that they are creatures of theory.
[49] See for instance *Biographies of Scientific Objects*, ed. Lorraine Daston (The University of Chicago Press, Chicago and London, 2000); Hans-Jörg Rheinberger, *Towards a History of Epistemic Things: Synthesising Proteins in the Test Tube* (Stanford University Press, Stanford, 1997) and Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (The University of Chicago Press, Chicago and London, 2006)
[50] Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (The University of Chicago Press, Chicago and London, 2006)

What this has done I believe has raised the kind of issues that strongly suggest the underlying epistemic role of entity concepts which is required by both philosophers and historians to understand how research processes developed in the ways they have and the methodologies that have driven them.

Arabatzis' work on the electron is somewhat principal in drawing attention to the historical centrality of theoretical entity concepts sufficient around which to build and centralise historical accounts. He argues that this approach necessarily involves us in a two-way communication between history and philosophy of science. A philosophical reconstruction of the historical use of entities is required here because there is a certain complexity ontologically and epistemologically to them that requires philosophical analysis to understand. If we wish to talk about entities historically then philosophy is needed to identify what we are talking about here when we talk about objects or entities (the meaning and reference), how these things are constituted and what holds the different effects to which they are attributed together as manifestations of the same thing. These discussions are both guided by the history of them, but inform what it is that we are giving a history *of* and what are the salient aspects of such concepts *about which* such histories should be given. The essential point is that entity concepts are not so straightforwardly understood, and their role is interpretable only in the context of a philosophical framework.

In this respect I more or less agree with Arabatzis that when discussing the role of entity concepts in the context of historical research processes in the way historians seek to do, requires philosophical work to identify and track these concepts, in order to have a basis for identifying them as their descriptions change over time, even through complete conceptual shifts, and across disciplinary boundaries and fields of research. In which case we can then fathom how they are incorporated methodologically into the scientific decision-making that ultimately generated the historical events of interest. These require according to Arabatzis philosophical concepts for understanding the interaction between the kinds of properties entities have and the kinds of things they are, and between theory, representations and experiment. And it also requires some perspective on the reference of these concepts or terms.

My perspective on this then is to argue that the organization of histories around entity concepts as historians do has a philosophical basis underlying it in the methodological practices of scientists themselves and their use of these concepts. More strongly put, entity concepts through the way they have been used have played central roles in setting the conditions of, and providing the basis for, their own historical development and the research processes they are part. In fact such historical stories are necessarily bound up with the philosophical dimension of the epistemic use of these concepts, and to ignore this leads to misleading accounts of a concept's own development and of the distinction between concepts and research processes. In this way the historical centrality of particular theoretical entity concepts and their particular trans-historical characteristics used by historians to construct histories, can be seen to rest upon a more epistemic account of the roles of these concepts. The historiographical point then is that we shouldn't give histories of concepts merely as the outputs of historical processes, but rather we need to incorporate those concepts more deeply as part of the driving dynamic of historical conceptual change and development. Historians have over the last 10 years or so begun to pursue just this line of thought.

So what have historians identified as historiographically relevant and important about entity concepts, and how are these accounted for in epistemic terms? The central perspective of historians is that these objects have their own history independent of ideas about them, research programs, institutions or disciplines. In some respects this is driven by the productive value of these objects, and the essential importance of this productivity to research programs. Here then I want to single out some of these historical accounts.

In parallel somewhat to Hacking, Rheinberger and Buchwald emphasise the centrality of the experimental system, as the realm in which the production of events, machines and objects takes place through other than strictly theoretical processes.[51] This movement takes account of the fact that there is something deeply important to science about tools and tool-use. Science is not simply a process of picturing nature or telling stories about it, it is about producing, probing and exploring. Buchwald himself emphasises the importance of scientific objects transforming themselves into useful manipulable tools for further probing nature and posing new questions for their gradual acceptance in a scientific community. Productivity is the key. Where Buchwald treats this as essential for the historical understanding of the 'coming to

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[51] Hans-Jörg Rheinberger, *Towards a History of Epistemic Things: Synthesising Proteins in the Test Tube* (Stanford University Press, Stanford, 1997) and Jed Z. Buchwald, *Histories of the Electron: The Birth of Microphysics* (Boston, MIT Press, 2004).

being' of new scientific objects, Hacking takes it to be epistemologically essential to even knowing of a world outside our direct perceptions.

Rheinberger treats experimental systems as the basic epistemic units of science. Within this epistemic things have complex functions, which when applied or used can force the evolution of the experimental system in which they are bound, by producing surprising or unencountered results. Rheinberger documents the dramatic shifts in the focus and attention of Zamecnik's laboratory as it tracked through different 'epistemic objects'. The laboratory's original research was into the metabolism of cancer cells, but shifted through microsomes, to protein synthesis and eventually RNA. An epistemic object is foremost a question generating machine. Thus 'epistemic' for Rheinberger is cached out similarly in functional terms. In some way one might argue, Rheinberger perceives that these 'epistemic objects' frame research as themselves objects of research, something which will have resonance with what I say about theoretical entity concepts. It is in terms of the object that information is interpreted, understood and applied, and through whose agency experimental systems themselves develop. At the same it is subject itself to transformation and alteration. There is thus an essential dynamic relation between such objects and their experimental systems.

But Rheinberger's central epistemic agent, or functional unit of scientific practice, is in fact the experimental system, not the 'epistemic object'. The experimental system is the complex of practices that is central. The role of embedded epistemic objects is more or less confined to that of being an object of research, a centre of attention if you will. This is what makes them 'epistemic'. Its role is organisational in this respect. Their chief active function arises when, through the study of them, some novel result is obtained that leads to a renovation of the experimental system. There is no broader consideration by Rheinberger how such objects as particular kinds of concepts used in particular ways might contribute themselves to the direction experimental systems might take i.e. how they are constructed and interpreted. I have no doubt there is an important degree to which an experimental system in Rheinberger's terms is itself a construction of its objects in manner a Hacking would describe as reliance on the object as an experimental tool. It is just as possible in many instances to invert the perspective and to see the entity concept as central and the experimental system as dependent on it.

In any case Rheinberger wishes only a broad perspective from which patterns in history can be understood. My goals are to trace the particular ways scientists reason with entities as epistemic tools. Perhaps the differences are equivalent to the top-down/bottom-up distinction in explanation. In any case there is no reason to downplay that Rheinberger has something of an 'epistemic' understanding in my sense of the role of scientific objects at least in the experimental aspect of scientific practice.

Arabatzis' research is however somewhat more central to my observations. His work is dedicated more or less to elaborating the role and function of 'hidden entities', i.e. why they are born, how they evolve and why they pass away. This involves coming up with some historiographical method that can balance the historicity of such objects against their 'trans-theoretical' nature as experimental objects. In this respect in his text *Representing Electrons* Arabatzis talks of the *active role* of an entity's representation which motivates their treatment as biographical subjects.[52] As Arabatzis thinks, there are many properties of entities which we witness during their development that resemble the development of human subjects and would normally form the focus of a human biography. It is possible to interpret entities in varying circumstances as "active agents whose internal dynamics transcend the beliefs, abilities, and wishes of human actors and act as a constraint on the development of scientific knowledge."[53] According to him what really justifies an approach like this is that a theoretical entity is outside the control of any individual and indeed often transcends any individual's, group's or perhaps any social body's intentions and ambitions. They have as he puts it, a life of their own. As such a natural way of dealing with them historically is by writing their biographies.

An idea like this of course needs elaboration. For Arabatzis the representation of an entity is of central importance. Scientists represent theoretical entities with various properties which are themselves constrained by certain laws and principles. In the first place this theoretical baggage is relatively resistant to transformation and particularly attempts to enrich its theoretical structure in order to account for novel phenomena. Arabatzis' case study is the development of the electron from the birth of the concept in the 1890's until the advent of modern quantum mechanics circa 1925. To illustrate his point he refers to the attempt by Ernest Rutherford to fit a novel theoretical structure in the form of orbital motion to the electron to account for the structure of the atom. The entity however resisted this attempt to manipulate it. From its already

[52] Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (The University of Chicago Press, Chicago and London, 2006)
[53] *Ibid*, p 36.

established representation the conclusion was deduced that the entity would in such circumstances spiral into the nucleus. The theoretical scaffolding or ‘previously acquired character’ that entities carry with them, and had become in many ways permanent, prevented complete reinterpretations, and thus provided them with a measure of independence from theoretical manipulation. Part of this permanence Arabatzis asserts is associated with experimentation. “Theoretical entities embody, appropriately translated, quantitative and qualitative features of experimental situations.”[54] Experimentally inferred properties can often be stable, despite theoretical shifts. Although these are certainly theory dependent, their values remain fixed and thus act as constraints on future development. The charge-to-mass ratio was determined early on, and while the meanings of charge and mass both changed, the value remained the same, and had to be fit within any new developments. Indeed experimental observations of the entity in general have often become associated with entities to form some kind of permanent identifiers. These always require some interpretation within any new theoretical pictures, and thus act as constraints on these. Biographically we would interpret them as something like the ‘writings’ of these objects.[55] There is thus a continual interplay between these experimental constraints and the activities of scientists to force their own conceptions upon these objects. Scientists it seems aren’t all knowing or omniscient, they can’t predict all the ramifications of their proposals and the results when they are meshed with older theoretical parts. And this is why it is appropriate to talk of the *active* nature of entities.

At the same time Arabatzis makes mention of the ‘heuristic value’ of theoretical entity concepts. A theoretical entity concept contains in some respects the seeds of its own development. These are heuristic in the sense that they guide scientists in appropriate directions to further elaborate the concept. “..as soon as they are introduced, further questions about the unobservable entities in question arise and provide heuristic guidance to the practicing scientist.”[56] These can consist of unknown or unmeasured properties, inconsistencies in the representation, simplified assumptions or problems rectifying the representation with other established body of theory. Scientists rely on these as heuristics for directions of research and theory-construction. They are in this sense an investigative resource. Arabatzis considers this heuristic aspect part of their active nature. He gives the example of Thompson being guided by his concept of the electron to look for the charge-to-mass ratio and formulate a way of measuring it.

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[54] *Ibid*, p 37.
[55] There is clearly some similarity here to Rheinberger.
[56] *Ibid*, p40.

Arabatzis thus provides something like a strong role to entities in theory-making practices and experimentation. The idea of entities as active participants strongly intimates this. I think nonetheless the critical aspect of the historicity of hidden entities that Arabatzis identifies, which seems absolutely to require some kind of philosophical consideration of how these entities are being used and their seeming complex structure in this regard, is their synchronic and diachronic identity. Arabatzis asserts that when we talk about hidden entities in practice we need to keep separate the objects from their representations. Representations are always partial. As he puts it, "Representations are interest-driven and do not exhaust all the properties of the objects they stand for."[57] As such making no distinction between the object and its representation would compromise their sense as inexhaustible. This I think makes a similar methodological point to that made by Vienna Circle theorists that there is as part of the use of such a concept an acceptance that there is always more to be said about it and that what is known is at best partial. Secondly we have to be able to provide for genuine disagreement about scientists over an entity's representation, that is, disagreements over how to describe what they consider to be the same thing. Arabatzis gives the example of the early disputes over the electron's shape and structure between Abraham, Lorentz and Einstein, Bucherer and Langevin. Their positions were all tied in some way to the electron's attributed experimental manifestations, which provided the basis for Kaufmann to test these positions.[58] This kind of independence as we have already noted is also identified by Hacking. Of course on the other hand we have the phenomena of the persistence of entity concepts over time. The Thompson electron and the Dirac electron were very different in terms of their representation. But scientists have historically considered them attempts at descriptions of the same thing.

These observations about the historical behaviours of entity concepts justify writing histories as biographies of scientific objects, or structuring histories from the 'point of view' of the concept. It at once acknowledges their centrality to various historical research processes and their freedom from any individual's expectations. What I would say is that this activeness and independence is itself part of deeper story about the role of entity concepts that has yet to be fully teased out and incorporated into our historical accounts. Various historians such as Rheinberger and Arabatzis have pointed to an aspect of it, but there remains more to be said about the way in which theoretical concepts provide, or have provided historically, the conditions and resources for their own historical development, and I think this is fundamental.

[57] Theodore Arabatzis, *The historicity of scientific objects* (forthcoming), p 6.
[58] *Ibid*, p10.

When we talk about the activity of a theoretical entity concept in terms its contribution to a research process, it rests at base I believe upon certain formative and non-determinative epistemic roles and functions of that concept. Likewise when we refer to the independence of a concept from its representation we are really I believe pointing to the importance of these epistemic roles in facilitating research processes that themselves countenance the development of this description as part of that very research, but also different conclusions about these descriptions at the same time and conflicts in these representations. Thus they support the kinds of events that Arabatzis identifies as significant aspects of the biographies of entities. As such if our aim is historically to understand the part played by a theoretical entity concept in the development of a research process, then this should proceed by virtue of identifying their epistemic roles in the development of that research, as essential, often formative (or at least central), elements of that research. The resources and constraints research relies on to proceed. These provide themselves a frame by which we can structure such a narrative continuously.

What this means ultimately and this is what I think the historians above have more or less realised, is that we can't tell histories of concepts like theoretical entities simply in terms of the conceptual changes in those concepts (as a succession of 'conceptual events') without acknowledging that the concepts themselves have a methodological role framing these very developments through their fundamental part facilitating ongoing processes of research. To do otherwise gives the misleading account of concepts merely as simply end-products of research processes, and not more correctly as means by which many of those research processes have functioned. That at least is one the chief assertions of this dissertation.

1.3 Discussion

So what all these particular cases do I believe in their different ways, is put forward grounds for taking an epistemic approach towards the roles of concepts in general, and theoretical entity concepts in particular to historical accounts of research processes. To some extent this kind of perspective is part both of an older tradition and of a relatively new but still diverse movement in philosophy and history towards recognizing these epistemic roles as somehow central to the characterization and understanding of the concepts and their historical involvement.

That said what the discussions above provide us with is a collection of observations and considerations that inform us about these epistemic roles: how they operate, how to understand them, where to look for them and so on. In this respect there are a number of important elements that are worth drawing out here briefly since they are in essence the ideas this dissertation pursues.

1. Scientific concepts play epistemic roles in ongoing processes of research, broadly expressed as saying they act as resources and constraints, to relative degrees. This means that research process rely on these concepts to help constitute the possibility of further research by helping provide interpretative or conceptual tools for understanding and conceptualizing phenomena, and thus giving researchers some tractable grip on it for the purposes of further investigating it, generating sets of research puzzles and problems, and paths of investigation, as well as acting as experimental tools for producing and probing phenomena, and as explanatory, conceptual and inferential tools for resolving those problems. Of course expressing roles in these categories (explanatory, conceptual, investigative, inferential, experimental) is just suggestive of what might constitute the epistemic involvement of concept historically, which is itself open to investigation, and dependent on how a concept has been relied upon in any historical context to advance, or at least produce, research towards a set of defining goals for that research. They certainly aren't exhaustive as I've defined them. But they provide nonetheless what I consider a good general 'organising' basis for approaching such an historical analysis, which as we'll see is important for understanding the continuity in practice of such entity concepts.

2. Our understanding of the role of scientific concepts in research processes needs to be redeveloped, away from treating them as simply the ends of such processes, but rather as part of the means by which those processes develop. This implies understanding the ways in which these concepts play epistemic roles as resources and constraints for those processes. It is as such historically misleading to give an account of certain concepts simply by reconstructing them independently of these roles, i.e. in terms of their specific *meanings* at specific times. Concepts certainly are required to represent nature but confining concepts merely to this role ignores their more dynamic roles in the context of research processes. This necessitates arguably a different conception of scientific concepts than the traditional 'meaning-container' or 'ring-fenced' views.

What these roles are historically and how they've operated is of course a question of research itself.

3. Part of this more dynamical element is associated with the open-endedness with which concepts are often applied. The Vienna circle and their respondents took different perspectives on where this was located with respect to the concept (as an element of its meaning or not) but all agreed that it was part of the basis of its epistemic value, with which it could be used inductively, given new descriptions and applied in new situations. We can actually identify three ways this 'open-endedness' with respect to the use and characterization of the concept might be meant.

   i) The concept is acknowledged to lack certain aspects of description, like a knowledge of the inner structure or cause of the underlying entity, property etc, or of its kind categorization. This doesn't necessarily undermine its usefulness nor belief that it corresponds to an underlying reality and refers.
   ii) The concept is treated as open to the addition of more statements regarding it and not pinned to the known statements i.e. is treated as inexhaustible and always open to further investigation.
   iii) There is a disposition on the part of scientists in the context of research to change their minds about even central descriptive elements without abandoning the concepts and thus not to affix the concept purely to its descriptive elements.

   These all may be ascribed to a methodological position on the part of the scientists using and managing their concepts. It is a deliberate choice on their part how to work with them.

4. More broadly one would say that scientists in the context of ongoing processes of research anticipate redescription and revision as part of the necessity of their use of concepts when investigating complex and uncertain systems, whereby we understand their role as a basis upon which to further investigate, theorise about and experiment with these systems. As such the concept itself need not be treated as fixed in its description, but in fact it is often advantageous that conceptual meanings be allowed to shift and adjust as part of the very idea of the concept as a starting point or means for

investigation and not as a final fixed representation of the world. Yet at the same time through its epistemic roles it provides conceptual material for this investigation, theory-construction and experimentation. We have then a starting point at least to gauge the often puzzling methodological practices of scientists with respect to their concepts. From this perspective concepts aren't fixed at all but are built in to research processes as part of the set of tools by which they can operate dynamically and variably.

5. Of the epistemic roles we might extract historically for theoretical entity concepts, Hacking has gone some way to express what experimental roles include in the case of theoretical entities at least – probing phenomena and so forth. I don't however think we should believe that this occurs strictly outside a theoretical context. Nor do I think, as we'll touch on throughout this dissertation, Hacking fully expands on how entity concepts are used epistemically in this way, since they are importantly often manipulated experimentally for their own investigation, not simply the investigation and testing of other phenomena.

6. To some degree these epistemic roles provide an invaluable ground to generalize historically and philosophically about groups or classes of scientific concepts, which we categorise and discuss in terms of their epistemic roles. For instance natural kinds can and probably should be identified as a group by their epistemic role in research processes as units over which researchers can reliably generalize and in fact are interesting as a group because of these roles. Thus we can start to build up a methodological picture of science organized around the epistemic functions of such concepts. Likewise we can talk about the group of theoretical entity concepts, and study them historically in terms of their epistemic roles which it is argued in this dissertation give us insight into why and how scientists have relied historically relied on these concepts and how they have managed or resolved their 'unobservability' epistemically. This isn't a claim that these concepts are simply identical in their epistemic roles, only that there may be general things to say which are of historical and philosophical significance, and it provides an alternative to the more difficult ontological perspective which tries to argue that entity represents an ontological class with certain metaphysical and physical properties despite the fact that many such concepts represent things that are significantly different. If our aim is to understand

methodological practices then organizing research processes around the epistemic roles of groups of concepts means, if it is a valid approach, that we can reach general conclusions.

7. Turning then more specifically to theoretical entities, when it comes to such entities historians of science inform us of their potentially central role in historical episodes, as a centre scientific activity. As Arabatzis puts it, "entire domains of theoretical and experimental practice have been structured around hidden entities."[59]

These considerations then provide the basis for this dissertation. These are of course mostly quite general points and a chief ambition from this point onwards is to provide more substance to them through the application of them to particular cases and particular philosophical problems. In this respect I've chosen a manageable (and interesting for many reasons) class in theoretical entity concepts for which I think there is something important historically and philosophically to say about them in terms of their epistemic roles. The next is take these concerns and practice precisely what I've preached by trying to show how we can use the idea of epistemic roles to generalize historically about a specific class of concepts and their roles in ongoing processes of research.

[59] Theodore Arabatzis, *Hidden entities and experimental practice: towards a two-way traffic between history and philosophy of science* (forthcoming), p7.

Chapter 2: Theoretical Entity Concepts and their Epistemic Roles

From this point on I'm going to restrict myself to theoretical entities, or more precisely the concepts representing them. By this class of concepts, as specified in the introduction, I mean the historical class from the 19th century on of theoretical terms corresponding to unobservable/indirectly observable (or were at some historical point unobservable), thing-like, spatio-temporally located natural kinds that are imputed various properties, particularly various causal ones, such that we might talk of them as causal agents (e.g. atoms, sub-atomic particles, classical genes, force fields, continents…). This is not because I don't believe what I say doesn't extend to some degree to other kinds of scientific concepts (or theoretical terms in general, such as kinds, processes or properties). In the last chapter the observations I gave on the history of studying concepts from a more epistemic perspective crossed these boundaries. But the theoretical entities at least with these properties above we can group together in terms of their epistemic function to a significant extent. That is part of the historical perspective this chapter puts forward. Of course this is not a claim that all such agents are identical in their use, only that certain salient aspects of it are identifiable historically and can be talked about in these general terms.

The last chapter showed that there has been in the philosophy of science relatively sporadic interest in studying the use and practice of scientific concepts in terms of their necessity and contribution to the construction of theory and experiment and other elements of scientific practice. Concepts are necessary resources and constraints for practicing scientists to do science and are constructed not just to give accurate statements about the world but also a basis upon which to further investigate and describe the world. This is what I mean by the epistemic value of a concept. It descends from the now rather old idea that conceptual frameworks precede analysis of the world; that research questions and so on are all dependent on the language of investigation.

In this chapter I'm approaching theoretical entity concepts in precisely this way and for these reasons. My task is to elaborate what I think are the general epistemic uses that have tied the use of these concepts together historically, and show how this allows a fruitful elaboration of their historical role. I'll try to fathom thus the deeper underlying epistemic reasons for the construction in modern science of such unobservable causal agent concepts and suggest how their epistemic value has manifested itself through the different constructive uses that are made of them. Some of this does rely on some basic shared ontological aspects of these concepts and particularly their attributed causal agency, but it extends to their value in explanation, and their roles as concepts in conceptual networks, and also as objects of, and tools for, investigation and research. More broadly our interest is in the way causal agent concepts through their epistemic roles acts have acted as resources and constraints for the development of their own representations and the theories and experiments that makes use of them. I will suggest that an important aspect of this epistemic value has been an historical open-endedness in the descriptions or representations of these entities that has been sustained by, and is in fact part of, these epistemic values. Accordingly I propose that the historical roles of these concepts can be captured through the description of them as 'epistemic frames of research and investigation'. This shifts our historical understanding of the value of such concepts away from what their interpretation and meaning, towards their function of providing resources and constraints for scientific practice. Particularly it captures the sense in which these concepts have been used historically to frame the understanding of phenomena scientists have, and the very approach they take, through framing paths of investigation and relevant problems. In this respect we can fathom the central role and persistence that posing unobservable, or indirectly observable, causal agents has had in the history of science.

This chapter is to some extent thus rather abstract and its historical observations rather simplistic. But its ambitions are broad, which is from some broad historical observations on the roles of theoretical entity concepts to collect a set of epistemic roles and values which themselves provide a framework for spelling out more precisely and in more depth the epistemic functions of particular historical examples.

2.1 Some essential Epistemic Roles

So I begin this investigation here by outlining what I think are some of the significant central and essential historical roles that theoretical entity concepts have played in scientific practice. This involves elaborating what I mean by an 'epistemic roles' and the categories of them which I think have been important for theoretical entity concepts. As such I'm going to rely on explanatory, experimental, conceptual and finally investigative, in reconstructing their historical uses, and give a better sense of how these might relate to processes of research and to each other.

2.1.1 The Epistemic Value of Explanation

Explanatory roles as we've already expressed connect entity concepts to phenomena requiring explanation, for some purpose associated with the goals of a given research process. That is, explanatory hypotheses are made for reasons that at some level connect back to the aims of the research. This requires itself development of the concept to fit it to its explanatory purpose: i.e. certain property ascriptions. Of course these explanations happen in theoretical contexts that allow researchers to accept or judge a particular concept as 'explanatory' or at least potentially so with further development. The judgment of an explanatory role, assessments about what makes the entity explanatory, can however develop relative to changes and adjustments in this background theory, and the explanatory role acts epistemically in this regard as a basis for varying interpretations.

The explanatory functions of theoretical causal-agent entities are one of the most significant ways in which entities have proved 'epistemic', and invariably the reason for their introduction. The continents were posed in continental drift theory to explain geological phenomena, electrons were posed among other things as the cause of cathode ray phenomena and the internal structure of the atom, atoms were posed in the modern context to explain patterns of chemical reactions, the aether to explain the transmission of light and electro-magnetic phenomena, genes the transfer of heredity, and so on. The practice of then employing these entities further in new contexts was strongly bound up with their explanatory value. The basis for understanding this I think comes necessarily through an ontic understanding of explanation that sees the postulation of a theoretical entity as an attempt to gain an understanding of an aspect of the world through the designation of an agent in some way responsible for it or part of its structure. These explanatory relations between phenomena and entity become then essential epistemic connections which are relied upon for theoretical and experimental development. Scientists work with these connections as basic resources and constraints for their activity, e.g. through which they distinguish paths of investigation and research problems.

Of course 'scientific explanation' is itself a debated topic, with different conceptions of what it consists in. There remains a controversial question of how explanation works in actual scientific practice, with a strong movement towards emphasizing its contextual and pragmatic elements.[60] I think however that there remains among the standard conceptions of causal explanation, deductive-nomological explanation and explanation by unification useful descriptive value for the practices of postulating and deploying historically theoretical entities. In fact the structural elements that these types of explanations provide are elements that proved important for further use of the concept. All seem to be applicable to the history of theoretical entities at one time or another, although with different degrees of importance. Of these causal-explanation historically dominates, and will dominate my account below. I will give a brief outline of how these types of explanation have worked in the case of theoretical causal agent concepts and then proceed to discuss the epistemic value of these explanatory relations.

2.1.1.1 Causal Explanation

Historically causal explanation is the most significant epistemic role of an entity concept. It accounts for the postulation of many entities, including genes, atoms, photons, electrons, viruses, new planets and many others (but not all). And it is one of the significant ways in which entities are employed in new explanatory contexts to account for other phenomena. Members of this class are by definition causal agents which means they possess the causal powers and properties that enable them to function as explanatory concepts. While this use has evolved in a complex way in subatomic physics since the 1930's in the rest of science causal explanations and the supposition of unobservable entities, whether unknown chemical agents, or invisible planets, remains a strong dimension of scientific practice. Although there have been many unobservable causal agent suppositions in the history of science, it's origins in the modern context are perhaps connected to the history of atomic theory, which influenced and justified this methodological approach of causal explanation with unobservable physical objects or kinds in other fields such as biology. In the case of genes for instance atomic theory is explicitly cited by a number in the field early on as a rational to their approach of explaining and setting up a theoretical framework by virtue of an unobservable causal-agent.[61] That's however a story for another time.

Certain typical aspects of these causal-explanatory relations are the structural bases of the ways in which the concepts themselves are used. One critical aspect of causal explanation in

[60] Bas Van Fraassen, *The Scientific Image* (Oxford, Oxford University Press, 1980)
[61] See for instance T.H. Morgan, *The Theory of the Gene* (New Haven, Yale University Press, 1926), p1.

this respect is that historically the causal information doesn't need to be complete to be explanatory. This is in general a contextual issue about what counts as an appropriate explanation in the circumstances but in a trivial sense at least it couldn't be. The quantity of events that have a causal relation to a given event are extremely numerous. If we define causation in terms of counterfactuals (A would not have happened if B had not happened), then ponder all the things that had to happen for a car crash to have occurred. Wet road, dark sky, no lights on the other car, cars going too fast, etc, etc… The slightest change in any circumstance might have changed the outcome. In addition of course any causal chain can be traced back infinitely (ultimately to the beginning of the universe). Obviously causal-explanations don't work by giving full causal histories. They need only be partial and we are satisfied with that.

Less trivially however when entities are postulated or employed as explanations it is often only with partial information about the causal history of a phenomena, a partial causal mechanism, leaving many issues open to further investigation. This is an important aspect of the history of theoretical entity concept uses. The gene concept (see the next chapter) was formulated as a causal agent to explain discrete phenotype differences. It was assumed to have some causal role in the production of these differences at least, but the causal steps were not outlined and barely even conceivable by the biochemistry of the time.[62] The (moving) continent as a discrete scientific entity was posed as an explanation for many geographical features of the earth, but the exact causal pathways could not be provided, as no credible *causal mechanism* existed at the time to explain how continents might move.[63] In both these cases these postulations prompted controversy because of these insufficiencies. Appropriate partiality is a matter of degree and scientific community or individual preferences.

However what it reflects is an historical open-endedness in the use of such causal agents. They have been postulated as causal explanations and at the same time have created or framed problems for investigation. Scientists of course aren't often in a position to achieve anything else, but can nonetheless form beliefs about the tenability of such conjectures even though at times there may remain many open questions or outstanding issues. In this respect it is internalized as part of the methodology of using theoretical causal agents. Part of what science

[62] See chapter 3.
[63] Wegner failed to provide an adequate physical mechanism for continental movement and this was a major objection point for those opposed to the theory. Naomi Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Science* (New York, Oxford University Press,1999).

does is to further analyse and flesh out causal-histories given some presumptions about the causal agents involved. To put it in Lipton's terms 'understanding is not some sort of super-knowledge, but simply more knowledge.'[64]

But providing an element of the causal history of an event doesn't really tell us why scientists select unobservable entities as causal-explanatory causal agents. Salmon pursues a different formulation, which ties in explanation more firmly with the concept of 'understanding' and the need for a more ontic definition to capture scientific practice.[65] For him to give explanations is to show how events and statistical regularities tie into the causal network of the world. Scientists have historically formed the belief there is some causal-relational structure underlying the world, and associated understanding with having information about that structure. Part then of the investigational methodology of science is the assumption that coincidences in nature don't happen without a cause. Science acts to remove seeming miraculous occurrences by finding the underlying causal relations between events. If phenotypes show some pattern that gives a statistical relation between that observed in parents and offspring then it is assumed there must be some causal-explanation for this. The coincidence demands explanation. Where this becomes 'ontic' is by the acknowledgement that what explains a coincidence is some kind of mechanism or structure connecting coincidental events. It thus involves the postulation of physical things and causal-physical connections. Explanation in other words depends upon the elaboration of a physical mechanism. The causes of an event are a proverbial black box, with many interacting parts, some observable some not, which science attempts to delve deeper and deeper into to refine further what these parts are and how the mechanism works in terms of laws and principles, and thus give better explanations of a broader range of phenomena. Of course when such mechanisms can't be formulated with the aid of concepts representing observable things, processes etc then it is right that we postulate unobservable things to do this for us, because we take this causal structure to be there even when we can't see it. It is the quest for causal explanation that drives us to postulate many theoretical causal agents on this basis. Such entities have the function of filling out the picture of how things are caused.

If we take all this seriously, we can refine the historical epistemic function of our causal agents with respect to causal-explanation. Theoretical causal agents are used 'epistemically'

[64] Peter Lipton, *Inference to the Best Explanation* (2nd Ed., London and New York, Routledge, 2004), p30.
[65] Wesley Salmon, *Causality and Explanation* (New York, Oxford University Press, 1998), particular chapters 4&5.

to produce partial causal stories and provide partial mechanisms that account for events. Some amount of the ‘epistemic’ value of an explanation is that such explanation furthers our understanding of the world in terms of this ‘mechanism’ analogy. This understanding in terms of mechanism forms a basis upon which to conceptualise connections and relations in the world. It is has an important conceptual function imputing a structure to the world, even if very unclear and hypothetical, which can be used to frame further investigations. This will be talked about in more detail in section 2.4. What’s important from this ontic standpoint is that **a** causal explanation with a theoretical causal agent is explanatory because it poses some structure to the world by connecting events. This structure itself can be seen to underlie the usefulness of a causal-agent in its application and development as an explanatory, experimental and theoretical resource. This is what makes explanation epistemic in the sense that I’m using.

However this shouldn’t be taken to imply that causal explanations have been dependent for their success on having thoroughly worked out mechanisms. As already expressed, partiality is an historical feature of explanation and the mechanisms it puts forward. Part of the epistemic value of a causal explanation is that it provides a framework within which scientists can structure their own interpretation and investigation of events and phenomena, including the mechanism itself. Partial causal understanding is of itself useful understanding in the context of ongoing research.

What I want to emphasise here then is that causal-explanation with theoretical entities creates explanatory relations, which are epistemically useful connections between postulated things and phenomena making use of the presumption of unobservable causal relations in the world and black box mechanisms. Such causal agent concepts in this way act as resources for developing theory and experimenting where no observable cause seems available or sufficient to that task.

2.1.1.2 Explanation by Unification

The whole story about theoretical entities and explanation cannot be told strictly in terms of causal-explanation. Certain instances of the postulation of theoretical entities (at least in fundamental physics) can probably better be classified as unifications, and certainly established theoretical entities are often applied to unify diverse phenomena under the same principles, causal or otherwise. The unification model of explanation was developed first by Michael Friedman and then Philip Kitcher, as something of a successor to the D-N model.[66] It argues that scientists give explanations by showing that phenomena can be merged under a minimum number of general principles or independent assumptions, thus contributing to understanding in this way, rather than by providing a causal mechanism. Friedman's aim was to give an account of the explanation of laws, which Hempel's model had struggled to do. Of course this model fits nicely to certain instances in which laws are generalized, such as the reduction of terrestrial mechanics and heavenly instrumental laws (Kepler's laws) to the basic set of Newtonian principles.

With respect to theoretical causal agents it seems at least in higher physics that some entities have been proposed by way of unification of diverse phenomena. The quark was proposed for reasons of symmetry to explain the symmetrical relations between different kinds of hadrons (a type of subatomic particle including protons, neutrons and mesons) and their dynamic properties (such as isospin and strangeness) which could be represented by the SU(3) symmetry group. Murray Gell-Man famously illustrated pictorially the relations between these particles according to their properties, a representation referred to as the 8-fold way.[67] The complexity of these structures could be unified (in the sense of simplified) with the supposition that these particles were composed of even smaller ones known as quarks; baryons (protons and neutrons) with three and mesons with two each. It seems natural enough to talk of this as principally a *unification*, the idea being that this group structure could be reduced to, and thereby explained by, the properties of component particles forming an irreducible representation of the group. The interactions of Baryons and Mesons and their relative complexity could thus both be explained by a simpler system of (originally) 3 component particles and their properties. It is in fact usually thought that Gell-Mann introduced quarks as instrumental fictions, but this is not in fact the case.[68] The attitudes of

[66] Michael Friedman, "Explanation and Scientific Understanding," *Journal of Philosophy 71* (1974), pp. 5–19; Phillip Kitcher, "Explanation, Conjunction, and Unification," *Journal of Philosophy 73* (1976), pp. 207–212.

[67] M. Gellmann 'A schematic model of baryons and mesons', *Physics Letters*, vol. 8, issue 3, pp. 214-215; Also by George Zweig; G. Zweig, 'An SU(3) Model for Strong Interaction Symmetry and its Breaking' CERN Report No.8181/Th 8419 (1964), and 'An SU(3) Model for Strong Interaction Symmetry and its Breaking: II', CERN Report No.8419/Th 8412 (1964).

[68] "That is baloney. I have explained so many times that I believed from the beginning that quarks were confined inside objects like neutrons and protons, and in my early papers on quarks I described how they could be confined either by an infinite mass and infinite binding energy, or by a potential rising to infinity, which is what we believe today to be correct. Unfortunately, I referred to confined quarks as 'fictitious', meaning that they could not emerge to be utilized for applications such as catalysing nuclear fusion." Quoted in Peter Rodgers, "The Many World of Murray Gell-Mann" (Interview), *Physics World* (June 2003).

other physicists were mixed[69], but once experimental results began to support the existence of structure within nucleons and the concept proved robustly useful in other theoretical environments they changed their mind.[70]

The aim of the quark model was less about causality and more about providing a reason for the structure and variety of the subatomic world (the sort of structure that would seem to demand explanation according to Salmon). Explanation by unification thus in this instance appears to be the significant motivation for the introduction of the quark concept. Whether there are instances in which other theoretical entities outside the realm of particle physics can be said to have been postulated for principally unificatory reasons I leave open. In fundamental physics entities (more precisely their properties) are bound up with complex mathematical symmetry rules and group theoretic principles, which evoke certain mathematical expectations about the way the subatomic realm is organised. These were relied upon for the quark hypothesis. Potentially the original discovery of new atomic elements through the development of the periodic table to explain chemical reactions and substances would also count as the introduction of entities for the purposes of unification.

Like causal explanation, unification pertains to a particular understanding in these cases in the form of relations between certain unobservable entities and phenomena to be explained. These relations aren't necessarily causal in these instances, but rather unify the phenomena under various principles invoking these entities. These relations can be used by scientists to set up and guide their research and apply the entity concepts more widely, although under the rubric and motivation of unification rather than causation. We could thus talk about quarks in epistemic terms as particles of unification rather than causation. The history of quarks at least can be framed within a discussion of how the motivation for quarks, unification, was a guiding epistemic ideal behind the use of the concept as a representation of symmetry groups

[69] See A. Pickering, *Constructing Quarks*.(Chicago, University of Chicago Press, 1984), pp. 114–125. Pickering gives a much more complete historical account than I can offer here.

[70] At the Stanford Linear Accelerator, in an experiment in which electrons were scattered off protons, the electrons appeared to be bouncing off small hard cores inside the proton (A. Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 577). Richard Feynman analysed this data in terms of a model of constituent particles inside the proton (they didn't use the name "quark" for the constituents, even though this experiment provided evidence for quarks). R.P. Feynman, "Very High-Energy Collisions of Hadrons", *Physical Review Letters* (1969) 23 (24), pp. 1415–1417. Sheldon Glashow, John Iliopoulos, and Luciano Maiani recognised the explanatory potential of a fourth type of quark in the context of the Standard Model. S.L. Glashow, J. Iliopoulos, L. Maiani, "Weak Interactions with Lepton–Hadron Symmetry". *Physical Review D* (1970) 2 (7), pp. 1285–1292. A fourth quark allowed a theory that has flavor-conserving Z^0-mediated weak interactions but no flavor-changing ones. Work proceeded from there to the introduction of new quarks for explanatory purposes, in conjunction with experimental results and the eventual detection of some of these in the late 70s.

in attempt to resolve its own inconsistencies and problems (such as the nature and operation of the strong force between quarks) and its application to further phenomena (such as new forms of decay). Extending the explanatory capacity of the quark model meant preserving and extending its unifying capabilities. In the former case of the strong force this amounted to fitting new symmetry relations through a new property (or degree of freedom) labelled quark colour, and preserving independently the already existing symmetry structure of flavour.[71] The strong interaction was analysed through this framework in the field of quantum chromodynamics. The latter case amounted to the assumption of 3 new flavours of quark expanding the group structure of flavour to SU(6).[72] In both cases the explanatory relations of unification between quarks and the phenomena through the attribution of symmetries amounted to a resource and constraint - a methodological basis - for the development of the quark model. In this sense for the quark model explanation by unification was of fundamental epistemic importance. The way in which the quark explained played an important part in respect of what counted as problems and areas of investigation.

Physicists even if non-committal about the quark concept as the source of these relations did appreciate its explanatory value and this did motivate them to pursue the model further in terms of the language of underlying particles and in so far as they did they were constrained and guided in their use of the concept by its established explanatory relations. Development of the theory was framed in terms of quarks even it was only to be taken a useful way of theorising.[73] After all a theory of particles was available and familiar which scientists could rely upon with quarks to guide their research and formulate questions; for example quarks as fermions were expected to obey the exclusion principle. When it seemed one quark didn't under the simple flavour model, a research process began that led to the concept of quark colour and in turn explanations of strong interactions.[74] This very research process presupposed that there was a physical issue of how quarks as particles interacted.

[71] M. Han and Y. Nambu, 'Triplet Model with Double SU(3) Symmetry' , *Phys. Rev*. (1965) 139, B1006 - B1010.
[72] Many researchers were involved in this developments including Zweig and Pais himself. A. Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 559.
[73] "I can attest, however, that the early practitioners of SU(6) were 'thinking quark' a good deal of the time, not because they were sold on the reality of these odd objects, but because they gave such handy and concrete pictures." A. Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 559-560.
[74] M. Han and Y. Nambu, 'Triplet Model with Double SU(3) Symmetry' , *Phys. Rev*. (1965) 139, B1006 - B1010. Recounted by A. Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 562-562.

In fact the majority of historical uses of unification with theoretical entities has come not with the postulation as in the example above but with the application of it (its causal powers and properties) to new explanatory situations, thus unifying different phenomena or events under the one causal-explanatory framework or under causal-explanatory frameworks with a common element. The unificatory power of the atom has proved immense in this respect. Perhaps, as Salmon identifies, unification and causal-explanation are just separate possible goals or principles of scientific explanation valid in their own contexts. But they are not necessarily exclusive descriptions.[75] Additionally an entity concept might explain why several seemingly different entity concepts possess the same properties by imputing them to the same entity. The electron's representation is a classic example of this. The coincidence of Larmor's electron, Lorentz's ion and Thompson's corpuscle, particularly the charge-to-mass ratio, was explained by arguing that these were the same entity in each instance. [76] As such this unified concept was able to explain cathode ray phenomena, the Zeeman effect and electricity, and thus unify all these phenomena under the same principle. But this use of unification is separate from the contexts in which theoretical entity concepts are generally formulated. These unifications rely on established causal-explanatory functions, and thus signify the epistemic value of those explanatory roles for making further explanations and assigning new explanatory roles. Unification however remains a significant part of the epistemic use of entities and accounts certainly for part of their success.

2.1.1.3 Deductive-Nomological Explanation

While there are questions over the general applicability of the D-N model of explanation, there are several instances in the history of science where the postulation of a theory entity seems to have been strongly motivated by something similar at least to a D-N type explanation. Again the examples are from particle physics, which is unsurprising perhaps, because of the close association between the D-N models and mathematical laws of nature. In these cases the theoretical entity was posited to account for a perceived disparity between what a natural law predicted and an experimental result. I say something *similar* to a D-N explanation because these are not on their face strictly deductive-nomological in the sense that the law alone does the explaining or is the essential part as Hempel imagined it. The particle concepts are the principal part of the explanation in these cases, but the law figures too in the

[75] Wesley Salmon, *Causality and Explanation* (New York, Oxford University Press, 1998), p73.
[76] See Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (Chicago and London, University of Chicago Press, 2006).

explanans. But these explanations were not causal and there doesn't seem to have been any strong instance of unification involved (the new particle didn't seem to have been proposed for this purpose).

The most pure case of this is probably the postulation of the neutrino from the 1930's. Here conservation laws plus the hypothesis of a new entity deductively explained particular experimental results. Between 1909 and 1911 experiments by C.T.R Wilson and Lise Meitner, Otto von Baeyer and Otto Hahn performed an experiment that appeared to show that the energies of electrons emitted by beta decay ($n^0 \rightarrow p^+ + e^-$) had a continuous rather than discrete spectrum, in apparent contradiction to the law of conservation of energy, as it appeared that energy was lost in the beta decay process.[77] The attempts to discover monochromatic elements simply failed. By 1914 Chadwick[78] had a convincing experimental result strongly suggesting a continuous spectrum and this was confirmed in 1927 by Ellis and Wooster[79] In addition the spin of the Nitrogen-14 nucleus was measured as 1, in contradiction to value that should be obtained on the measured spins of the neutrons and protons of ½.[80] It thus seemed that the laws of conservation of energy, momentum and angular momentum were breached. The validity of these principles had already been put into question by problems applying quantum mechanics to the nucleus which we'll discuss in chapter 4. The neutrino debate and the explanation of β-decay became a further fertile ground for a debate about them. Pauli first postulated the neutrino (which he called a neutron) in 1930.[81] His goal was to make the β-spectrum 'understandable' which meant more or less being able to deduce it from this law. This required including in the premises the supposition of a new particle with the required missing values. To put this more formally the plan was to explain the experimental results, which was a surplus of conserved values on the left hand side of the nuclear equation compared with the right. Assuming the conservation law plus the existence of an unmeasured particle with the missing conservation values, explained the results of the experiment. Commitment to this law thus facilitated, through explanation of the β-decay spectrum, the hypothesis of the neutrino. As this commitment to the conservation laws won out over Bohr's

[77] A. Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 151-160.
[78] J. Chadwick, 'Distribution in Intensity in the Magnetic Spectrum of the β rays of Radium (B + C)', *Verhandlungen der Deutschen Physikalischen Gesellschaft* (1914) 16, pp. 383-391.
[79] C.D. Ellis and W.A. Wooster, 'The Average Energy of the Disintegration of Radium D', *Proceedings of the Royal Society* (1927) A117, pp. 109-123.
[80] R. Rassetti, 'Selection Rules in the Raman Effect', *Nature* (1929) 123, pp. 757-759
[81] In a letter to a meeting in Tübingen. See in Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 316.

position the neutrino gained more currency, aided by the development of Fermi's theory in 1934 of weak interactions.[82] It was accepted by Bohr in 1936.[83]

Another significant case of this appears to be Chadwick's postulation of the neutron through α-ray scattering experiments with Beryllium. This happens to be the case study of chapter 4. So I won't say much except to say that Chadwick expresses the situation in something close to D-N terms.

"The experimental results were very difficult to explain on the hypothesis that the beryllium radiation was a quantum radiation…. It is evident that we must either relinquish the application of the conservation of energy and momentum in these collisions or adopt another hypothesis about the nature of the radiation. If we suppose that the radiation is not a quantum radiation, but consists of particles of mass very nearly equal to that of the proton, all the difficulties connected with the collisions disappear, both with regard to their frequency and to the energy transfer to different masses. In order to explain the great penetrating power of the radiation we must further assume that the particle has no net charge." [84]

These were not in a strong sense causal explanations because in neither the neutron nor the neutrino case, neither are directly the cause of the phenomena under investigation. They are in actual fact effects. Both were outputs of a certain causal interaction. The strategy was to use knowledge of the component outputs of the reaction (in terms of energy etc) to provide information about possible other components. The cause of all these things lay in the causal interaction event (the decay process) itself.

Perhaps whenever particles have been proposed to preserve certain laws in given experimental situations, one may consider D-N explanation an important participant in the particle's postulation. And certainly as far as the properties of subatomic particles are concerned, many are reasoned from conservation laws and the like, and many D-N explanations are given in science with the properties of established theoretical entities that obey certain laws. In many of these cases causal and D-N explanation overlap, because an entity is postulated as a cause and the phenomena (or properties of it at least) deduced via the

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[82] E. Fermi, ‚Versuch einer Theorie der β-Strahlen. I' , *Zeitschrift für Physik* (1934) 88, pp. 161-177.
[83] See Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), pp311-320, for a history Bohr's shifting attitudes.
[84] James Chadwick, "The Existence of the Neutron", *Nature*, May 10 (1932).

properties of the entity and laws themselves. The D-N explanation is such cases provides the form of (and part of the justification for) a causal-explanation.

D-N explanations also provide explanatory relations of epistemic value like the other two kinds, although in a more limited sense. Particularly it links these particles to certain theoretical and experimental contexts, which provide a setup for further investigating and theorizing about the particle, but also the underlying principles of the reaction itself. This was certainly the case with the neutron concept, as we'll see.

2.1.1.4 Elaborating explanatory relations as epistemic relations

So what I've done above is give more or less a list of the types of explanatory roles that theoretical entity concepts have historically played and given some hints as to why we can understand these explanatory relations as epistemically significant in the way that these concepts have been used. Here I want to elaborate in a more general way on the historical value of these relations and give some more precise and concrete uses to demonstrate what kind of information and insight we get from studying such theoretical entity concepts with respect to the kinds resources and constraints they have provided scientific practice.

a. Explanatory Relations for Attributing Properties

Explanatory relations have played a clear epistemic role in the development of the representations of their entity concepts, meaning they have provided or been used as resources for that development. When theoretical entities have been linked to phenomena so as to provide explanations of them it is not unusual for the properties of the phenomena being explained to be directly reassigned to the explaining entity. This is often necessary for the explanation to work. Entities have gained characterization this way. In other words the explanatory relation is used to feed information to scientists about the nature of the causal agent, or in more active terms, is taken as resource to build up a picture of their causal agent. The apparent circularity of this worried G.N. Lewis. "The properties of electricity have been explained by assuming it to be composed of electrons, after which we naively consider the electrons as made up of electricity, and speculate concerning the distribution of electrons around the centre. We also have regarded atoms as possessing properties similar to those of the larger bodies of which they compose."[85] He reports on this as a "kind of inference modern science has sanctioned." Generally no explanatory value seems to flow from assigning properties to be explained directly to entities. In fact the explanatory value of entities like electrons and atoms was found in more abstract properties like their particulate nature, independence, causal-agency, etc. In other words the entities seemed the best explanations for other reasons, but they were still considered by their proponents to be explanatory despite properties being simply downloaded to their entities. There is a lot to be said about this phenomenon, and it has played an important historical part. Atoms were assigned bonding powers, to explain molecular phenomena, and many of the properties of elemental materials were simply reassigned at the atomic level. This raises of course the kind of methodological complaint Lewis put forward, that entities aren't really doing any explanatory work, they are just being assigned the properties that do the explaining. The answer to this is that the explanatory or epistemic value of an entity has come through other avenues, background presumptions and aspects of the concept. Larmor's electron wasn't posed because the electron concept itself could causally explain the powers of electricity using a different theoretical language. It was posed initially because the concept itself relied upon a minimal structure as an identifiable or separable part of electricity to which it was accepted that such causal properties could be jointly assigned and in this case located in space and time. These properties formed the basis of the mathematics that could reproduce the equations of the electricity.

This should be considered part of the epistemic value of an explanatory relation. Justifiable explanatory relations provide a basis upon which to assign properties to a concept and thus develop its representation. The explanatory relation and the properties that justify it support the attribution of these other properties to the concept which would otherwise seem arbitrary or unexplained. At the same time these other properties make the explanation complete. Genes had to be able to replicate to produce genetic phenomena; electrons had to have electrical powers to produce electrical phenomena and so on. Of course such reliance on on the explanatory role of a concept need not manifest itself only at the time of its introduction and initial formulation, but can be a continuing part of its development in ongoing research process where scientists are forced to reconsider the causal-explanatory mechanisms of their

[85] A quote from Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (The University of Chicago Press, Chicago and London, 2006), p13. G.N. Lewis, *Valence and the Structure of Atoms and Molecules* (New York, Chemical Catalog Co., 1923), p42.

explanations and revise the properties that need to be attributed to complete an explanation in the face of new data and new phenomena.

There is thus an epistemic interplay between explanations and representations in this respect which is worth further exploration in the context of more detailed case studies. Of course if explanatory roles can provide a basis for constructing representations, then they can also provide a basis for changing, or reconstructing, them too.

b. Explanatory relations as open-ended resources of investigation

What this last case suggests is again the point there is often certain open-endedness to the way explanatory roles have worked. Explanations good enough for scientists to infer that they are correct in their supposition of a theoretical entity haven't necessarily explained everything about the entity, but left aspects open to explanation themselves. This is part of what drives investigation with respect to such concepts. This idea of open-endedness with respect to explanatory relations can thus be more generally expanded upon.

As suggested with respect causal-explanations, explanatory connections have historically often been made with a clear sense that the connection or relation is not completely described or understood, even with the agency of attribution (which may of course may just raise its own issues of research i.e. these properties might require their own explanation), but is itself an object of further research and explanation. This is particularly the case with causal explanations. Scientists have accepted that the entity provides a causal explanation of a phenomenon but also recognized that the *causal mechanism* by which it operates is something unknown and open to investigation. Some scientists may consider it a detriment to the theory that such an explanation is not sufficiently had. The continental drift hypothesis' lack of physical mechanism for the movement of these discrete continents was the main argument used against it, despite its otherwise explanatory value.[86] In the next chapter we will see that the gene concept was formulated initially with no clear idea at all of how genes might give rise to differences in phenotypic features, only that there was some causal relation at work. The causal mechanism was entirely lacking. The actual answer to what kind of partiality in a causal mechanism can be permitted in a given circumstance invariably relies upon contextual

[86] Naomi Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Science* (New York, Oxford University Press,1999).

and methodological factors and thus varies from case to case, but has invariably formed a nexus of dispute among scientists.

In a broader sense an explanatory relation may in the context raise issues that require explanation themselves, which in turn provokes scientific activity. Of course the supposition of a new concept rarely comes without raising more questions. But explanations do often require further explanation. In some cases this leads scientists towards an investigation of the inner workings of the causal agent to explain its causal powers and properties, particularly if these seem to have an unaccountable structure or pattern. It was of course in the framework of the atom's causal-explanatory relations that the research program examining atomic structure, involving names as famous as Rutherford and Thompson was partially at least, generated. The suggestion of an inner structure was the result of patterns in its causal-explanatory relations expressed on the periodic table, aided by the discovery of the electron and radioactivity.

It is important to understand in these situations the role the concept itself plays in its own investigation. The established explanatory relations and properties of the concept are the basis upon which its own structure is experimented upon, investigated and ultimately described. It is not a free investigation…it is a guided investigation, and as such the original explanatory relations play an epistemic role in this regard relating the entity to other aspect of the world. We have already mentioned investigations of the quark being guided by its value as a unifying element, which formed itself a constituting element of its further use and investigation. The gene example in the next chapter will give an example of this practice, but it is a practice widely relevant to many unobservable causal agent concepts.

In most cases it is the *causal agency* of these entity concepts which provided these epistemic roles. Other types of explanation not making principal use of the causal-explanatory relations of an entity have themselves raised their own questions which required examination as we suggested in the previous section. The use of the neutrino as an explanatory concept in D-N explanation raised explanatory problems itself about the interactions involved, and how the neutrino came into existence. This was part of what led to Enrico Fermi's theory of weak interactions and eventually field approaches to the matter interactions. Even with these causal agents introduced by other forms of explanation the causal agency remained a vital feature of how the entities were conceptualized (both quarks and neutrinos were developed in respect of theories of interaction).

c. Causal-Explanatory relations as tools for investigation

Another really quite distinct way in which causal-explanation play an epistemic roles it seems is that it they give scientists a means for obtaining information about the underlying entity, since they are essentially meant to reflect an underlying real world relation. One direct use of this relation is via its use in experimentation. A causal relation once posed provides something to work with. Particularly they have been used to investigate the causal powers of an entity itself experimentally. By changing initial conditions and observing the effects these causal powers can be elaborated, and theories of its structure tested and investigated. The gene in the next chapter provides an ample example of how this was relied upon in practice. The causal relation between gene and trait was both a starting point of research into the gene concept, and also into the nature of the causal relation itself. The gene was constructed on the basis of it and investigated and further developed by virtue of it. In this sense the explanatory role was thoroughly epistemic. Thus a causal relation is not just physical but also epistemic. We single out causal-explanatory relations because they often have the role in the justification of the concept in the first place, reflecting the dual roles of an entity not just as an explanation but also as a means of further investigation.

Of course not all causal-explanatory relations can be used in such a way. The causal relations of continents to geological phenomena were not in any sense manipulable or controllable. Likewise there is not much one can do with astronomical phenomena like black holes in this sense. But of course these relations, as we've already mentioned, have other epistemic roles or value, as resources and constraints of investigation.

For those entities, which have an experimental manifestation, then causal-explanatory relations sometimes provide the context and tools by which they can themselves be measured. One of the best examples is Thompson's own use of the electron concept which is nicely detailed by Arabatzis. Thompson came to the conclusion by 1897 that the cathode rays were composed of small charged particles from atoms. He used the experimental data to infer characteristics of the electron given this assumption i.e. that they must be material and negatively charged given their motion through electric and magnetic fields. The assumption of a small charged particle of course was the perspective through which these behaviours could be understood in terms of such properties. As Arabatzis puts it, this "reminds us that the representation in question was not passively read from the experimental situation."[87] With the electron as his explanatory device he was able to develop further questions including questions of measurement, such as the value of their mass-to-charge ratio. He then constructed a cathode ray experiment to measure it. But of course Thompson relied upon the causal powers ascribed to the electron to make this measurement and thus ultimately his causal-explanatory solution to the cause of cathode-ray phenomena. In some sense he took advantage of this relation to structure his investigations and his understanding of the phenomena.

Of course while causal-explanatory relations can work very obviously in this respect, there is no reason to doubt that it might not for other types of explanation if not necessarily through the agency experimental investigation. In all case explanatory roles and their elaborations, set constraints on further conceptual and theoretical developments or descriptions of the underlying entity, while providing elements that themselves require explanation, thus setting the epistemic goals and requirements for such description to deal with. Further development of the quark concept relied on, and was bound by, its already established explanatory roles accounting for the group structure of hadrons and baryons.

d. Causal-Explanatory Relations for Manipulation

In support of this a perspective on causal-explanation which I think promises to give some insight into what has made unobservable causal agents concepts valuable as epistemic tools comes via James Woodward's idea of causal-explanation as tied to relations of manipulability. On Woodward's definition of causal-explanation, information is explanatory when it is relevant to manipulating, controlling or changing nature, in an 'in principle' sense.[88] I think Woodward clearly anticipates the functional value of causal-explanation through this understanding and it is certainly relevant to a consideration of the uses of causal agent concepts. The ability to manipulate an entity to produce different outcomes through its causal agency is a basic epistemic function of such relations. What one needs for

[87] Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (Chicago and London, University of Chicago Press, 2006), p97.
[88] Woodward, J, *Making Things Happen: A Theory of Causal Explanation*. (Oxford, Oxford University Press, 2003), p10.

manipulation are invariant relationships, which are the type scientists usually try to establish with causal agent postulations, thus giving themselves an object of manipulation. Such a postulation gives a mechnanism by which to produce more *information,* to use Woodward's words. In a more abstract sense we use a causal relationship to test out counterfactual questions by asking 'what-if-things-had-been-different-questions'.

Woodward is helpful here and right to identify manipulation as an essential aspect of causal-explanation. Scientists are certainly often motivated by the desire to gain control over phenomena and thus be able to direct it to practical purposes. But this is only one aspect of the story and he does miss I think the broader epistemic role that 'manipulation' provides in theory development. He sees the essence of causal-explanation to lie in its extension of our interest in manipulation and control of the natural world. However it seems to me that manipulation can be a means to an end and I would hesitate to propose that in the case of causal agent entities it is some kind of end in itself. Even in the case of Hacking, the ability to manipulate an object is important because it allows us to develop new experimental situations and test new phenomena. My addition then would be to say that manipulability is an essential mechanism which we take advantage of to generate knowledge towards achieving the goals of a given process of research. Woodward's insights about causal-explanatory relations can be I think be generalized this way.

It is worth pointing out that on Woodward's theory it is natural for causal-explanatory relations to sustain a degree of openness. On his theory scientists hypothesise causal agent entities and causal relations involving them as the seat of invariant relationships. Invariances are then identified without any need for strict spatio-temporal chains (per Salmon) or laws or even to unify or systematize. Lack of knowledge about aspects of such relations including the causal agent itself is no necessary bar to the use of them. "We have at least the beginnings of an explanation when we have identified factors or conditions such that manipulations or changes in those factors or conditions will produce changes in the outcomes being explained."[89] What makes a theoretical entity explanatory then depends on the background context and the like, but if so we identify these factors with the theoretical causal agent and it becomes an agent of our manipulation when we apply this causal relation in different circumstances. In this way Woodward's understanding of causality in scientific practice,

[89] *Ibid*, p10.

seems to import an important epistemic content to it, which is often relied upon in the case of theoretical entity concepts and forms a part of their use in ongoing processes of research.

e. Constitutive Properties

Finally it is worth pointing out the some properties and relations ascribed to entity concepts have historically acted as constitutive of these epistemic uses of causal-explanatory roles, as generally essential to the explanatory value of the concept (and thus the justification of it). These are the properties like the unit nature of electricity which were not attributed but provided the substance of the explanation in terms of a new entity so understood in terms of this property. These themselves are not historically free from interpretation (or open-endedness in this regard), but act I believe as a frame for interpretation, through which scientists have constructed their representations and interpreted phenomena (they are intimate with conceptual roles in this regard). When considered in this way the neutrality of the neutron and the stable unit nature of the gene can be looked on in these terms, as central to the explanatory roles of the those concepts initially, and their further explanatory applications, and then to the conceptual uses of them. We'll elaborate on such properties in the next section.

Considering then the various kinds of explanation for which theoretical entities have been applied, what I've tried to do in this section is set-up in an admittedly broad way, what I think nonetheless form the kinds of epistemic values that explanatory relations have been provided researchers by their theoretical entity postulations. In general the historiographical task is to explore how scientists for different entity concepts have relied on these roles to structure their research.

For these purposes then, we can define such as role as follows;

> <u>Explanatory role</u>: Concept A plays an explanatory role for research process R, when it is assigned as an element of an explanation of a phenomenon or phenomena (or of a conceptual problem) that is an issue for this research process for the purpose of progressing towards its epistemic goals. This is dependent upon and defined by a set of 'constitutive' properties that are historically basis to the construction of these

explanations and also to justifying belief in the existence of the entity. It is the historical task to identify what these are in a given case.

2.2. Entity Concepts as Conceptual Resources and Constraints in Theory-Building

The second epistemic function of these causal agents which I'll raise here stems from their nature as a scientific concept. This aspect has many dimensions, but it arises from the fact that the basic stuff of scientific theories are concepts and their relations. As concepts theoretical entity concepts have provided structure to the unobservable world and to phenomena that require understanding. To quote Woodward,

"…we introduce concepts (including concepts of cause and explanation) and characterize them in certain ways at least in part because we want to *do* things with them: make certain distinctions, describe certain situations (which usually requires being able to tell whether the concept applies, on the basis of evidence that we have some possibility of getting), calculate with them, use them in proofs or arguments, and so on."[90]

Concepts work on the level of conceptual frameworks and networks, and it is out of these that theories and representations are composed. The function of theoretical concepts within these networks is another way of approaching their epistemic role in scientific practice. This approach is complementary to and overlaps with the thoughts on the epistemic value of explanatory relations above. Conceptual schemes are built with respect to explanatory relations. But there are uses of concepts that contribute more in terms of providing resources and constraints for the way in which conceptual frameworks can be structurally assembled.

2.2.1. Roles in theory-construction

We have already noted in the last chapter that Thagard gives a pretty reasonable list of the functions of concepts. These include categorization, learning (an aid to learning things about the entity or kind), memory (an aid to memorizing things about the entity or kind), deductive inference, explanation, problem solving, generalization (learning new facts about the entity or kind from new examples), analogical inference (use the entity or kind to make similarity judgments about others), language comprehension and language production. However where

[90] *Ibid*, p8.

theories meet the world is not through single concepts, but in the form of conceptual frameworks, which are composed of part/whole, kind, property, law-like, and causal relations between a collection of different concepts. What is the role of theoretical entity concepts within these networks and how is it epistemic?

In an abstract sense theoretical entity concepts have played the part of conceptual structures with which and around which conceptual frameworks could be erected or composed. In some instances when approaching the unobservable world the hypothesis of an entity has been the starting point to representing it with a theoretical structure. In other instances entity concepts have been used to provide completeness or order to conceptual structures that have problematic aspects (the hypothesis of the neutrino for instance). In any case one can talk about the relative centrality of a theoretical entity concept to a theoretical or conceptual framework, and the degree to which it is relied upon as an essential element of the framework around which frameworks are revised. Fundamentally explanatory roles contribute to this as do the ties to experimental contexts. But also historically there is a tradition in modern science which is perhaps being restricted in higher level physics (but not other fields) of building scientific theories around entity concepts, of starting with these concepts first, and building up their relations and structure second. Entities (and kinds) as the traditional bearers of properties and relations are of course essential to ontological frameworks or conceptual languages, which has been noted by many philosophers through the years. In the case of theoretical entities what has made them historically valuable is that they have provided structure to unobservable aspects of the world that provide a conceptual grip for resolving problems at the level of observable phenomena, but also for further theorizing about that unobservable aspect.

In fact I would put it that there are really two main conceptual functions for theoretical entity concepts. The first is as a central part of conceptual framework, elements out of which theories are built. The entity concept thus provides the function of a resource for putting together theoretical positions or models that contribute in some way to the research process through for instance enabling descriptions, calculations, proofs and predictions. Often it is part of developing explanatory roles, through constructing theories that provide the mechanism for them. This role is thus to some extent an abstract one. What gives it content are particular properties and relations associated with the concept that are constitutive of its roles in theoretical construction stably in the context of an ongoing process of research. Their role in this regard is defined thus by their centrality to the demands of the research process to achieve a theoretical understanding or description through the application of the concept, to particular phenomena, and by the properties and relations that are fundamental to that construction. These properties and relations are historically identified but closely associated to the possibility of conceptual framework (model and theory) building with these concepts. In many cases the conceptual use of an entity concept relies on its discreteness and independence to construct mathematical models, that group these and explain with their collective behaviour. Others as well see in the gene case relied on their unit structure for decomposing material into unit parts that could then be conceptually related. Knowing what these are requires examination of individual cases and piecing together how they resource conceptual construction.

The important element in this respect is that fact that this role is not a static one, but a dynamic one. We try to understand what the consistent elements are that form the succession of conceptual responses, in the form of theory and model construction with the concept, to certain research problems. For example the developmental processes of individual theories such as the development of Lavoisier's views on the causes of calcination and combustion, which went through considerable change from 1772 to 1789, can be centralized historically around the conceptual role of his oxygen concept.[91] The one fixed point of his conceptual system was air, later to become oxygen, and its presence in calcination. Around this idea Lavoisier structured a succession of conceptual systems, which ranged over and gave varied answers to how to categorise air, how to formulate its interactions with other substances, and what its involvement in chemical reactions might be. These all changed, but the concept retained its place as the central element of the conceptual system even though its properties and relations changed markedly. The constant element which was constitutive of the construction of this system for Lavoisier, given its central explanatory role, was its relation to calcination and combustion, as an entity that acted through absorption into these materials. Lavoisier felt the problems of combustion and calcination could be solved by the presupposition of a certain theoretical entity that worked on this basis, and used this as the starting point for the construction of succession of conceptual frameworks which Thagard nicely documents.

[91] I'm relying here on the account of the formation of oxygen theory given by Thagard who is particular interested in reconstructing the stages of conceptual development in Lavoisier's thought. Paul Thagard, *Conceptual Revolutions* (New Jersey, Princeton University Press, 1992).

In the second respect these conceptual roles are also part of the basic tools researchers use to structure the phenomena connected with the aims of this role (phenomena 'under investigation'), in such a way that it makes amenable to further investigation and conceptual development. In other words it helps through the particular concept involved, conceptualise an aspect of the unobservable world, that provides a basis or starting point with which to work towards an understanding, explanations, problem resolutions and so on. This is part of what a particular conceptual role provides researcher. If the goal of a researcher is to generate a theoretical position using the entity concept through certain central properties, to create a theory explaining or describing certain phenomena, the concept provides at the same time a basis for the interpretation and structuring of it centred on these constitutive properties. We'll see later for instance that the gene concept was very much relied on to provide a way of visualizing and structuring genetic material, for the purpose of provide some kind of conceptual starting point for working towards a more complete understanding of the operation of that material. Of course the basic ideas of particle theories of matter, have taken the particle concept as starting point for the conceptualization of the forces and structure of matter. In this sense they act as a basic conceptual 'grip' on aspects of the world, for the purposes of further conceptual network building.

In either case these roles are meant to be 'epistemic' in the sense of using these concepts towards particular goals. It is also meant to reflect that what a role has to account for is not fixed or permanent, but continually updating with new experiments and information, which themselves require the adjustment and rebuilding of conceptual frameworks. But as in Lavoisier's case the entity concept continues performing its roles in this respect, through properties constitutive of it playing this role, providing at the same time a conceptual starting point for interpreting such new information.

In these respects then we define conceptual roles as follows;

> <u>Conceptual roles:</u> concept A provides a conceptual role for research process R, when it provides a conceptual resource for the purpose of constructing conceptual frameworks (in the forms of theories or models) for precise purposes that aid this research process towards its goals, by providing relevant descriptions, making distinctions, constructing proofs, giving a framework for calculations, and so on, by relying on basic stable 'constructive' or 'constitutive' properties that are historically identifiable and which

provide the basis for these frameworks to be built (which in the process will include the addition of new properties and relations to the entity as part of ongoing research). In addition part of this role of a concept creating a conceptual framework for specific purpose connected to ongoing research, is to provide a basis for the interpretation of observations, phenomena and data in terms of the entity, thus providing a concept-laden approach to the world that acts as starting point to further theory building and experimentation with respect to those observations etc….

2.2.2 Constitutive Properties: Properties for theory-building!

I've raised a few times already the fact that certain properties ascribed to the underlying entity play a significant if not formative part in the operation of these conceptual roles. These properties or relations have been to some extent almost constitutive of these roles, as the basic elements out of which conceptual frameworks are constructed and reconstructed, and phenomena interpreted, as in the case Lavoisier and his treatment of his air or oxygen concept as the causal agent for combustion and calcination through its absorption (rather than emission as in the case of phlogiston). Relationally then metals absorb oxygen; oxygen causes combustion: were the relations at the basis of his conceptual treatment, and his own conceptual development. Without these properties there would have been nothing for Lavoisier to build on.

We might in fact call these properties themselves, 'epistemic properties' precisely because of the epistemic work they do. What I want to do then is expand upon the kinds of properties that have been historically important and stable for theoretical entity concepts as constitutive of ongoing research in this regard. Of course these are 'general' comments, not every observation I make applies equally to all historical cases. They serve nonetheless to flesh out the relations between certain properties and conceptual roles.

2.2.2.1 The role of basic metaphysical components

Metaphysically one relatively staple use of these concepts is the ability to more or less collocate properties either in time or space. In the introduction I included thing-likeness which I consider the equivalent to this ability. These agents are thus property place-holders which situate these packets of properties in time and space. The conjunction of properties has been essentially relied upon in theory-building applications of such concepts. Thus Thompson's theory of the electron relied on the simultaneous presence of mass and charge.[92] The early gene was given simultaneous powers of regeneration and causal powers to effect development.[93] The quark model was developed precisely by taking advantage of this to suppose the property of colour in addition to flavour. Of course the ability to collocate properties in an entity is subject to background theory (which can be associated with which etc), but the fact that this has been to some extent possible with many entity concepts historically reflects on their flexibility, and thus value, when looked at in epistemic terms, albeit at a highly fundamental metaphysical level. Something similar can be said of the way relations can and have been conjoined to such concepts. These often have some kind of spatial and temporal dependence, which itself relies on taking advantage of the entity as thing-like, and thus relatable to other ontological elements. This includes part/whole and kind relations, and also of course causal ones. With this we can give some kind of flesh of course to the idea that theoretical entity concepts have had a valuable role as building blocks in conceptual networks.

Other metaphysical or basic properties have played a strong part in the way theoretical entities have been used historically. They generally share for instance the presumption that they are natural kinds and thus can be generalized over on the basis of some similarity amongst their members. The presupposition of natural kindness as I mentioned in chapter 1, should be taken primarily as an epistemic one. It governs how the concept is used in explanation and inductive generalization, especially how properties are assigned across the class, and also investigated, as a set of objects with an assumed common underlying basis for this inductive and explanatory power. Particularly important conceptually is the ability to generalize over the class and thus treat them as something which can be at the basis of generalizing conceptual network (a theory or model). This has historically a central place for theoretical entity concepts which usually trade on their identity or some strong class of similarity between their members for explanatory success. Particle physics of course is founded on the identity of different particles (to the point indistinguishability). But in the case of something like the gene this has been important too, where historically it was used as a class which could be generalized over, and general statements made. In modern terms part of the disruption of the

[92] See Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (Chicago and London, University of Chicago Press, 2006), pp. 94-105.
[93] See chapter 3.

concept is the fact that this might not be possible at all, and what is really represents by the gene is so complex and diverse, that term ‘gene’ itself is no longer of any conceptual value.[94]

Others too come up as often very important resources in theory-construction with theoretical entities. These are not shared throughout and have a history in physics of being abandoned. Discreteness (spatio-temporal separateness) for instance proved invaluable to the development of the kinetic theory of gases. It proved vital to the way in which the classical genetic theories were developed. Likewise the continental theories relied upon being able to treat continents individually and talk about their movements independently. Quantum theory has made it much more difficult these days to identify ‘discreteness’ with particle concepts even though of course it used to be at the heart of the way macroscopic phenomena were explained in terms of particulate elements. But it remains fundamental in various fields.

With other theoretical entities discreteness was never important. With physical fields for instance or wave entities, this kind of independent non-overlapping locality was not a criterion. In fact they can overlap in which case they can usually be operated upon to produce a single field or wave entity. Fields were introduced precisely to find another way to talk about the conveyance of something from one location to another in terms of a discrete body, but allow a continuous form to do that itself and act causally over a broad space on elements within.

2.2.2.2 Low-level properties

These more metaphysical properties are of course to some degree always present, but mostly they’ll slip under the radar of our further description because they slip implicitly into the practice of using theoretical entity concepts and their explanatory value. All these properties of course have a more metaphysical ring to them, and don’t specifically identify the entity from any other. In any individual case certain constructive or constitutive properties are particular to particular entities and can be identified by analyzing the ideas about their entity researchers relied on and inherited from one another in their attempts to construct theories. The use and interpretation of these properties depends as always to some extent on background presuppositions. Charge and mass for instance are fairly basic properties of particle concepts and have played a variety of epistemic roles in the whole tradition of using

[94] See particularly chapter 5.

particle concepts to theorise about the atomic and subatomic world, even through significant conceptual shifts. The gene concept was centralized around its unitary nature and presumed ability to reproduce itself, which formed the basis of the way genetic material was approached and theories about it and hereditary phenomena were constructed. Many such properties are closely connected with the explanatory roles of the entity concept. The neutron concept's neutrality of charge was the basis of its explanatory use, but also of its conceptual use in the construction of radioactive formulae. But it is not always perhaps the case that properties needed for introductory explanations are necessary for all its conceptual uses. The neutron's presumed subjection to quantum principles allowed it to be the centre of conceptual role towards giving an account of the structure of the nucleus in terms of quantum principles, but this played no role in the original explanation that justified the entity's introduction.

To some extent then Hacking's intuition that theoretical entities possess low-level causal properties might have an overlap with these constructive properties. Experimenters do work with such properties in designing their experiments. In many cases the properties themselves are closely associated with experimental situations and set-ups and can be interpreted and used within these environments. It is certainly the case that experimental situations and phenomena associated with causal agents, expressed for instance through causal-relations or properties such as a charge-to-mass ratio, have consistently been at the centre of different conceptual frameworks or theories using the entity concept. For Arabatzis this is a prominent part of the history of the electron. In the early 20th century different accounts of the electron's shape and structure from Lorentz and Einstein, Abraham, Bucherer and Langevin which were all referred back to Kaufmann's experiments on the velocity dependence of the charge-to-mass ratio, taking that 'experimental entity' to be coextensive with their 'theoretical' one through their reliance on these properties.[95] It is an empirical question whether this is always the case (at least for concepts that have a significant experimental life), but is probably likely to be the case where the entity acquires its properties through its explanations of certain experimentally occurring phenomena, where these set-ups then form the basis of its future experimental production and use. The neutron concept in chapter 4 is a significant example in this respect.

In any the elaboration of such properties requires case by case analysis of those using a concept across time and research fields, searching for those constitutive of the epistemic use

[95] Theodore Arabatzis, 'The Historicity of Scientific Objects' (*forthcoming*), p10.

of their concepts. It's not one that can be done *a priori*, although in many respects some such properties can be seen in their historical contexts as fundamental to the value of the concept in theoretical construction. Whether they overlap with Hacking's low level properties, and the properties experimenters typically rely upon is itself an empirical question, and would certainly be an interesting one, as it would tell us something quite deep about the reliance on theoretical entity concepts, and the connection between theory, experiment and the core beliefs about an entity.

In general what makes these constructive or constitutive properties important and interesting, where they are identified, and what is in fact their defining character, is that in the context of ongoing research they provide stability to the concept which resources and constrains possible theoretical responses employing it. Because they are used for interpreting phenomena in terms of the entity concept in this way they are constitutive of approaches and less themselves variables of it to be changed and reformulated in the process of conceptual change. They make the very use of these concepts in varying research environments possible. This should not be taken to imply however that such properties and their uses are not open to interpretation and reinterpretation. This is a fairly regular historical occurrence, and in fact many development processes work at giving more precise or better interpretations of these properties. But the important matter for my purposes is that they retain their epistemic functions at the centre of the conceptual roles these entities play for ongoing research.

2.2.2.3 The epistemic roles of representations

It would be however important however to generalize that these conceptually and explanatory roles, reliant as they are on these properties as I've defined them, do not capture completely the epistemic roles that theoretical entity concepts have played. Representations of theoretical entities in general have acted as epistemic units in the use of those entity concepts. A theoretical entity concept is always the subject of representation i.e. attempts to describe, or account for what it is via models of it etc. Part of what a concept of a theoretical entity does is carry a representation of the underlying entity. As noted in the last chapter in the discussion of Arabatzis' work, the established representation of an entity is itself a resource and constraint for practicing researchers. Arabatzis identifies the fact that representations or attempts to represent theoretical entities have a history in the electron case of generating problem or puzzles that provide heuristic directions for theory development and experimentation.

Quantum mechanics developed partially this way, as a response to the problems of representing electrons. What we can say about these is not this reliance is of different nature from that I've talked about above, but rather that is part itself of the dynamics of pursuing a conceptual role. In this sense the role can be identified at the basis of these patterns of research. My approach then differs for Arabatzis to the extent that I'm more interested in discovering historically these elements which structure research and how they form a frame for that research and indeed these disputes over representations, rather than the resulting dynamics of the research developments that are at some level relying on them.

Nonetheless it is important to be aware of the ideas around which an entity concept could be said to have acted epistemically. Another important example which would count for me as a specific example of how conceptual and explanatory roles become elaborated at a low level, are *theory-constitutive metaphors*. These metaphors which we more closely examine in chapter 6, were introduced by Boyd and elaborated by Cummiskey, but also recognized by Achinstein, refer to the analogies that are often possessed by entity concepts from their very introduction which are used by scientists as a guide to the nature or behaviour of the concept they are proposing, as vague as it might be, presenting something like a loose interpretation of a term in which properties and descriptions can be formulated more precisely.[96] In this sense they have epistemic value as general investigative and problem solving heuristics for dealing with the representation of the entity itself and employing it theoretically. Usually it is the connection of the analogy with something familiar and tractable which provides guidance as to how to deal with the unobservable entity at hand. Electricity for instance was originally considered as a fluid. At various stages, the brain was considered like a computer system, the gas particle as an elastic ball, the ether as a fluid and atoms like planetary systems. In these cases the metaphor provided a grounding for research into the entity and its behaviour and/or the way it would be applied in developing theory to explain particular phenomena. These metaphors are not permanent and in many of these cases above have become inadequate, but they do serve as elaborations of conceptual and explanatory roles that act epistemically for researchers involved, and thus display I think the primary importance of these roles as a framework for just such constructions.

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96 R. Boyd, *Metaphor and Thought* (Cambridge, Cambridge University Press, 1979); D. Cummiskey, 'Reference Failure and Scientific Realism: a Response to the Meta-induction', *The British Journal for the Philosophy of* Science (1992) 43(1), pp.21-40; P. Achinstein, *The Concepts of Science* (Baltimore, The John Hopkins Press, 1968).

2.3. Experimental Roles

A lot has been already written about this since it is perhaps the one aspect where the epistemic value of theoretical entity concepts has already been appreciated. Hacking and Cartwright are inspirations for this approach from the philosophy of science, Arabatzis, Gallison, Rheinberger and Buchwald from the historical side. This aspect only applies to those entities that have an experimental life. Not all do. Neither continents for instance nor black holes are easily discussed in those terms. However other concepts have had strong experimental lives, and persist in some way as objects of laboratories, somewhat separate from their lives as entities of theory. Generally however there is a two-way interaction between experimentation and theorizing (for this see Steinle).[97]

In the case of this experimental life there is no doubt some basis for Hacking to talk about the manipulation of entities. Experimenters have made use of understood causal properties to fashion new experimental set-ups and test the relations of other phenomena. In this sense they have acted as epistemic resources. But there is more to it than this. Causal agents have provided through these powers essential parts of interpretative frameworks for interpreting phenomena and experimental systems. When they have tested phenomena using causal agents they have done so because they are able to interpret phenomena with relation to them. In Hacking's example of parity conservation, that conservation was framed in terms of behaviour of subatomic particles and tested as such, through the agency of electrons, on the basis that 'slightly more left-handed polarized electrons hitting certain targets will scatter, more than right-handed electrons.'[98] If Hacking is right about the involvement of causal agents in experimentation, and its relative independence, then we have to understand that these agents have played a role in framing their understanding and response to each new challenge. Experimenters have 'thought in terms' of these agents, through the control they have had of them via their causal properties, which were linked to (or manifested by) experimental equipment and set-ups. They planned accordingly. This is what comes with having 'manipulative' control of entities.

What do we talk of then, as being the experimental roles of a theoretical entity concept? I think there are two ways historically that such concepts have aided research processes. The

[97] Friedrich Steinle, 'Experiments in History and Philosophy of Science', *Perspectives on Science* (2002) 10(4), pp 408-432.
[98] Ian Hacking, 'Experimentation and Scientific Realism', *Philosophical Topics* 13 (1982), pp 154-172.

first is in the sense of Hacking, the testing or probing of phenomena, but also the testing of theoretical positions. The second is the actual use of the concept through manipulation to generate new phenomena. The first we have just talked about. Certainly when a concept is well established it begins to play these kinds of roles (not the other way around as Hacking asserts), and its part of the idea that theoretical entity concepts play a role interpreting phenomena; e.g. its causes or structure. This use can become an embedded part of a process of research, as well see in the case of the neutron, where through its basic properties and relations it acted as a central resource for testing theories about nuclear forces and nuclear structure, and probing the inner structure of the nucleus. However what Hacking misses I think is that these roles also include the investigation of the underlying entity itself. That is, experiments often rely on the basic properties of the entity and the established experimental set-ups (for which the entity usually plays an explanatory role) to investigate the properties, behaviour and inner structure of the underlying entity. As I'll suggest repeatedly throughout this dissertation, such investigation if not at the outset, often becomes pertinent to a process of research, in the course of which it is realized (if not at the outset) that having an understanding of it is necessary to achieving its goals. As such the experimental role that an entity concept has for testing theoretical positions, often involves testing claims about the entity concept itself, its properties and relations or simply probing them. In the case of the gene this was more explicit than in the case of neutron, since the structure of the gene was itself realized as simply a necessary part of achieving an understanding of the processes of development.

The second type of experimental role I want to identify overlaps with the first, but is nonetheless historically important enough to single out. This is the use of entity concept, through something like manipulation in Hacking's terms (conceptually at least), to produce or generate phenomena relevant to the process of research. Hacking doesn't explicit talk about this, but as I've tried to emphasise so far, we're trying to take account of the fact that research is itself dynamic and constructive conceptual (theory or model building) processes aren't fixed to a certain set of information, but rather have to deal with dynamic sets of information for which the standard methodology is piecemeal accumulation. Part of achieving the goals of a process of research however revolves around expanding the amount of information available for the purpose of constructing more accurate theoretical positions. This accords of course with this methodology, and the belief that improving theoretical positions generally goes hand in hand with the accumulation of information. As such entity concepts are often explicitly employed in experimental contexts to explore phenomena for the purpose not of testing them, but for extracting new phenomena relevant to the process of research. This is a strong characteristic of the neutron concept for instance.

To summarise then:

> Experimental roles: concept A plays an experimental role for research process R if it is employed through property assumptions regarding the entity and agreed experimental contexts for which the entity is usable or manipulable, to either test phenomena or produce new phenomena considered in context relevant to the goals of R.

As I suggested somewhat earlier, it is an open question to what extent such experimental uses depend mainly upon Hacking's low-level causal properties, or these properties that I've singled out as playing constitutive roles (for explanations or conceptual-framework construction). In many cases I suspect there is an overlap, since the basis of the experimental use of a concept is often grounded by its experimental roles, through the relations they construct between experimental set-ups and certain sets of phenomena, and the properties of the concept these rely on. But as a research process evolves of course, just like theorists, experimenters begin to make use of elements of the evolving representations of these concepts to develop these explanatory roles more fruitfully.

2.4 Investigative Roles

Investigative roles are not independent roles but really the amalgam of the way explanatory, conceptual and experimental roles are relied upon for the purpose of investigating some aspect of the phenomena relevant to a research process. A research process is of necessity an ongoing investigative process, and it relies on its concepts to frame the relevant questions for pursuing that research, and a conceptual structure for doing so. This is what an 'investigative role' is meant to reflect. The entity concept provides resources and constraints for an investigation. It imagines that there is an aspect of the phenomena or complexity to the phenomena that is uncertain or complex which requires some conceptual basis for its experimental, explanatory or conceptual elaboration and development. As such an investigative role helps provide a language of conceptualization and interpretation for it, and experimentation on it. We say in this respect that the entity concept help ‘frame’ the investigation of an aspect of the world.

The way in which other epistemic roles provide an entity concept with investigative functions for a research process is not too hard to see. An open-ended explanatory role for instance, raises the question of the explanatory mechanism at operation, in which case the concept sets up a framework for the structured investigation of that mechanism, and in respect of beliefs about the entity. The cause of continental drift was looked for by virtue of the assumption that continents were in fact moving, and in respect of assumptions attributed to the continents in order for them to play their explanatory roles. Both the gene and neutron concept exemplify such investigative roles for pursuing explanatory mechanisms. As such the concept is relied on to generate an investigative program.

As experimental tools, part of the function of an entity concept can be investigative, where the goal is for instance to probe a phenomena and investigate its properties. These roles then of course combine when experimentation is used to probe a causal-mechanism. There is no reason why conceptual roles aren’t themselves in some sense investigative, being used to explore theoretical possibilities and raise research questions. In a sense whenever we talk of the concept being used as tool to structure research into a particular phenomena we say it is playing an investigative role, whether through its explanatory, conceptual or experimental roles. Elaboration of these roles as well see is tied in generally closely to investigative aspect of the use an entity concept. It drives and structures the investigative process so to speak. The research questions in respect of a given theoretical entity then are what investigative roles it has performed for an ongoing process of research, and how have they operated through the combination of their explanatory, conceptual and experimental roles.

> <u>Investigative roles</u>: concept A plays an investigative role for research process R, when it is relied upon as means for the investigation of phenomena relevant to R. What constitute ‘means’ is defined open-endedly, but includes essentially providing resources and constraints that sustain that the theoretical and experimental elaboration of the phenomena, through providing a resource for the conceptualization of these and the starting point for their consideration and interpretation, generating in the process research questions and issues associated with its operation in this respect, and by

providing resources for their experimental examination. We express this by saying that the entity concept helped 'frame' the investigation of these phenomena.

As we'll see the gene concept played a strong investigative role in the examination of genetic material, and the phenomena of heredity. The neutron framed the approach to nuclear structure and nuclear forces.

Having said this it's important no entity concept acts in isolation in this regard and is part of a broad network of concepts that contribute to these investigations and provide the structured basis for it. Nor are all investigative roles that an entity concept might play necessarily so central to that investigation. But what's interesting historically is the general coincidence of these two, and the respect to which research processes have been reliant on their entity concepts for framing further investigations of some aspect of the world, particularly unobservable aspects.

2.5. Methodological Openness

In the last chapter we raised the observation, made variously by a number of philosophers over the years in different contexts, that there appears to be an 'openness' or 'open-endedness' regarding the use of theoretical terms, or in Hacking's case, the representations of experimental entities. I gave this schematic classification of what I considered how this open-endedness has historically manifested itself in the case of theoretical entity concepts.

i) The concept is acknowledged to lack certain aspects of description, like a knowledge of the inner structure or cause of the underlying entity, property etc, or of its kind categorization. This doesn't necessarily undermine its usefulness nor belief that it corresponds to an underlying reality and refers.

ii) The concept is treated as open to the addition of more statements regarding it and not pinned to the known statements i.e. is treated as inexhaustible and always open to further investigation.

iii) There is a disposition on the part of scientists in the context of research to change their minds about descriptive elements without abandoning the concepts and thus not to affix the concept purely to its descriptive elements.

What I want to suggest here is that these aspects of openness should themselves be seen to have played an epistemic function, and should in this respect be considered part of the methodology of the use of such concepts historically. While I won't talk about this in terms of meaning, as it is not necessary to a consideration of these methodological points, it is clear that some aspects of the openness of concepts pertains to the meaning of the concept, and thus imply something of an openness, partiality or vagueness of meaning in the representation of the concept. The logical positivists and their opponents thus had a point, but of course the logical positivists criterion of meaning misses the fact that this openness stemmed not from 'lack of observability' but from a willingness to keep the meaning of concepts in certain circumstances open on account of such things as their lack of observability.

So the suggestion here is that firstly there is an historical openness to the way causal agent concepts have been used which manifests itself in different ways, and secondly that this can be understood as methodological and part of the epistemic value of such concepts. Historically it is perhaps one of the most significant features of the way theoretical entity concepts have been used which will form an important part of our account of the case studies in the following two chapters.

These ideas about the historical open-endedness of theoretical entity concepts in the context of research tie in with this framework of epistemic roles we have constructed. For instance causal-explanatory mechanisms have been frequently incomplete and required further investigation, or as we'll see ideas about these causal mechanisms have been subject to change with different interpretations and descriptions, without the abandonment of the role. This is the starting point for realizing that such roles are to an extent constitutive of the use of the entity concept, just as they constitutive of the justification of belief in the entity. Likewise the inner structures of the entity's themselves need not have been part necessarily of the original supposition of it but left open. Nor do they seem always to have been necessary to the use of a concept, which is Hacking's claim in the context of experimentation.

In fact in many respects the historical experience is that these kinds of questions have themselves been the subject of investigation and an issue for the research process. It will be extremely important for my claims about referential practices and conceptual continuity to note that for the ongoing processes of research for which these entities play epistemic roles, inner structure, causal-mechanisms, and in general the properties and behaviours of the entity, are or become questions of investigation for that research, but that these questions are in fact framed by these epistemic roles. Research into the entity itself is historically carried out through the agency of the roles it plays with respect to that research, as the development of the concept becomes relevant to achieving the epistemic goals of that research. But solving those goals still takes place through the agency of these roles, and as such this investigation manifests often as usually an elaboration of them. As will see it's an extremely important observation of the way practice works with respect to entity concepts and their fundamental embeddedness in research processes, that the gene concept was developed with respect to its causal-explanatory relation to traits, and its conceptual role as a unit of heredity. There was no *free* investigation of the gene (an idea which makes little sense for a theoretical concept). It was structured through its contributing roles towards the goal of understanding heredity and development. In this respect the concept might be said to play an investigative role in the investigation of its underlying entity, which is far from tautological of course, but actually quite intuitive. Theoretical entities aren't investigated free of any kinds of assumptions about them. My claim will be that epistemic roles are formative in this respect since they provide the basis for this investigation (see chapters 5 and 6). However the point of giving this sneak-preview if you like is to point to a relation between ongoing processes of research, the open-endedness of theoretical entity concepts historically, and the epistemic roles they have played for this research. As an object of investigation the description of the entity is subject to change and development, the assignment of new properties and behaviours so on, in order to achieve these research goals. Its open-endedness and development can be accounted for by its position in the context of a process of ongoing research.

This treatment of theoretical entity concepts is however internal to methodological practice with respect to those concepts. These concepts aren't conceived simply to make an ontological claim about the world, they're conceived because in same way they aid a research process in its endeavours. At the same time such entities are by definition hidden or less accessible. There is often simply no justification for theories that at the outset could describe all their aspects, or detail precisely its causal explanatory mechanism. Experiments may need to be done, more observations taken etc. It may still be reasonable for a scientist to introduce a theoretical entity as an explanatory concept without having this kind of theory or information. And it may be accepted as such by a group of scientific opinion, on the weight of background principles that help determine what a good explanation counts as and abductive inference. It may be posed in fact hypothetically, leaving its reality a question for its future success or otherwise. But where complex and uncertain phenomena are involved, the wisdom and necessity of open-ended concepts is a methodological caution that informs the way scientific attitudes respond and accept descriptive development and change as part of the use of that concept. And here the context of an ongoing research process becomes evident, because it's in this context, at the research frontier, that the entity concept through its framework of epistemic roles, helps provide resources and constraints for that research, but allows it to develop in a flexible way that responds to ever-arising phenomena and data, including the development of the entity's representation itself. In other words, methodologically researchers have tried to use theoretical entity concepts to frame their research of phenomena for the purpose of their further investigation of it towards reaching their research goals, acknowledging in the process the uncertainty of their understanding, the incompleteness of their approach and the general need for more scientific work. As such through its epistemic roles these concepts are a means for their own further investigation. When the dispositions of researchers are taken into account, and the way in which they have responded historically to conceptual change, I think we get the basic picture that methodologically scientists move from outwards, inwards. Theoretical entity concepts are used to provide a conceptual basis to doing just that!

2.6 Theoretical entity concepts as epistemic frames of research: an historical picture

As such I think there is a general way to describe what theoretical entities have historically contributed to practice, but also a way of re-conceptualising precisely what concepts are in the context of ongoing research. I won't however provide any strong evidence for this here. The case studies that follow will show I hope the usefulness of such a way of describing theoretical entity concepts which provides a general historical understanding of them, which draws together these aspects we've already discussed even if it doesn't in fact tell us much new over what I've already postulated above.

What we've done so far is give content to the notion of an 'epistemics of unobservable causal agent entities' for the purpose of structuring historical accounts of the roles of theoretical entity concepts in ongoing processes of research, for which narratives of historical developments of such research can be framed in terms of these contributions. This tells us that such entity concepts in the context of ongoing research processes have not just been passive additions to the ontology of the world but have acted as resources and constraints for scientific practice. I think these elements are captured by describing theoretical entity concepts historically as *epistemic frames of research and investigation* for the scientists using them relative to particular processes of research. This idea packs together two significant elements. Firstly, theoretical entity concepts have acted as bases for research, not simply the subject of it, providing through their epistemic roles a concept that can be referred to or relied upon, for pursuing the goals of a process of research, through the construction of theory, explanation and experimentation, and by helping frame paths of investigation. Secondly however these concepts have played these roles to varying extents open-endedly, not through thorough descriptions or representations. In this way the word ‘frame’ reflects their loose structuring of some aspect of the world when the scientific methodology of developing and relying on the concept historically is considered, and we factor in the dispositions of scientists in the context of research to change their understanding or descriptions of the entity they’re dealing with, and the underlying importance of a concept’s epistemic roles as part of the basis for doing this. In this sense these epistemic roles and constitutive properties provide a framework the use of the concept and the development of it, in the face of complex phenomena and uncertainty. The notion of ‘epistemic frame of reference’ aims to capture these various uses and approaches to such concepts in historical practice, for theoretical entity concepts.

My use of the word ‘frame’ as a description of a concept is not new of course. Computational philosophy has developed theories of concepts around this idea.[99] However my use of ‘frame’ here is not coextensive with theirs, which refers more to the how a concept is applied as a representation rather than how that concept broadly considered mediates our understanding of and response to the world, and in the process helps generate representations. But ‘an epistemic frame of reference’ is a position on how to understand what we should take such ‘concepts’ to be in these contexts. That is, it asserts that concepts are not just containers of descriptions or representative elements, but also devices defined and pinned down by roles that provide scientists a kind of basis from which to construct such descriptions and representations. In the context of research, when their descriptive variability is acknowledged, this ‘frame’ element becomes the central historical and methodological way of understanding what these concepts are in the context of ongoing processes of research. In

[99] M. Minsky, ‘A framework for representing knowledge.’, in P.H. Winston (Ed.), *The psychology of Computer Vision* (New York, McGraw-Hill, 1975), pp211-277.

other words, it gives us a more historically useful conception that I believe reflects the role of theoretical entity concepts in the context of ongoing processes of research as part of the structure of that research, not simply an end product or output of it. In fact describing such concepts simply as ring-fenced containers gives us very little insight into how these concepts are sustained in research processes, and as I'll assert in chapter 5 and 6, simply generates philosophical problems about the meanings of these concepts that are not part of the ordinary experience or concern of researchers, serving to mystify rather what is actually going on. Theoretical entity concepts have not just been abstract containers of information, but also abstract producers and processors of it, and this the key to an historical treatment of them.

2.7 Conclusion

Whether the concept of 'epistemic frame' has historical relevance or not, what I've given here is at least the beginnings of what we could call an *epistemics* of theoretical entity concepts; that is, a description of the kinds of historical epistemic values and uses that such concepts have displayed. While I've talked in quite general terms my ambitions here were to establish the kind of criterion that have value as a starting point with which to further analyse the epistemic roles of particular historical examples, and with which we can talk generally about a specific class of concepts in science. With this in hand we can broaden our perspective on the role of such concepts in scientific practice, beyond the narrow consideration of them as simply representing 'discoveries' or information about the world. This is vital to a better and more accurate appreciation of their constructive and active value in the business of investigating, theory-making and experimentation. The next task then is show how the concept of an epistemic frame and more generally the analysis in terms of these epistemic values is a useful reference point for dealing in depth with particular historical cases, and in particular in accounting for their role and persistence in historical episodes of scientific practice.

Chapter 3: Case Study – The Mendelian Gene (1900- 1953) and Open-Concept Realism

In the last chapter I tried to set up a framework for understanding the historical role of theoretical entity concepts in practice as *epistemic frames of research and investigation*. This means to say that in epistemic terms such entity concepts provide both resources and constraints for specific processes of research that derive from their associated epistemic functions and values for this research. This is a claim which depends on an examination of the way such concepts have been used historically in scientific practice, and it is the function of the case studies in the next two chapters to show just how this practice has operated in at least these historical instances and that we can examine and reconstruct the contribution of these entity concepts to this research in these terms, and gain a perspective on how to describe the concept itself in this specific context of ongoing research. Of course as I've already suggested the framework I've set up of epistemic roles is meant as a guide to this historical work, and not a substitute for it. These roles provide a basis for giving a narrative of historical developments with respect to a certain concept, by tracing its participation in these, and epistemic value for them, at the frontier of research. But the way in which these roles were expressed and operated was unique to each concept, and is constructible only from the particular circumstances and events of this process of research. The goals remains however to form a perspective on how to understand the concept and its part in this process, the methodology of its use, and its effect on the research choices made.

As such my approach to these two case studies however is not simply to try to force them both into the same schema; it's to develop my discussion in a way that raises the interesting philosophical and historical aspects relevant to each, and that make each case instructive for this more epistemically attuned approach. For the gene concept particularly, it's the realist attitudes and their association with the use of it as an investigative tool that tells us something about the way realism emerges in scientific practice and is relied upon, that doesn't necessarily fit with classical philosophical expressions of it. The neutron concept is on the other hand an excellent illustration of the way in which a research process and theoretical entity concept could be dynamically intertwined through these epistemic roles, such that the development of the concept itself was managed completely within the context of elaborating its epistemic roles for the purpose of furthering that research. Trying to treat the concept distinctly from this research process and its epistemic roles is thus very hard thing to do for any historical analysis.

On to the gene concept: as noted already a specific benefit of this of this 'epistemic frame' claim, and the one I will focus on in this chapter, is that it offers a more promising account of the historical use of theoretical entity concepts in scientific practice. It is implicit, and sometimes explicitly expressed by scientists, that such entity concepts offer investigative, explanatory, experimental, and conceptual functions that sustain the concept and provide the basis for its use. It allows us conceive why such terms are often employed in open and flexible ways, and why certain details of the concept's representation or description such as its inner structure, or its precise causal mechanism, often don't prove essential to their persistence in science. Scientists can be fundamentally wrong about these, yet will retain the concept. This doesn't make scientists necessarily pragmatists. As I will argue in this chapter they have maintained in various instances an 'open realism' that the concept corresponds to an element in reality nonetheless. But by conceiving of such entities epistemically as 'frames' of research more weight is placed on the epistemic values of a concept, as a tool of investigation – not just an object of it - and less weight on hypotheses of what the entity is, how it behaves, or how it precisely brings about the phenomena attributed to it. These things in fact it provides a basis for the very investigation of. Indeed a concept that is open and flexible in this respect is a concept that is useful, in fact necessary perhaps, when dealing with the contingencies of complex and epistemically inaccessible systems.

The Mendelian or classical gene, I think gives one of best examples of an entity employed epistemically to frame research and investigation for which there is somewhat explicit acknowledgment from the scientists involved of its role in this regard. But more than this the classical gene's history displays how scientists may balance the open-endedness of a concept with its epistemic values to generate a concept flexible in its application, yet with enough structure to provide a basis for the research into those open or unknown aspects. In this situation the value of understanding an entity epistemically as an 'epistemic frame' comes strongly to the fore in an account of historical episodes of scientific practice.

My basic goals with this case study are to:

1. Show how we can spell out the historical use of a theoretical entity concept in these epistemic terms, and give a history of the concept's development with respect to them. This means elaborating the concept's role up until about 1953 in providing resources

and constraints for theoretical and experimental research into hereditary and developmental phenomena.

2. Show that these epistemic roles both permitted and took advantage of conceptual openness or open-endedness in the early gene concept that stemmed from uncertainties in the causal mechanism and structure of the concept, and a methodological willingness to preserve by virtue of these epistemic values the concept despite this lack of knowledge. In fact methodologically the gene concept acted as a starting point for investigation, to give scientists some theoretical and experimental grip on hereditary and developmental phenomena, for which the gene itself was part of these objects of investigation framed by the very properties and roles imputed to.
3. I will add to this story however another dimension by suggesting how these uses, particularly the investigative ones, were rationalised on the part of scientists who supported the gene in the form of what can be called an 'open-realist attitude'. This is important to understanding how scientists generally, but not always, might sustain the uses of their theoretical entity concepts in these ways.

3.1 Introduction

The 'gene' is for many philosophers and historians one of the most vexing concepts in modern science. Many have heralded its death since the 1980's, as no longer meaningful, 'bewildering', beset by multiple conceptions and reconceptions, only to see it carry on in scientific practice. The history of failed definitions of the gene is extensive, and yet scientists still go on searching for a definition.[100] To others it is plaintive that there is no longer one gene but a plethora of different functional and structural concepts used by different scientific communities in different ways. There is little or no continuity between these concepts and the older original ones that existed before the 1960's. On the other hand the term is still used by scientists in an often unilateral way as though it had some general meaning, even if there seem to be different types and categories of them. The 'gene' is in some respects as alive and as epistemically functional (even if its biological functions seem to be diverse) as it ever was, being the catalyst for new definitions and new categorisations, and investigations. There is something of a core belief which is still prominent among many geneticists and molecular

[100] See for instance. Gerstein MB, Bruce C, Rozowsky JS, Zheng D, Du J, Korbel JO, Emanuelsson O, Zhang ZD, Weissman S, Snyder M (2007). "What is a gene, post-ENCODE? History and updated definition". *Genome Research* 17 (6): 669-681

biologists that there is some unit of heredity, and within this framework a variety of different points of view and definitions of what it is drives practice in diverse fields.

Much of this ‘aliveness’, if I might call it that, was present in the concept from the outset, in the form of constant redefinition, description and re-description of its causal connection to phenomena and inner structure, as well as constant new applications of it in experimental investigation and causal explanation. My focus here is not on the modern debate, except to point out that the current uncertainty of the concept is not new but has a long history, which tells us something strong and interesting about the way unobservable causal agent concepts have been used in scientific practice. My attention is thus on the historical gene before the 1950s and Watson and Crick’s elaboration of the structure of DNA and especially the early period up to the 30s.

The relevant research process then wherein we assess the roles and uses of the gene concept aimed at an understanding of the bases of heredity (the transmission of characteristics) and development (the production of them), which were for geneticists particularly with the advent of the gene concept, closely associated issues. The way I describe then these historical events can be connected within this general framework of epistemic goals. Of course historians might have some concern over this imputation of a link between understanding ‘heredity’ and understanding ‘development’, where Mendelian genetics seemed of itself simply concerned with the former. This was certainly part of the way in which geneticists explained what the gene concept referred to, and what it achieved.[101] But from the outset it’s clear amongst geneticists that the two were deeply interconnected, and the biochemical analysis of developmental processes would be part of the understanding the processes of heredity and vice-versa. To quote Bateson in 1907, “The study of heredity and variation...is itself a purely physiological inquiry, and as such it must range itself among other physiological inquiries; standing next beside, and looking constantly for support to, physiological chemistry.”[102] The gene concept was applied in other contexts, for other purposes, such as for instance delineating species, and understanding the processes of evolution. As well see however there’s a direct continuity between the use of the gene to study and account for heredity, and its use in the study of development, to the extent that the gene concept of the former was the

[101] See for instance the classic textbook Thomas Morgan; Alfred H. Sturtevant, H. J. Muller and C. B. Bridges. *The Mechanism of Mendelian Heredity*. (New York, Henry Holt, 1915), where they are keen to set the development question aside, and to have the Mendelian principles considered on the basis of their success as a causal explanation of hereditary phenomena.
[102] W. Bateson, ‘Facts limiting the theory of heredity’, *Science* (1907) 26(672), pp.649-660; p. 650.

basis for the development of it in the context of the later. Researchers like Bateson understood early that Mendelian mechanisms generated questions that required a deeper research into the basis of those mechanisms to understand and comprehend what was happening in heredity, which became more apparent as the investigation of such phenomena threw up more and more complexities. In any case on a broader level researchers understood that understanding heredity and development were necessary roads towards a more fine-tuned account of exactly how it was that organisms came to reproduced in the particular ways they are, which was the context in which the gene concept was employed and elaborated through a more or less continuous process of research and investigation to lower and lower levels of operation.

With this in mind, the first section of this chapter will focus on the conceptualisation of the gene concept as a theoretical entity concept, and show how it was treated by its proponents as an open-ended concept being both uncertain in structure and causal mechanism. Many prominent geneticists nonetheless maintained a realist belief in the concept. In this situation I think we have to define a more specific category of realism which we can refer to as 'open-concept realism'. This type of realism is I think of broad use in the description of scientific attitudes with other unobservable entities other than the gene concept, and signifies a rationale for the use of such concepts when approaching the unobservable world. The second examines the evolving epistemic values of the concept to geneticists and the use of the concept as an epistemic frame. Indeed what proved perhaps the most useful aspect of it was in fact its openness and flexibility. Geneticists and others combined basic epistemic features of the concept, such as its explanatory value with its openness in structure and mechanism to make the concept a starting point for framing their investigation and research into hereditary phenomena, including into the biochemical basis of living systems. They placed the gene concept and its causal-explanatory relations at the centre of how they thought about and pursued their research, developing their ideas and experiments in respect of it. There was a method to this madness. The openness of the gene concept was balanced by and complemented its epistemic use as an epistemic frame. The gene set-up an epistemic structure that was used as a basis upon which to guide investigation and theory-making; a basic skeletal view of the causes of hereditary phenomena, which didn't prejudice the outcome or overreach a genuine lack of knowledge. But it did provide a necessary frame of investigation, particularly into gametic material, and elements for theory construction. This was vital in the context of a phenomenon for which the causes were evidently so uncertain and complex, and criticism of speculation so quick. As such the gene was the necessary methodological principle at the heart of the discipline of genetics, and its ambition of understanding development and the causes of heredity. It was central to and drove this process of research.

3.2 *Invisible Elements Called Genes*: Introducing the Gene Concept

Not everybody in the first half of the 20th century treated genes or factors the same way. There was a diversity of opinion of what to make of them and whether they were even necessary. But a strong and dominant current was at least weakly realistic, and took them seriously as terms that had a correspondence in reality with a degree of opinion over how much more they were willing to infer. Some such as Morgan and Muller felt they had reason to characterise this element as a particulate chemical unit, with a precise spatial location. Others such as Bateson, Johannsen and East refused to do so, preferring to speak in analogical terms only about the relations between genes and molecules, keeping it completely open what the gene was. Yet all these respective opinions coalesced broadly I believe around the notion of a gene as an unknown and invisible thing-like causal agent with particular central properties including its unit nature, the relative constancy of the gene, its property of multiplying itself, its segregation in germ cells and importantly its causal relationship to observable characters.

In this part I want to explore the material and causal assumptions that constituted the early or classical gene concept as it was formulated by prominent geneticists in favour of it. This is more or less a direct study of how the gene was visualised by these scientists, where they agreed and disagreed. What are particularly important to highlight in this section are three things. The first is the openness of the material basis of the concept, and the willingness of geneticists to work with a concept on its epistemic values alone without necessarily needing any theory about what it was they were dealing with. Likewise this extends to the causal mechanism of the concept. The exact causal pathway by which the gene operated in causing partially or otherwise hereditary characters were in many ways as uncertain. The second point to make is in fact that above all the other properties geneticists relied upon to justify this concept was its causal-explanatory agency which relied itself on fairly basic assumptions about the gene concept. Thirdly, acknowledging this shouldn't be taken as a sign of the instrumentalism of many of these geneticists. I think this interpretation as it is given by Falk in this instance is wrong.[103] There needs to be more subtlety in describing the attitudes of many of the geneticists in this situation. Many were willing to use a concept with a large

[103] Raphael Falk, 'What is a Gene?', *Studies in the History and Philosophy of Science*, Vol. 17, No. 2 (1986), pp. 133-173.

amount of uncertainty and openness but still commit to its existence or a correspondence to a loose degree.

3.2.1 The Gene Concept - Simply an *Etwas*?

One important aspect of the classical gene which makes explicit the more complex attitudes scientists have had towards theoretical entities which serve particular valuable epistemic functions was their willingness to use and justify the concept on the basis of its epistemic roles alone, without being able to describe what the concept actually represented in nature or how it operated beyond some loose causal-explanatory relations. Geneticists and cytologists argued that we can keep distinct the epistemic value of the gene concept, the practical use of it and its representation of something unknown in reality, from any real understanding of what it is.

This attitude took time to develop but when it had it was forcefully argued for by many geneticists that supported the concept. The term gene (*gen* in German) was first coined by Johannsen in 1909 who was keen to keep distinct the Mendelian particulate conception of heredity and the earlier particulate ideas of Weismann and Darwin among others. Other terms terms with similar application existed at the same time, were 'factor', 'allelomorph', 'unit character'[104] and gametic 'element'.

The history of the gene entity is of course well-known. The character or *Merkmal* was introduced by Mendel in his famous 1866 paper.[105] It was done to explain certain patterns he had noticed in the inheritance of certain traits in pea plants, which were the ratios that these traits possessed with respect to others in breeding experiments. He supposed these could be explained by the fact that certain laws of segregation and independence held between characters, which themselves possessed powers of dominance and recessiveness. It is plausible to argue that Mendel was instrumentalist about his theory, treating his *A*'s and *a*'s as simply part of a formalism for evaluating hereditary relationships and making experimental

[104] The term 'unit character' presented conceptual problems, tied as it was in Bateson's work to phenotypic characters thus obscuring the causal relations of gene and trait.
[105] Gregor Mendel, *Versuche über Plflanzenhybriden* Verhandlungen des naturforschenden Vereines in Brünn, Bd. IV für das Jahr, 1865 *Abhandlungen* (1866) pp 3-47. For the English translation, see: Druery, C.T and William Bateson, "Experiments in plant hybridization". *Journal of the Royal Horticultural Society* ***26*** (1901), pp *1-32* and reproduced in W. Bateson, *Mendel's Principles of Heredity* (London, Cambridge University Press, 1902), pp40-95.

predictions. Certainly Mendel doesn't say much about how we are supposed to take these. But when he does say something relevant it is more or less a clue as to a deeper correspondence between these *Merkmale* and gametic material.

"The law of combination of different characters which governs the development of hybrids finds therefore its foundation and explanation in the principle enunciated, that the hybrids produce eggs cells and pollen cells which in equal numbers represent all constant forms which results from the combinations of characters brought together in fertilisation."[106]

It is clear from this that gametic material was considered the causal material responsible for the laws. Later he refers to the basis of these laws as "founded on the material composition and arrangement of the elements [*elemente*] which meet in the cell in vivifying union."[107] These elements could be meant generally, as elements of cells, or meant as causal elements for characters. At times his use of the term 'character' slips between traits and causal element, or is at least ambiguous between the two. For example the egg and pollen cells of peas, "represent in equal numbers all constant forms which results from the combination of the characters when united in fertilisation."[108] In this sentence forms have taken the place of traits and characters some causal element of the gametes.

Whatever Mendel thought, when Mendel's work was popularised by Bateson in the very early years of the 20th century, after being lost for 34 years, he inherited the ambiguity in the use of the term 'character' with only a relatively clearer sense that at least sometimes it referred to the hypothesis of an underlying particulate causal agent, i.e. that these Mendelian elements *A* and *a* mapped to something in the gametes.[109] Certainly he imagined there was a correspondence with some gametic material. Like Mendel, Bateson believed that the gametes solely carried the causes of character, in the sense that the gametes were 'bearers' of it, of which their germinal correspondences maintained their independence according to the laws of segregation. Hence the combination *Aa* did not represent a blended inseparable combination, but the presence in the gametes of an *A* and an *a* which would be separated during meiosis and transmitted independently. At the same time he was keen in the introduction to the 1902 text to emphasise the lack of knowledge we have of this correspondence and how unwise it

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[106] Gregor Mendel, "Experiments in plant hybridization" in W. Bateson, *Mendel's Principles of Heredity* (London, Cambridge University Press, 1902), p76.
[107] *Ibid,* p88.
[108] *Ibid,* p73.
[109] W. Bateson, *Mendel's Principles of Heredity,* 1st Ed. (London, Cambridge University Press, 1902), p24.

would be to speculate about it; "…. of the nature of the physical basis of heredity we have no conception at all".[110] Thus Bateson left the correspondence relation between unit-characters and gametes completely open, although it is clear implicitly that he was relying on its existence. Nonetheless there is however little sense in which a particulate causal agent in germinal material for Bateson lay behind characters.

By 1913 and 1914 however Bateson's language had altered to the point of view that definite particulate causal agents were the cause of characters, which was necessary to his presence-absence theory[111]. As he put it "the essence of Mendelism lies in the discovery of the existence of unit-characters or factors," which "essentially determine and cause the development of" attributes of living organisms.[112] However we could only conjecture as to whether it was a material substance, or phenomenon of arrangement. But whatever it was it had to be of such a nature "that in cell-divisions of gametogenesis it can be treated as a unit."[113] Yet while it is clear that he held that these factors were particulate and shared something of a shared causal role, Bateson took the view that we couldn't limit these factors to objects united by a common nature if we wanted to account for their causal function. For partially these reasons Bateson rejected the hypothesis of Sutton that genes lie on chromosomes, which, for Bateson seemed too limited a genetic material and an incautious assumption lacking sufficient evidence.

Bateson vacillated over how to understand this concept, yet his use of it revealed that there were two currents in his thinking. First, there was his insistence on pointing out that the material basis or bases of the factor was open and unknown, perhaps far beyond our means of understanding, and second that the concept was nonetheless a functional concept for hereditary investigation, the explanation of hereditary phenomena and the solution of problems. "…we have now the means of beginning an analysis of living organisms and distinguishing many of the units or factors which essentially determine and cause the development of their several attributes."[114] The lack of understanding of factors nor the causal mechanisms by which they operate shouldn't according to Bateson distract from the use of them or from their usefulness. This is a viewpoint that describes many of the

[110] *Ibid*, p3.
[111] The presence-absence theory took a causal agent to explain dominance, and the lack of one, or the default position, to explain recessiveness.
[112] William Bateson, *Problems of Genetics* (London, Cambridge University Press, 1914), p3.
[113] W. Bateson, *Mendel's Principles of Heredity,* 3rd Ed. (London, Cambridge University Press, 1913), p268.
[114] *Ibid*, p.2.

perspectives taken by the geneticists that ultimately won sway in the field with respect to the gene concept in its early years, perhaps up until the late 1950s.

For Johannsen (1911) the gene concept was a history of explanatory success. "The astonishing evolution of this mode of research has given an almost interminable stock of special results and cases that at first might seem incompatible with the Mendelian views have been analysed more thoroughly on a large scale and have shown themselves in accord with Mendelism."[115] Johannsen was referring to such problems as Bateson's fowl hybrid comb example and the problem of blended inheritance, for which Mendelism proved adaptable. While it is clear that Johannsen treated the gene as the explanatory factor for hereditary phenomena he too emphasised the unknown nature of it. "As to the nature of the genes it is as yet of no value to propose any hypothesis; but that the notion of 'gene' covers a reality is evident from Mendelism." In other words similarly to Bateson, a gene concept was justifiable but the nature of to what it referred was open and unknown.[116] In the *Elemente* of 1909 Johannsen referred to the gene as an *etwas.*[117] This was also how Bateson had referred to it. Johannsen thought there was a corresponding 'something' but was unprepared to speculate on its nature. Yet such was no bar to the employment of the concept which had proved its explanatory and predictive success. Johannsen instead of postulating an inner nature, preferred instead to talk in strictly metaphorical terms of chemical structures which might be capable of reproducing the properties of genes.

East in 1912 dedicated an article to the gene concept. Again it proceeded on the basis that the 'gene', or 'factor' in this case, was a concept of immense scientific value and as such was justified even if nothing more could be said of it than it was a 'scheme that aids in describing an inheritance…'[118] Again however, in the sense of a loose or open correspondence as Bateson and Johannsen, he exclaims, "though it must have a basis in reality if it is to describe a series of genetics facts." He likens the situation to a chemical molecule distinguishing carefully their application from knowledge of what they are or their inner structure. "The term factor represents in a way a biological reality of whose nature we are ignorant just as a structural molecular formula represents fundamentally a reality, yet both as they are used

[115] William Johannsen, 'The Genotype Conception of Heredity', *The American Naturalist*, Vol. 45, No. 531 (1911), pp. 129-159, p133.
[116] *Ibid*, p133.
[117] William Johannsen, *Elemente der exakten Erblichkeitslehre* (Gustav Fischer, Jena., 1909), p123-124.
[118] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), pp. 633-655; p632.

mathematically are concepts."[119] Like Bateson, for East a lack of knowledge of the inner structure of the genetic factor was unimportant and secondary to its application.

There were various reasons why these geneticists opposed speculation about the nature or correspondence of the gene or factor concept. It was quite significant to all of them that they prevent the premature association of the gene concept with those particulate assumptions of Darwin, Weismann, Nägeli, Naudin and De Lage, which were regarded as speculative and arbitrary. Geneticists wanted their idea considered on its own merits and apart from these, and so naturally directed attention to what could be done with the concept and away from its supposed similarity with them, at the same time asking the reader to accept that underlying this successful application of the concept was a reality. At other times however it was simply a genuine lack of evidence and genuine uncertainty. There was simply not enough known about germinal tissue and biochemicals to make any kind of strong presupposition without crossing into speculation. As such geneticists worked in the space between speculation and the inferences that followed successful use of a particulate causal agent concept by limiting those inferences. But they were also as we will see in later sections, cautiously setting-up and justifying something of an epistemic frame to guide their own further investigation of the material basis of heredity which left open the nature of the gene but took the concept nonetheless as a essential starting point to doing this.

The justification for a belief in a reality underlying the gene concept for those that were willing to accept it stemmed from abductive inferences geneticists made implicitly and explicitly from the explanatory success of the concept, which was also used upon to distinguish genetic theory from earlier particulate theories. Morgan wrote in 1926 that, "In the same sense in which the chemist postulates invisible atoms and the physicist electrons, the student of heredity appeals to invisible elements called genes."[120] By this Morgan meant that the existence of such elements and the justification for their postulation followed as an explanation from the data and the relations between traits in the same way it had for Mendel. Morgan accepted in the context of the extra evidence available to him that the discrete characters of traits and the laws they obeyed could be extended down to an unobservable independent causal unit. "…the independent reappearance in later generations of the characters of two individuals combined in a cross can be explained by the theory of

[119] *Ibid*, p634.
[120] T.H. Morgan, *The Theory of the Gene* (New Haven, Yale University Press, 1926), p1.

independent units in the germinal material."[121] Morgan in this textbook (*The Theory of the Gene*) reconstructs the evidence and basic use of the gene concept without any reference to chromosomes, which by this time had been imputed to be the bearers of genes, and this included its use in understanding the phenomenon of linkage.[122] In this case the point was undoubtedly to show how successful the gene concept was in organising and explaining hereditary phenomena. Like the others he was keen to separate the assertion of an underlying basis for it from any speculation as to what it was. The explanatory success of the gene concept in the instances he describes, including linkage, "tell us no more than that something derived from one parent separates, in the germ-cells of the hybrid, from something brought in by the other parent."[123] As he put it in an earlier paper specifically directed at critics of the gene concept the simple fact of the behaviour of traits during a multiple cross would be enough to conclude that the germ plasma "must therefore be made up of independent elements of some kind."[124]

This aspect of the use of the gene concept, largely without understanding or clear conception of what it represented, was thus justified by its success in causal-explanation and application to new problems. The result of this was a dependence of the gene concept on the causal relation between gene and trait: its principal explanatory role. It was in virtue of this that the gene possessed 'causal agency' and we will see that it provided a flexibility to the concept to deal with new phenomenon. But given its obvious importance to the explanatory function of the concept, it was important to assert that relation with clarity. Morgan pointed out himself the importance of the causal law like process with which gene and trait were connected calling it a 'fundamental assumption'.[125] Johannsen earlier had made more or less the same demand that this be clearly understood, and used his pure line experiments to back this up. It was the essence of his 'genotype conception' of heredity that the causal relation was from genotype to phenotype and not vice versa. The explanatory value and methodological advantages of the gene concept rested on this. Castle however postulated that trait changes could affect genes, in which respect the causal-explanatory value of the gene concept became much more difficult to perceive.[126] This is precisely on the basis that Muller and East

[121] *Ibid,* p26.
[122] The fact that certain groups of genes tended to be inherited together, and didn't independently assort.
[123] *Ibid,* p26.
[124] T.H. Morgan, 'The Theory of the Gene', *The American Naturalist*, Vol. 51, No. 609 (1917), pp. 513-544; p515.
[125] *Ibid,* p27.
[126] W.E. Castle, 'The Inconstancy of Unit-Characters', *The American Naturalist*, Vol. 48 (1914), pp. 352-362.

responded. Non-constant factors undermined the epistemic advantages of the concept.[127] East, who was selling the gene concept in terms of its epistemic functionality in particular made perhaps the strongest association between this functionality and the gene concept, declaring that 'one cannot measure or describe by changing standards.'[128] For Muller it denied the basis of success upon which the assertion of a factor could even be made.

At the same time as the gene concept was open-ended, undefined, and uncertain in terms of its inner structure, so was its causal-explanatory mechanism. Morgan's textbook makes no postulation in that regard. "The theory of the gene, as formulated here, states nothing with respect to the way in which genes are connected with the end-product or character."[129] It was as much a source of openness in the understanding of concept as it was a source of criticism. As Morgan puts it, "Again the theory has been unfairly criticised on the grounds that the organism is a physico-chemical mechanism, while the genetic theory fails to account for the mechanism that is involved."[130] As he points out though there is nothing in the basic physical properties ascribed to the gene concept that breaches any known possible chemical or physical process. As such the concept should stand or fall by its explanatory value in dealing with hereditary problems not developmental ones; "the fact remains that the sorting out of the characters in successive generations can be explained at present without reference to the way in which the gene affects the developmental process."[131] This was in fact the basis of Morgan's response to those that criticised the gene concept for simply ascribing properties to the gene concept that actually needed, according to critics, to be explained in first place! "It has been said….that the assumption of invisible units in the germ-materials really explains nothing, since to these are ascribed the very properties of genetics with those of development."[132] But as in the electron case and Lewis' objection (see chapter 2), the gene's explanatory value came from other sources, namely the explanation of character transmission through its unit structure and self-replication, and as such there existed a basis upon which such developmental properties and other powers could be assigned. In fact Morgan distinguishes the gene from earlier particulate conceptions of heredity on the basis that they lacked this very link to Mendelian laws of transmission which the gene had, and thus were

[127] H.J. Muller, 'The Bearing of the Selection Experiments of Castle and Philips on the Variability of Genes,' *The American Naturalist*, Vol. 48 (1914), pp. 567-576.
[128] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), p646.
[129] T.H. Morgan, *The Theory of the Gene* (New Haven, Yale University Press, 1926), p26
[130] *Ibid*, p27.
[131] *Ibid*, p26.
[132] *Ibid*, p27.

simply arbitrary assignments of properties without ulterior justification. As such the theory of the gene could be justified "without attempting to explain the nature of the causal processes that connect gene and character."[133]

In the end then scientists in favour of the gene concept had to walk a balance between open-endedness, innovative research framework and speculation. But the success of the concept in explanatory and problem solving tasks justified their commitment to it, and belief in its correspondence to a reality as uncertain as that was. Historically, when Morgan and the *Dr osophilia* geneticists demonstrated how linkage relations between genes could themselves be explained by positions on a chromosome and the mechanism of crossing-over it was not only good further evidence for the gene, but it gave more structure to the concept, positioning it, and confining it to a certain chemical substances.[134] Beyond that however the gene still remained a largely uncertain and open issue. The question remained how these substances worked, what precisely composed them, how the gene was identified and demarcated within.

3.2.2 Open-Concept Realism

The attitude by geneticists which I have described above towards the gene concept is a particularly interesting and important one, because it questions assumptions we might have about how scientists operate with unobservable entities. Certain philosophers have grappled with how to understand and define the attitude attending such uses where there is a conceptual uncertainty, lack of definition and open-endedness of description. Often the simple category of 'instrumentalism' has been applied too quickly. Falk, who is chief among philosophers and historians for raising the modern complexities of the gene concept, refers to it as an 'instrumental reductionism' on Johannsen's, Morgan's and other geneticists' part, and considers their use of the concept to be framed by an 'as if' attitude towards it. In other words they cut themselves off from the reality question and proceeded 'as if' the gene were real in a truly hypothetical fashion.[135] Both categorisations of 'instrumental' and 'reductionism' are I think inaccurate in this instance. 'Instrumental' here is misleading because the geneticists that took this point of view were realists even though they couldn't or wouldn't say too much

[133] *Ibid*, p27.
[134] Morgan, Thomas Hunt; Alfred H. Sturtevant, H. J. Muller and C. B. Bridges. ***The Mechanism of Mendelian Heredity***. (New York, Henry Holt, 1915).
[135] Raphael Falk, 'What is a Gene?', *Studies in the History and Philosophy of Science*, Vol. 17, No. 2 (1986), pp 133-173. See also Lindley Darden on Bateson in 'William Bateson and the Promise of Mendelism', *Journal of the History of Biology*, Vol.10, No. 1 (1977), pp.87-106.

about what they were realist, preferring to leave the question open. They made abductive inferences from the various explanatory and problem solving successes of the gene concept to an underlying yet unobservable corresponding reality, which was at least some kind of causal agent in the germ material with a set of properties necessary for that explanation. There was certainly a definite commitment to the concept over and above its usefulness. Part of the promise of the gene concept was the frame it provided for investigation and part of this was an expectation of how the germinal material was structured as causal units and thus a basis upon which to investigate it and ultimately find out more about genes. The gene concept was used in this way and it was motivated by a more or less a realist attitude towards it. I think in fact with the early gene concept it is easy to confuse a lack of concern with the uncertainty of the nature of the 'factor', with an instrumental attitude towards it. Morgan manipulated genes as material units, not 'as if' they were material units, even though he couldn't identify their structure or composition.

Reductionism was certainly a goal of the scientists that developed the gene concept. Morgan thought that "the fundamental problem in the study of heredity" was the investigation of the germ structure itself.[136] But it is a little misleading to apply it too in this context for a few reasons. First reductionism was neither a priority nor even possible at the outset, and certainly not necessary to the use of the concept, for many geneticists. Johannsen for instance thought the chromosome interpretation, lacking evidence as it did for him, 'idle', and preferred to put the structure question to one side. East, Morgan, Bateson, all thought in their various ways that nothing really could be said about this. Secondly there is an implication in Falk that as such the term was thought to be impermanent to be eventually reduced to something else and rendered superfluous, when, except for Bateson perhaps (though not consistently), there was a commitment to the term describing a thing, something identifiable and distinguishable in the germinal material, with enough commonality that such talk of a 'something' was justified. Much of the structure that made the concept explanatory in the first place, was assumed to be reproducible in any material reduction, i.e. in this case, its unity, causal agency, separability and independence, which would maintain the concept as a useful one, even if it was manifested in different ways. Third, the lack of a reduced basis was just one aspect of the unknown or uncertain aspects of the 'thing' at hand which also included lack of an understood

[136] T.H. Morgan, 'Factors and Unit Characters in Mendelian Heredity', *The American Naturalist*, Vol. 47, No. 553 (1913), pp. 5-16

causal mechanism for how its effects were achieved. To those that opposed the gene concept these were also problematic.

I think that a better phrase for these kinds of attitudes displayed by scientists in the case of the gene would be *open-concept realism*, where what scientists are realist about is open, meaning it is partial and uncertain in the context of what was known and expected at the time, with nonetheless a commitment to some kind of correspondence between that term and reality. As will be apparent from what I say below, open-concept realism can be seen as a rationale or an appropriate epistemic attitude for the concept to be used in the ways it was given its uncertainty of description.

3.3 The Gene Concept as an Epistemic Frame

The historical claim of this approach is that historical episodes of scientific practice can be structured with respect to the epistemic values (explanatory, experimental, conceptual, investigative) of their chosen concepts, and understood through the roles they have played as resources and constraints. Certain concepts have been more important than others in this respect and theoretical entity concepts have tended to play central epistemic roles, framing strongly the research and investigative choices scientists have made, and framing the understanding of scientists of their phenomena through the conceptual frameworks for which they are essentially involved. The part played by the classical gene concept and development of the field of genetics can be historically traced in these terms. From the outset it was understood that the gene, its laws and basic properties provided a framework to explain and make predictions about the transmission of discrete characters. We might remember doing as much with Punnett squares in high school. But there was more to the way the gene concept was used than this. It was treated as a broad framework for the understanding of the transmission and development process over a much wider ground than just the classical Mendelian phenomena. Its collection of loose causal relations and vague causal agents, possessed enough detail through their assumed unit structure, stability and self-reproducibility to provide structure and a methodology, tools and resources, to provide a base for future theoretical and experimental development, or ongoing research, into what were the complex phenomena of heredity and development. The gene concept both framed problems and provided resources for solutions, it both suggested research directions and framed the approach to them. The concept was at the centre of the early history of genetics, and directed and motivated the path it took.

Richard B. Goldschmidt in the 1950s looking back on the history of genetics described the history provocatively but accurately I think when he pictured the gene and the basic properties constitutive of its roles for genetic research as something of a 'mind set'. "Saturated as we are with the clear, unassailable facts of Mendelian inheritance, we are conditioned to thinking in terms of discrete units which can be shifted and recombined like dice in a throw, never losing their identity in a kind of splendid isolation."[137] Goldschmidt was of course critical of the gene concept and its application, and wanted it replaced by his own field concept of genetic material interaction and causation. But what he had perceived was the extent to which the gene framed a way of understanding heredity with certain patterns of thought dependent on basic features of the gene concept, in this case its discreteness. Goldschmidt thus argued that what was required was not only a 'different terminology' but a 'different mental attitude.' "In this new way of looking at fundamentals, the atomistic view of collaborating independent units is replaced by a concept of unified action of a field type which results in a hierarchical order of not strictly delimited fields."[138]

While Goldschmidt may have been talking with a touch of rhetorical flourish, at the beginning the gene concept and Mendelian laws were understood to constitute a method of approach to the problems of heredity. Bateson in 1902 pronounced, "we have at last a brilliant method, and solid basis from which to attack the problems of heredity."[139] As Bateson presented the situation, the aim was to find the physical basis, the inward and essential nature, "the causes…of heredity", but we remained in a condition of knowing "absolutely nothing". But by application of Mendel's principles "we are enabled to reach and deal in a comprehensive manner with phenomena of a fundamental nature, lying at the very root of all conceptions, not merely of the physiology of reproduction and heredity, but even of the essential nature of living organisms."[140] Bateson's 1902 text is full of statements that express the methodological value of the 'unit-character' concept and Mendel's principles, and its function in grappling the great unknowns of heredity and beyond. The method was powerful enough to convince

[137] Richard B. Goldschmidt, *Theoretical Genetics* (Berkeley and Los Angeles, University of California Press, 1958), p95.
[138] *Ibid*, p191.
[139] W. Bateson, *Mendel's Principles of Heredity,* 1st Ed. (London, Cambridge University Press, 1902), p x.
[140] *Ibid*, p35.

Bateson that biology now had something rivalling physics and chemistry; quantitative and exact.

The selling of the gene concept and Mendelian principles in this way as a methodology and theoretical framework was pursued extensively right through the classical gene period. More cautiously Johannsen proclaimed that the genotype conception does not "pretend to give a true or full 'explanation' of hereditary, but may be regarded only as an implement…..But as yet it seems the most prosperous idea."[141] In a way that emphasised however the visual metaphor that such a shift provided a new vision of the world or new way of seeing it Bateson pronounced,

"With the recognition of unit-characters our general conceptions of the structure and properties of living things inevitably undergoes a change. We begin to perceive outlines where previously all was vague, nor can we doubt those outlines will very soon become clearer."[142]

This I think captures the feeling of many geneticists and later cytologists and biochemists that the gene promised some kind of conceptual basis not only to grapple with phenomena which had up till that point been largely unfathomable, but also to see deeper into it. There is the sense contained here that the gene was a starting point for investigation and understanding. This was certainly his sympathy when he expressed in a later text that, "we now have the means of beginning an analysis of living organisms and distinguishing many of the units or factors which essentially cause the development of their attributes."[143] For Bateson the gene concept promised a method and framework for deriving the causes and processes at work in heredity and development.

For Thomas Morgan and his group, the gene concept provided the essential conceptual basis for the analysis of gametic material. This was as we'll explore below, one of its chief investigative roles, which was pursued through the conceptual, explanatory and experimental roles of the genes, and its constitutive properties, particularly its unit nature and self-

[141] William Johannsen, 'The Genotype Conception of Heredity', *The American Naturalist*, Vol. 45, No. 531 (1911), pp. 129-159
[142] W. Bateson, *Mendel's Principles of Heredity,* 3rd Ed. (London, Cambridge University Press, 1913)
[143] W. Bateson, *Problems of Genetics* (London, Cambridge University Press, 1914), p2.

replication. The gene in this respect provided a resource for composing chromosomes, and for conceptualising and formalising the interactions of chromosomes.

Even much later the sense of the gene as grounding a basis for research into questions of development and heredity was active. Muller who considered the stability of the gene its critical feature in 1946 considered this to provide a solid basis of research into the ‘bafflingly complex and seemingly erratic character of biological things.’ He refers to it as ‘frame of reference in relation to which the passing phases of other features are adjusted,’ akin to constants in physics, such as the mass-energy relation of pendula. Muller perceived the genotype conception as thus the ‘starting point of investigation’, a ‘controlling structure’ to which all other processes could be referred.

I think this kind of language was not just rhetorical but tells us something deep about the importance of the gene concept to scientific practice during this period. The conceptualisation of the gene by the users of it, relying to greater and less extent on its causal-agency and other epistemic roles with their attendant constitutive properties was a basis for explanation, theory-making, problem-solving, and experimentation in genetics, providing a set of evolving problems and paths of investigation for both producing phenomena as part of a developing process of research, and providing resources for interpreting and understanding it.
What I want to do here then is outline the kinds of roles the gene concept was playing throughout this period and how they themselves formed part of the structure of an ongoing process of research into heredity and development.

3.3.1 The Classical Mendelian Conceptual and Experimental Uses

The most significant role that the gene concept was playing in this respect was conceptual; providing the conceptual basis for the organisation of a quantitative system for working with hereditary phenomena. It is well known of course the Mendelian laws of independent assortment and segregation with their built in factor concept constituted a formal calculus for the explanation and prediction of hereditary phenomenon. This system provided the initial explanation of Mendel’s pea phenomena and geneticists quickly discovered that many characters ‘mendelized’ and could be explained in the same way, with the same dominance/recessive relations.[144] Classical Mendelian research worked by starting with a set of trait phenomena and trying to conceptualise them in terms of a finite set of factors.[145] The quantitative nature of this system impressed many geneticists, and was used to advertise its advantages by, amongst others, Bateson, East and Morgan. East especially, with his wish to distance the formula from its potential correspondence in reality, presents the system as a 'conceptual notation as is used in algebra or in chemistry.'[146] Morgan constructs the theory of the gene axiomatically to establish a formal system of relations between genes such that those principles would "enable us to handle problems of genetics on a strictly numerical basis, and allow us to predict, with a great deal of precision, what will occur in any given situation."[147] Its conceptual role in this respect was expressed particularly through its unit nature and constancy through generations, which allowed such formal systems of greater and greater complexity to be constructed to give better accounts of the actual complexity at work in heredity systems, particularly in respect of the relation between gene and traits. [148] Thomas Morgan for instance in a paper of 1917 adapts the Mendelian system beyond simple allelomorphic pairs and single unit-factor causes for any one trait, to a multi-allelomorphic (allowing for mutations of the same unit-factor) multifactorial system for the purpose of accounting for the recognised complexity of factor-trait relations evident from his *Drosophilia* research including for new mutations without having to revise the system as they arose. Understanding this required that geneticists give up any notion that a trait is the product of one unit-factor acting alone. The conceptual system provided a resource for responding to this development, as it did in the case of incomplete dominance, mutations and the pure line experimentation of Johannsen. Geneticists worked at constructing schemes of factors often quite complex that could reproduce a particular trait phenomenon across generations, with systems of dominant, recessive and mutant genes, and later modifying factors, represented by symbols and organised by at least the law of segregation, where a particular task was to pose relevant factors minimally for a particular trait, and sort out which were allelomorphic. With such a scheme they could claim an understanding of the phenomena, and a reduction of it, with which came the possibility of predictions but also the further analysis of the relations between these factors. As Morgan put it, "In order to study the relation of these characters to

[144] Characters or genes *mendelized* if they segregated according to Mendel's laws.
[145] See R.A. Emerson, 'The Inheritance of a Recurring Somatic Variation in Variegated Ears of Maize', The American Naturalist (1913) 48, pp.87-115. This is a classic case of this kind of conceptualisation.
[146] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), p634.
[147] T.H. Morgan, *The Theory of the Gene* (New Haven, Yale University Press, 1926), p25.
[148] *Ibid*

each it has become necessary to combine many of them and in order to represent the results some system of symbols must be adopted."[149]

Of principal importance to the operation of this conceptual role in the context of the analysis of heredity, was the stability of the 'unit-factor'. Both East and Johansen stressed that its conceptual use in this regard was only possible by virtue of this assumption. For East it was constitutive of the very use of the gene concept, without which it would have no particular organisational value. Arguing in this vein more or less pragmatically, or 'epistemically', "As I hope to show, a factor, not being a biological reality but a descriptive term must be fixed and unchangeable."[150] East has his reasons for making such a statement, that shouldn't necessarily be interpreted anti-realistically as we've seen. More his strategy is to convince of the epistemic value of the gene concept in terms of its epistemic roles, without having justify any idea of what the underlying reality might be. The standard of measurement for him is thus the 'helpfulness' of the Mendelian notation, which he attests to through the success of geneticists in bringing many problems like quantitative characters within its scope. "I do maintain that the Mendelian notation fits the facts of inheritance."[151] As he put it, the definition of a unit-character was vague and difficult to formulate yet in essence the notation operates through description with 'unvarying hypothetical factors' such that if such descriptions fail then 'Mendelian notation fails.' East clearly took this to be constitutive of the conceptual use of the factor concept, perceiving its necessity to the further use of the concept as an aspect of ongoing research for describing by the reactions of fixed and unchanging units. This property in his words, made the concept 'most helpful to investigation', adding that 'one cannot measure or describe by changing standards'[152]. The conceptual role in this respect was clear for East when it came to dealing with the experiments of Castle, which Castle explained with a varying factor, influenced by an organism's phenotype.[153] East pointed to the complexity of such explanations. If one does assume varying units, then one has no real conceptual base from which to work. If one does then it determines how one constructs one's responses to new phenomena. "If under identical conditions the expression is different, then a new standard, a

[149] *Ibid*
[150] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist,* Vol. 46, No. 551 (1912), p634.
[151] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), p639.
[152] *Ibid*,p646.
[153] W.E. Castle, 'The Inconstancy of Unit Characters', *The American Naturalist* (1912) 46, p.352-362.

new unit, must be assumed; that is, factor A by any change becomes factor B."[154] From this starting point external and internal conditions affect the expression of characters, not change them, such that internal changes (changes under the same environmental conditions) could be attributed to the operation of gametic factors that specifically acted to modify the output of others during development. So the stability of the gene concept was relied upon for the hypothesis of the idea of a 'modifying factors' in the course of reseach, which became themselves part of future genetic explanations and conceptualisations.[155] These were the result of relying on the idea that genes were stable, ordinarily non-variable. Variations within this framework thus could be explained by the control of genes on the expression of others, and this seemed the best explanation to East, but not by changing genes themselves, which simply undermined the whole conceptual role of the gene concept.

However the first to really emphasise the importance of 'stability' as methodologically characteristic of genetics was Johannsen, who expressed it as what he called 'the genotype conception of heredity', by which he meant to refer to the fixed body of gametic material such that 'as special genotypic constitution always reacts in the same manner under identical conditions – as all chemical or physical structures do.'[156] But of course under diverse conditions the way in which genotypic differences could express themselves could be substantial; 'Differences in genotypical constitution (as well as differences in chemical or physical nature) are not bound to manifest themselves at all – and still less do in the same sense – under all conditions.'[157] The chemical metaphor was very important to Johannsen in this respect, because it gave a way of elaborating what the stability of the genotype could mean, and how it could be conceived that under different conditions it might produce different outcomes but the same under the same conditions, just like molecules in chemical reactions. In this sense it acted for him as a 'theory-constitutive' metaphor. Johannsen took this stability to be principal lesson underlying the success of his own experiment on pure lines and the consistent expression of the genotype under the same conditions. Methodologically then and conceptually, "the genotype is thus an 'ahistoric' view of the reactions of living beings..."[158] These ideas marked it out as very distinct from what Johannsen called the transmission

[154] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), p648.
[155] See for instance Sewall Wright, 'Color Inheritance in Mammals', *Journal of Heredity*, Vol. 8, No. 5 (1917) pp 224-235.
[156] William Johannsen, 'The Genotype Conception of Heredity', *The American Naturalist*, Vol. 45, No. 531 (1911), pp. 129-159; p.146.
[157] *Ibid*, p.146.
[158] *Ibid*, p.139.

conception, whereby phenotypic features were directly transmitted through genetic material, and differences in phenotype signalled always differences in that material. For such theories genetic material was a fluid structure, undergoing constant fluid variation. As Johannsen argued however, "Discontinuity and constant differences between the 'genes' are the quotidian bread of Mendelianism"[159], which could be measured by its success as a conceptual approach. As such Johannsen took the stability of the gene concept or the genotype as central to the way in which theory was constructed in genetics, and for distinguishing Mendelianism and his own pure line theory from older theories of heredity. It was in this sense constitutive of the Mendelian approach.

As such as simple units provided a space of investigation themselves, through which linkage relations between genes were identified, conceptualised and then investigated in the same unit terms by Bateson and Punnett.[160] Groups of genes did not assort independently, but tended to segregate in groups. Within these groups there was a certain tendency to split in smaller groups. This led to Morgan's linear model of genes that modelled the possibility of splitting within a group by organising genes spatially along a line 'like beads on a string', such that the closer together genes were the less likely they were to split.[161] This was all evaluated by careful experimental study expressed in the language of genes, or more appropriately, character combination frequencies. But it was meant by Morgan to reflect a reality about genes (not simply an organisation of the data) and as such relied on those basic properties of the gene concept: causal agency (association to characters), segregation and moreover the sense that as discrete units they could be picked up abstractly and moved about, shifting their grouping relations to other genes, which was interpreted to tell us something about the actual spatial organisation of the gene. "The frequency of crossing-over furnishes evidence of the linear order of the elements in each linkage group and of the relative position of the elements with respect to each other".[162] Thus the unit conception of the gene provided resources for the investigation and conceptualisation of hereditary relationships, including investigation into its own relations. For Goldschmidt no doubt this was a perfect example of geneticists 'conditioned to thinking in terms of discrete units.' Such developments and uses of the Mendelian system relied on this conceptual role of the gene concept in this respect.

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[159] *Ibid*,p.147.
[160] W. Bateson and R. C. Punnett, 'On the Inter-Relations of Genetic Factors', *Proceedings of the Royal Society of London. Series B, Containing Papers of a Biological Character*, Vol. 84, No. 568 (1911), pp. 3-8.
[161] T.H. Morgan, *The Theory of the Gene* (New Haven, Yale University Press, 1926), pp22-24.
[162] *Ibid,* p25.

In addition the gene concept was tasked with important experimental roles, for the production of new hereditary phenomena and the testing of Mendelian principles. Experimentally early on in the history of genetics, the gene concept was used especially to determine relevant experiments and to build relations between factors and traits. A presumed factor basis for particular traits could be used to construct experiments exploring their interaction with other factors, their linkages for instance, or their affects on each other's causal output. The gene concept acted as an experimental tool for exploring the phenomena of linkage (discovered in 1905 by Bateson and Punnett)[163], leading to the discovery of sex linkage in 1906 (by Doncaster and Raynor)[164]. This role was certainly distinct from that of explaining or conceptualising the patterns in hereditary phenomena. It started with an assumed genetic structure and a set of genes for particular traits, used them as a reference point through their presumed stability for producing new phenomena in different genetic environments through successions of crosses. Experiments were set-up to follow them through generations given different crosses involving different genotypes and phenotypes and build up information about their effects and relations to other genes and traits. While this experimental use produced information relevant for the Mendelian system and constructing schemas for traits, as we'll see the advent of mutations greatly enhanced the use of genes experimentally as a means of examining genetic material and ultimately the composition of genes themselves, which can be described as a distinct role.

These roles then combined to provide geneticists the means for pursuing hereditary phenomena, which turned out to be enormously successful, in accounting for the new phenomena produced. To quote Johannsen, "The astonishing evolution of this mode of research has given an almost interminable stock of special results, and cases that at first might seem incompatible with the Mendelian views have been analysed more thoroughly on a large scale and have shown themselves quite in accordance with Mendelism."[165] Johannsen was referring to cases such as the problem of blended inheritance and quantitive characteristics, solved by Nilsson-Ehle and East. More complex cases like the species *Oenothera* were to follow, and were brought within the Mendelian system. Nonetheless Johannsen was well aware of the centrality of these conceptual roles and their constituent properties of the

[163] Bateson, W., *et al*. Experimental studies in the physiology of heredity. *Reports to the Evolution Committee of the Royal Society* (1905) 2; pp. 1–55, 80–99.
[164] L Doncaster and GH Raynor, 'On breeding experiments with Lepidoptera', *Proc. Zool. Soc. London* (1906) 1, pp. 125-133.
[165] William Johannsen, 'The Genotype Conception of Heredity', *The American Naturalist*, Vol. 45, No. 531 (1911), pp. 129-159; p.147.

Mendelian concept to driving the research process, as the very basis of investigation, but as a starting point or a means for it, not as an end of it.

"The genotype-conception here advocated does not pretend to give a true or full explanation of heredity, but may be regarded only as an implement for further critical research, an implement that in its turn may be proved to be insufficient, unilateral and even erroneous, as all working-hypotheses may some time show themselves to be. But as yet it seems to be the most prosperous leading idea in genetics."[166]

3.3.2 Investigative Role: The Material Bases of Heredity and Development

The investigation of the material bases of heredity and development relied essentially on experimental, explanatory and conceptual roles of the gene. The conceptual role as a stable unit of heredity and self-replication was certainly an important aspect of this role, providing a conceptual basis for the investigation of the material bases of heredity and development, which itself became recognised as essential to the goals of understanding these phenomena. As Morgan put it, "….the more characters that are obtained that show association in inheritance the further we may hope to go in our analysis of the constitution of the germ-plasma, which is admittedly the fundamental problem in the study of heredity."(16)

Let me mention some historical cases in this respect that also serve to show the continuity of this role. In the first instance Morgan's group relied on the gene concept to structure interpret and understand the phenomena of linkage, through plotting gene relations and percentage chances of co-occurrence, which led to the hypothesis that the chromosome contained or was composed of a linear arrangement of genes, as we've already mentioned. This move itself relied on the possession of the gene concept (and its unit nature) in the first place, but then provided researchers with a way of tracking cross-over points (through mutations), and the further complexities of chromosomal interaction such as multiple recombination. Investigation of the chromosome structure and material was thus to some extent governed by the gene hypothesis, experimentally and conceptually. Researchers at the time understood the role of the Mendelian approach to framing their understanding of chromosomes, by maintaining the distinction between them. The 'genotype conception' was not reliant on the chromosome hypothesis and stood on its own feet, whereby the actual biological entities

[166] *Ibid,* p.159.

responsible for the operation of genes could remain unknown and uncertain, while the study of hereditary phenomena in terms of factors proceeded. Even Morgan, Sturtevant, Muller and Bridges make this point in their central text promoting their account of the mechanism of Mendelian heredity in terms of chromosomes..."there is no result dealt with in the following pages that may not be treated independently of the chromosome; for, we have made no assumption concerning heredity that cannot also be made abstractly without the chromosomes as bearers of the postulated hereditary factors."[167] Bateson and Johannsen famously were both sceptical of the chromosome hypothesis, taking the chromosomes to be too homogenous a material to possibly account for the complexity of gene properties as a natural kinds (sharing some commonality) yet possessed of a huge variety of causal outputs, but they persisted nonetheless with a belief in their correspondence of the gene concept with a reality, and importantly with setting up a basis for thinking about what germinal material might be, and what to look for. Johannsen for instance liked the idea of a gene as a complicated organic molecule possessed of side-chains to account for its variety, a main chain for its stability, although he preferred to keep such idea metaphorical.[168]

For those that did favour the chromosome hypothesis, the gene concept played a distinct conceptual and experimental role in its justification and investigation. It was on the basis of this genetic division that scientists framed the position effect, as a relation between genes and their immediate neighbours on the same chromosome. Genes were used as experimental tools to test this effect, and the effect and position of modifying factors.[169] By such methods the functional organisation of chromosomal material was assembled, always relying however on the gene concept. Even though Goldschmidt was to question whether really the operation of germinal material and the chromosome should be understood in terms of genes, the concept nonetheless was language and tool of explanation and experimentation of chromosomal operation and structure.

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[167] T.H. Morgan, A.H. Sturtevant, H.J. Muller, C.B. Bridges, *The Mechanism of Mendelian Heredity* (New York, Henry Holt, 1915), p.viii.

[168] William Johannsen, 'The Genotype Conception of Heredity', *The American Naturalist*, Vol. 45, No. 531 (1911), pp. 129-159; p.143.

[169] See J.S. Dexter "The analysis of a case of continuous variation in *Drosophilia* by a study of its linkage relations," *The American Naturalist*, Vol. 48, No. 576 (1914), pp. 712-758; C.B. Bridges, "Non-disjunction as proof of the chromosome theory of heredity," *Genetics* 1 (2) (1916), pp. 107; C.B. Bridges, "Specific modifiers of eosin eye colour in *Drosophilia Melanogaster*," Journal of Experimental Zoology 28(3) (1919), pp. 337-384.

3.3.2.1 Investigating the gene itself!

As research progressed however, the gene concept continued to act as a conceptual basis for the investigation of the biochemical causes of development. But investigation was also heavily reliant on the causal-explanatory role of the gene concept, linking genes and traits. Gametic material was approached on the basis that a causal unit was operating and interacting with cellular elements around it.

In this respect, particular through its explanatory role (connecting germinal material with traits), the gene concept was relied upon to investigate germinal material chiefly through the investigation of the gene's own structure. This was just as structured an investigation, and depended upon what was thought of the entity itself. As expressed already many geneticists were happy to leave this question open and refrained from speculating about the gene, as to whether for instance it was a particle or not, because of the bad association with Weismannian particulate theory and others. Further it was felt that the process was likely to be extremely complicated given the vast range of effects that such an entity would need to produce. This made it uncertain whether to expect a causal agent of little variation in properties and structure or quite broad variation. Bateson postulated that it must be capable of operating by different causal mechanisms and producing quite different chemicals.[170] Earlier Bateson had expressed doubt over whether the gene was a material substance or a 'phenomenon of arrangement.' Morgan in 1926 could only say that the gene might be a molecule or it might "only be a collection of organic matter not held together in chemical combination."[171] This prefigured an obviously difficult issue. Geneticists had very little to work with in the gene concept, but they did have a set of expectations based on the properties afforded the gene concept by virtue of its Mendelian explanations (its unit nature and self-replication), and the causal-explanatory link between gene and trait. This proved an essential basis to structure and orientate their investigation and theory-making about the gene. The situation was helped by the chromosome hypothesis because it brought with it a spatial location for the gene and a material basis for it. But the 'chemical attack on the nature of the gene' as Muller called it relied nonetheless on these aspects of gene concept itself and on its epistemic roles, which provided resources for it.[172]

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170 William Bateson, *Problems of Genetics* (London, Cambridge University Press, 1914), p86.
171 T.H. Morgan, *The Theory of the Gene* (New Haven, Yale University Press, 1926), p310.
172 H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), pp. 1-37.

Muller was one of the chief participants in this. The constitutive properties of its conceptual role in Mendelian genetics, its unit nature and self-replication, plus its causal relation to traits, gave him basic information about what was to be looked for and how any chemical model of the gene needed to function. By 1922, the year of a particular paper on this exact subject, the causal chain between gene and character had been refined, such that genes were associated with enzymes, and controlling functions such as determining whether agglutinin exists in the blood, or whether homogentisic acid is secreted into urine.[173] But the variable actions of genes meant that the gene could not be variously any of these things; an enzyme, homogentistic acid, or in fact 'any-related substance'. As he put it "Each of the effects, which we call a 'character' of the organism is the product of a highly complex, intricate, and delicately balanced system of reactions caused by the interaction of countless genes…"[174] It would be thus absurd to think that the gene could share much in common with one of its effects. Muller relied on the causal relation between gene and trait for this piece of reasoning.

Much later, as biochemical knowledge improved the property that was used to investigate the gene concept itself, and which Muller and others relied on, was its ability to reproduce itself. Like stability, this was one of the most basic assumptions of the gene idea; an idea basic to the explanation of heredity through the production gametes, serving not just a descriptive but also epistemic role. For Muller the ability of the gene to take in material around it and generate new copies, gave him properties and abilities that needed explanation, and thus a starting point for composing a chemical model. As early as a paper of 1922 he had in fact declared that, "The most distinctive characteristic of these ultra-microscopic particles – that characteristic whereby we identify it as a gene – is its property of self-propagation."[175] Moreover the fact that a gene could be altered through mutation yet still continue to produce an effect and Mendelize in the same way, gave critical information about what a gene could be. Other assumptions would have to be involved of course, the chemical would have to have a certain position in the nucleus, it would have to have separable and identifiable causal powers. But Muller felt this was the most discernible and remarkable property for identifying the agent involved. "The fact that genes have this autocatalytic power is in itself sufficiently striking, for they are undoubtedly complex substances, and it is difficult to understand by

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[173] H. J. Muller, 'Variation Due to Change in the Individual Gene', *The American Naturalist*, Vol. 56, No. 642 (1922), pp. 32-50.
[174] *Ibid*, p33.
[175] *Ibid*, p33.

what strange coincidence of chemistry a gene can happen to have just this very special serious of physico-chemical effects upon its surroundings which productive...just this particular one, which is identical which its own complex structure."[176] As such the general principle of gene construction that permitted this autocatalysis, was 'the most fundamental question of genetics'. Right through his work Muller took this to be the fundamental property of the gene for the purpose of its chemical study.[177] In 1953 Watson and Crick justified their model of DNA by its ability to "carry out the essential operation required of genetic material, that of exact self-duplication."[178] Their model did not quite follow from the X-ray data but relied on a well-informed guess as to what could, given the evidential constraints, produce that effect.

Others such as Emerson, took the variability of gene products, and the complexity of their causal control of character, to be the guide.[179] Emerson worked on the basis that genes had a common chemical constitution and thus his method was to sort out materials and mechanisms that might reproduce such variability.

While this property of a self-propagating unit acted as an investigative base for measuring plausible candidates by, the most important ground for directly obtaining information about the gene structure was the causal relation between gene and its effects – in other words its causal-explanatory role. The chief manner by which this was exploited to study the structure and properties of the gene was via X-ray mutation. Mutations provided ways of varying the causal output of a gene, without destroying the gene itself and thus testing its effects. Muller saw this methodology as a common one in science. "…it was through study of molecular changes that molecules were analysed into atoms tied together in definite ways, and it has been finally the rather recent finding of changes in atoms and investigation of the resulting pieces, that led us to present analysis of atomic structure into positive and negative electrons having characteristic arrangements."[180] In effect mutation could be used to extract more information on the structure of the gene. The fact that mutations often didn't seem to affect the self-reproductive capacity or the causal ability of the gene to produce an effect was just

[176] *Ibid*, p34.
[177] Nothing had changed 20 years later. In a paper of 1946 he chose the particulate self-reproducing element of the chromosome as his chief identifying property for a gene. H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), pp. 5,11.
[178] J.D. Watson and F.H.C Crick, 'The genetical implications of the structure of deoxyribonucleic acid,' *Nature* Vol. 171, No. 4361 (1953), pp 963-967.
[179] S. Emerson, 'Genetics as a tool for studying gene structure,' Annals of the Missouri Botanical Garden 32 (1945), pp243-249
[180] H. J. Muller, 'Variation Due to Change in the Individual Gene', *The American Naturalist*, Vol. 56, No. 642 (1922), p 39.

such information. The ability for mutations to reverse gave the information that a change was thus not always a loss of material from the gene and thus the chemical agents involved needed to have some kind of ability to switch states. In each case the established causal relations of the gene provided the resource for investigation and theorising about it.

In a similar way the hypothesised relation between gene and enzyme, as its primary causal effect, prompted biochemists to try to model a structure that could reproduce enzymes. Delbrück, Gulick, Emerson and others worked in this direction.[181] As Emerson put it, genetics, by which he meant the controlling effects of genes and their hereditary stability, was ‘a tool for studying gene structure’.

In fact the supposed causal agency of the gene was the manner by which DNA was later identified. When Muller was writing in 1946 it was still not clear that what of the nucleoprotein substances in the chromosomes were genetic, with many thinking that given its presumed capabilities it must be protein. But this was causally tested by Avery, Macleod and McCarty by removing alternately the protein and DNA elements from disease causing bacterial cells, and seeing if the disease arose nonetheless in their host. In this way, through its causal agency, DNA was isolated.[182] This was the basis and starting point of the research of Watson and Crick.

In addition to the importance of these conceptual and explanatory roles in the investigation of gametic material, particularly of the gene itself, we should add the distinct experimental roles the gene concept was playing in this regard. We’ve mentioned already the use of mutation by Muller, which can be expressed as an experimental manipulation of the gene to produce phenomena that could help enlighten scientists on the nature of the gene. Muller talks of the wide-spread desire for instance of biologists ‘to gain some measure control over the hereditary changes within the genes’[183] for research or investigative purposes, in the hope that with this ability, ‘the problems of composition and behaviour of the gene, can shortly be

[181] M. Delbrück, ‘A theory of autocatalytic synthesis of polypeptides and its application to the problem of chromosome reproduction’, Cold Spring Harbour Symposia, *Quantitative Biology* 9 (1941), pp 122-126; A. Gulick, ‘A chemical formulation of gene structure and gene action,’ Advanced Enzymology 4 (1944), pp 1-39; S. Emerson, ‘Genetics as a tool for studying gene structure,’ Annals of the Missouri Botanical Garden 32 (1945), pp243-249.
[182] O. T. Avery, C. M. MacLeod, and M. McCarty, ‘Studies on the chemical nature of the substance inducing transformation of pneumococcal types,’ *The Journal of Experimental Medicine*, Vol 83 (1944), pp. 89-96.
[183] H.J. Muller, ‘The artificial transmutation of the gene’, Science (1927) 65, pp. 84-87; p. 84.

approached from various new angles and new handles found for their investigation.'[184] In other respects marker genes (genes with identifiable location on chromosomes) were important for mapping chromosomes, and exploring the patterns and mechanism of crossing-over, and then the position effect**.** The gene was manipulated in other ways too for such experimental purposes. Caspari in 1933 for instance, transplanted genes into different environments to test their effects. In this way the property of a gene as a 'reparable mutant' was discovered, whereby mutant allelomorphs functioned in the context of hosts with developed with normal versions.[185] Beadle and Tatum are famous for effecting a profound development of this experimental role of the gene in its own examination by 'reversing the ordinary procedure and, instead of attempting to work out the chemical bases of known genetic characters, to set out to determine if and how genes control known biochemical reactions.' This could be done by mutating particular genes through x-rays so as to prevent the production of a certain essential chemical for the organism and then placing the organism in an environment where this product was nonetheless available and the organism could survive. Manipulating the genes in this way could be relied upon to work out whether single genes were involved in some synthesis of a particular chemical or others.[186] The experimental role of the gene concept in this respect was being elaborated and developed all the time in new directions, but it remained focused on the task of the examination of genes themselves.

In a broader sense cytologists and biochemists such as Muller were working with the gene concept and its mechanism of explanation as they investigated the structure of germinal material. It affected what they took to be significant and important. It provided research goals and directed what they looked for, what kinds of functions they tested for, what counted as problems and what didn't. Thus the gene concept in this sense set-up the conditions of its own investigation, and played a role in guiding it. It provided a basis upon which to interpret and recognise the mechanisms they discovered through experimentation with X-rays, physical and chemical tests. As such there was undoubtedly an epistemic value to a theoretical entity like the gene concept. Without it theory-making about germinal material would have been much less structured and much harder given the essentially unknown nature of it at the time and

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[184] *Ibid*, p. 87.

[185] For instance Beadle and Ephrussi in 1936 transplanted mutant imaginal discs into Drosophilia larvae noting that the mutant colour gene develops the wild colour in such an environment. See A.H. Sturtevant, *A History of Genetics*, first publ. 1965 (republished Cold Spring Harbor Laboratory Press, 2001), p102.

[186] G. W. Beadle and E. L. Tatum, 'Genetic Control of Biochemical Reactions in Neurospora', *Proceedings of the National Academy of Sciences of the United States of America*, Vol. 27, No. 11 (1941), pp. 499-506.

how it worked. And this transcends the question of whether that concept had a correspondence in reality or not.

3.3.3 The Epistemic Value of an Open Concept

As already expressed the gene concept was accepted and maintained as an open-ended concept. The concept lacked complete description either of its structure or mechanism, a fact to be interpreted relative to the expectations of scientists at the time, and there was an accepted tolerance to be willing to reformulate descriptions, interpretations and representations in this regard. It translates to an ability to broaden, enrich, refine or correct the relations and properties of the concept, for epistemic purposes such as providing new explanations and addressing new problems. What sets this openness is a complex relationship between the attitudes and knowledge of scientists using the concept, their expectations, the background theoretical principles involved, and as we will suggest in the next section, a measuring up of the epistemic advantage of the concept to investigation, research and explanation, especially when unobservable elements are involved, against the theoretical implausibility of being unclear of what it is that is being employed or described, or how it functions. All these things are largely contextual and need to be reconstructed from what scientists at the time thought of the concept and how they used it. In many situations however, as for that of the gene, there have been clear epistemic advantages to openness and flexibility itself.

The classical gene concept has proved especially flexible and adaptable, and this flexibility arose from the circumstances of its postulation. Unlike with particle physics, there was a lack of background information in the biological field informing biologists how they were to treat and describe such causal agent concepts, and as such the postulation was made largely in a frame of ignorance and uncertainty. Biochemistry was evolving with genetics, for which the gene itself played its part. Geneticists thus often possessed a fairly open attitude to what was possible, emphasising their lack of knowledge and their willingness to consider all possibilities. What I want to focus on here is how this combined with the relative vagueness and openness of the principles governing the gene concept and its causal relations.

We have already mentioned that the causal relation between genes and traits were treated flexibly, and were refigured in such a way that maintained an explanatory link. The concept was not tied to any one way that link could manifest itself, but was treated as an open problem. Scientists that stuck to one position quickly found that life moved on, as others adjusted to meet explanatory goals and solve problematic cases. Certainly no principle on this became identified strongly enough with the gene concept that it couldn't be removed without removing the gene concept itself. This left open to geneticists, biochemists and cytologists the space to come up with more complex and subtle causal relations so as to account for the steadily more complex phenomena that the gene was being applied to and what the experimental results were telling them. Johannsen for instance took his pure line experiments to show the partial and variable nature of the relation. Different environmental conditions would express the genes in different ways. But they did assert the stability of the gene; the same environmental conditions would lead the genes to produce the same effects. As such the causal relation between genotype and phenotype moved in one direction only and was variable according to environmental circumstances. Further, where it was initially assumed by many that the relation between gene and trait was to one-to-one, it was quite quickly asserted that this was simply implausible given the complexity of traits.[187] The gene was a causal factor among potentially many responsible for a trait. This didn't however sink Mendelism. The explanation of Mendel's laws didn't require the postulation that factors were sole causes of the characters with which they were associated. Bateson's presence-absence hypothesis, that dominant characters were caused by a unit and absences were the default condition by lack of one, was criticised on this basis.[188] Once the causal meaning of the Mendelian factor had been cleared up in this way it facilitated an explanation. Gene mutation confirmed it.[189] Different mutations could affect the same character and these mutations seemed to mendelize in the same way as genes.

As it happened though this causal relation was to go through many many further developments. It prompted an expectation and search for intermediate agents. Research with colour inheritance by Sewall Wright prompted the link between genes and enzymes.[190] Further mutations in genes seemed to render enzymes responsible for certain traits to cease functioning, indicating a genetic cause. The method of mutations allowed Beadle and Tatum

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[187] The *Drosophilia* experiments contributed to this conclusion. T.H. Morgan, 'The Theory of the Gene', *The American Naturalist*, Vol. 51, No. 609 (1917), pp. 513-544, p515.
[188] Morgan showed in 1913 that the presence-absence hypothesis failed to account for situations where characters appeared to have been lost only to reappear later. T.H. Morgan, 'Factors and Unit Characters in Mendelian Heredity', *The American Naturalist*, Vol. 47, No. 553 (1913), pp. 5-16
[189] R. A. Emerson, 'The Inheritance of a Recurring Somatic Variation in Variegated Ears of Maize', *The American Naturalist*, Vol. 48, No. 566 (1914), pp. 87-115.
[190] Sewall Wright, 'Color Inheritance in Mammals', *Journal of Heredity*, Vol. 8, No. 5 (1917) pp 224-235

to provide a convincing method associating genes and enzymes, and to identify the causal functions of genes.[191] All this work took as starting point and guide, the causal agency of the gene and the connection of gene to trait. The theory of gene action developed as such that, "the development and functioning of an organism consist essentially of an integrated system of chemical reactions controlled in some manner by genes."[192]

At the same time the gene concept provided a relatively free device or 'open slate' upon which to suppose new causal powers when needed. Sewall Wright took advantage first hand the epistemic value of such an open-concept by employing all sorts of postulated gene functions together with Mendelian principles in order to provide a factor system that causally explained colour inheritance in mammals.[193] Among his postulations were factors which affected distribution and intensity of colour, factors which affected distribution of colour in contrast to white, factors which affected distribution and intensity of differentiation from yellow to black and so on. There was a freedom in creating explanations for such phenomena to assign causal powers in complicated and novel ways, which geneticists were willing to take conceptual advantage of. Bridges in 1916 and 1919 posed the possibility of modifying factors that influenced how other factors were expressed by modifying the mutant eye colour eosin.[194] With this Bridges was able to erect his own complex explanation for eye colour phenomena in *Drosophilia*.

As such the system was broadened to allow more than one trait to cause a character, which prompted the resolution of more complex character relations, notably multiply allelomorphic traits. Morgan provides a good example of just how the methodology could be used in that way. Morgan sorts out the simplest system of factors that could account for eye-colour phenomena amongst *Drosophilia*, displaying his own reasoning process, through the postulation of new recessive and dominant factors.

I think what these examples show is the degree of flexibility the gene concept possessed and the relative openness with which it was able to be reformulated in many respects and

[191] G. W. Beadle and E. L. Tatum, 'Genetic Control of Biochemical Reactions in Neurospora', *Proceedings of the National Academy of Sciences of the United States of America*, Vol. 27, No. 11 (1941), pp. 499-506.
[192] *Ibid*, p499.
[193] Sewall Wright, 'Color Inheritance in Mammals', *Journal of Heredity*, Vol. 8, No. 5 (1917) pp 224-235.
[194] C.B. Bridges, "Non-disjunction as proof of the chromosome theory of heredity," *Genetics* 1 (2) (1916), pp. 107; C.B. Bridges, "Specific modifiers of eosin eye colour in *Drosophilia Melanogaster*," Journal of Experimental Zoology 28(3) (1919), pp. 337-384.

expanded in terms of its properties and relations, while at the same time providing a basis or epistemic frame for those developments.

3.4 Framing its own investigation! The Methodology of the Gene Concept

We have raised then three key historical features of the early or classical gene concept above in the context of research into heredity and development. It's open-endedness, the open-realist attitude towards it by many scientists who worked with it, and its role as an epistemic frame of investigation, governed by certain conceptual, explanatory and experimental roles which were composed and relied upon in order to further this research. What I want to claim in this last part is that there was a methodological purpose to their combination in the case of the gene concept which stemmed from the uncertainty and lack of knowledge about the gene itself and the phenomena being dealt with. As such the gene promises I think to tell us something basic about the way in which theoretical entity concepts have combined these various elements in order to grant scientists a methodology suitable to dealing with aspects of the world for which they lack epistemic access and knowledge. Such suggests itself an historical motivation for the presupposition of unobservable entities over and above the simple desire to correctly describe an aspect of the world.

My point here then is that there can be methodological sense to employing an entity concept as an epistemic frame in conjunction with the openness of that concept, and holding a realist attitude towards it. Many geneticists appreciated the concept's epistemic value and believed that the concept provided a starting point of investigation, experiment and theorising. However the situation was characterised by immense complexity, and an acknowledged uncertainty about the mechanisms of heredity. This was reflected again and again in the attitudes of researchers. For instance Muller;

"I have tried to paint things in the rosiest possible colours. Actually, the work on the individual gene, and its mutations, is beset with tremendous difficulty. Such progress in it as has been made by minute steps and at the cost of infinite labour. Where results are thus meagre, all thinking becomes equivalent to speculation.....Let us, then, frankly admit the uncertainty of many of the possibilities we have dealt with as a spur to the real work."[195]

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[195] H. J. Muller, 'Variation Due to Change in the Individual Gene', *The American Naturalist*, Vol. 56, No. 642 (1922), p 49.

Both the gene and its action were recognised to be complex. As Muller described the chain of development in 1922 he recognised that, “Each of these effects, which we call a ‘character’ of the organism, is the product of highly complex, intricate, and delicately balanced system of reactions, caused by the interaction of countless genes,...”[196] In such an environment it was accepted that the gene concept through its roles and their constitutive properties should act as a basic frame and could not be fully described or represented, neither could its mechanism of action. In Muller’s words “the chemical composition of the genes, and the formulae of their reactions remain as yet quite unknown”.[197] As such to be productive it had to be open-ended, and open to speculation and re-interpretation. This is to labour a rather simple point. Its degree of flexibility and open-endedness was an epistemic response to the unclear and complex circumstances. The attitude of open-concept realism towards it reflected the willingness of its users to treat it this way. As such it could be committed to as a basis upon which to proceed in investigation, experimentation and theory-making. The realism of course was not necessary to the use of the concept as East emphasised in solving ordinary hereditary problems. For that it could be treated instrumentally. But as an investigative device for facilitating an investigation of germinal material, an open realist attitude made these actions rational, and certainly describes the attitude the majority of geneticists took with them in their consideration of the causes of heredity and development. For many these ‘causes’ were the essential biological question, and a reason why the gene concept was so valuable a tool in their research even if it escaped definition and therefore lacked any real theory of its structure.

As mentioned before, this realist attitude itself had justification. What sustained it were the epistemic roles of the gene concept, which together comprised the gene concept as an epistemic framework for the investigation of gametic material and hereditary phenomena, and the justification of belief in the gene itself (particularly its explanatory role). The gene concept provided a set of causal-explanatory relations which posed causal but stable self-replicating units within the germinal material, which could then be relied on conceptually and experimental. Conceptual and experimental roles for the gene concept developed as a result. Its formulation as a more or less precise calculus enabled precise predictions and could be used as an experimental tool in the investigation of other theories and phenomena, such as the

[196] *Ibid*, p.33.
[197] *Ibid*, p.32.

theory of species.[198] The commitment to the gene in further theory-making and as a basis upon which to theorise in more depth about the unobservable and unknown causes of heredity relied on the success of the concept in accounting for the observable phenomena. There was certainly no circularity in this.

This extended to applying the concept as an explanation of new phenomena and expanding the concept with new properties, functions and behaviours. As East put it with respect to the Mendelian explanation of discrete characters, it was "inconceivable that this [success] should be the result of coincidence. It was therefore still more inconceivable that only a small portion of the facts in each kingdom should come under the scope of Mendelism",[199] thus licensing the belief that the gene concept should be applied as the basis of investigation and conceptualisation for new hereditary phenomena. In the case of the gene the openness in the original formulation of the concept was supported by the fact that the gene concept provided means through its epistemic roles for developing the concept itself, in terms of its description of the underlying entity, and importantly, of the operation of these roles in more complexity. Developments thus often relied on and referred back to these basic aspects of the gene concept, they were not simply arbitrary assumptions. As we saw Johannsen and Morgan were keen to develop their ideas of the causal-explanatory relation between gene and trait, from a one-to-one relation, which could be attributed to Bateson, and and many-to-many relations which postulated at the same genotypic differences as the factor elements, not Weismannian particles.[200] It provided at the same times an investigative framework, whereby Sewall Wright could talk of the important of tracing back of the action of factors to discover the causal mechanisms of development, to which he could act towards producing a more complete understanding of colour.[201] This role framed their investigation of the underlying gene entity, its properties and behaviour. But it also acted at the same as the basis for belief in the underlying entity through the very success of the Mendelian system accounting for hereditary phenomena in terms of causal stable units. The causal relation between gene and trait for instance was thus cemented in place from the outset, despite the lack of any clear mechanism for it. This was directly relied on for reaching conclusions about the gene itself, and the

[198] See for instance Sydney Cross Harland, 'The Genetical Conception of the Species,' *Biological Reviews* 11 (1) (1935), pp. 83–112.
[199] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), p637.
[200] William Johannsen, 'The Genotype Conception of Heredity', *The American Naturalist*, Vol. 45, No. 531 (1911), pp. 129-159; T.H. Morgan, 'Factors and Unit Characters in Mendelian Heredity', *The American Naturalist*, Vol. 47, No. 553 (1913), pp. 5-16.
[201] Sewall Wright, 'Color Inheritance in Mammals', *Journal of Heredity*, Vol. 8, No. 5 (1917) pp 224-235.

sequences in the causal chain from gene to effect. But it was also open to refinement as long as this basic causal-explanatory link was preserved. As such within this frame the theory of this causal connection was elaborated to account for the complexity of the phenomena being encountered. Similar things were also true of conceptual and experimental roles of the gene, in the context of hereditary phenomena and the material basis of heredity and development. In the Mendelian context researchers elaborated the gene concept within the context of its conceptual role giving genetic bases for complex hereditary phenomena, hypothesising for instance modifying factors. The experimental roles of the gene concept too were continually being itself developed, to find new ways to extract information about the operation of the gene.

As it proved the basic ideas and presumptions about how heredity and development operated through Mendelian laws were not as absolute as thought. In fact the situation was extremely complex. But the explanatory link between genes as causal agents responsible partially at least for the development of traits persisted. Thus when the *cis-trans* mode of identifying genes emerged in the late 50s and 60s as a method of singling out the number of functional units (genes) responsible for a given trait, it relied quintessentially on the causal relation between gene and trait.[202] When Muller began his investigations of the gene, Falk imputes that this was the moment of a fundamental shift by genetics away from the concept of the gene as a unit of function to the gene as a unit of structure, defined in structural terms.[203] While that is no doubt true, the property of the gene Muller took as fundamental, its self-reproductive quality, was relevant because of its part of the original causal-explanation. It allowed Muller to talk in 1946 of the gene as a 'stable controlling factor'.[204] The property of self-reproduction was only interesting and only a plausible way of approaching the gene because of its part in the explanation to which the gene was tied. Remove this, and there was no justification for Muller considering the gene in those terms or in fact investigating germinal material for a unit carrying that property. It was as such constitutive of the critical epistemic value of the gene concept. Units of structure and function aside, the gene was certainly in the classical period at least a unit of causal-explanation, conceptualisation, experimentation and ultimately investigation.

[202] Seymour Benzer, "On the Topography of the Genetic Fine Structure" *Proceedings of the National Academy of Sciences* (1961), 47 (3): 403–415.
[203] Raphael Falk, 'What is a Gene?', *Studies in the History and Philosophy of Science*, Vol. 17, No. 2 (1986), pp 133-173.
[204] H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), pp. 1-37.

The openness and flexibility of application of the concept was thus complemented by and at the same time reliant on the epistemic nature of the gene concept as an epistemic frame. Of course scientists who used the gene concept in this way walked a precarious ground between openness and speculation, and were often criticised for it. There were many especially in the early days that found the gene concept unjustifiable more or less because of its openness and invisibility, which translated as 'speculative' or uncertain, and because its physical mechanism was simply inconceivable. Davis criticised the openness of the concept directly by attacking it because there were no grounds on which to isolate or analyse it.[205] Others according to Morgan doubted the entity because it dealt with 'no known chemical substances'.[206] As noted some simply thought that Mendelian causal-explanation restated the facts, and then 'by juggling with numbers pretends that it has explained something.'[207] When challenged in this way geneticists fell back on explicating patiently the epistemic value of the concept, sometimes de-emphasising the claims made about the structure of the gene concept itself, as East does, and leaving the concept even more open by declaiming against giving it any clarification or definition other than it possessed by virtue of its use; '… one can attain an idea by proper application.'[208] Others such as Morgan relied on the conceptual and explanatory value of the gene concept in different circumstances by showing how a particulate stable unit granted causal agency could account for variability and environmental contingency of characters.[209] In general then its epistemic uses supported its otherwise vague and uncertain description, as a demonstratively reliable means of further investigation, explanation, conceptualisation, and experimentation, indeed a means for extracting and interpreting information about the gene itself.

So scientists through an open-realist attitude could defend the openness of a concept and yet take advantage of it at the same time, through its epistemic uses, gaining methodological flexibility as a result, suited to the particular epistemic situation they were in. As already emphasised we can interpret this in rational terms. When confronted with a situation of relative uncertainty and unobservability practice depends on having a plausible base of

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[205] B.M. Davis, 'Genetical Studies on Oenoethera.III. Further Hybrids of Oenothera biennis and O. grandiflora that Resemble O. Lamarckiana', *The American Naturalist*, Vol. 46, No. 547 (1912), pp. 377-427.
[206] T.H. Morgan, *The Theory of the Gene* (New Haven, Yale University Press, 1926), p27.
[207] *Ibid*, p27.
[208] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), p645.
[209] T.H. Morgan, 'Factors and Unit Characters in Mendelian Heredity', *The American Naturalist*, Vol. 47, No. 553 (1913), pp. 5-16

analysis, something to theorise and experiment with, which is tied nevertheless to observable phenomena, experimental context or established theory so that testing can be performed. Having some kind of conceptual basis for approaching such a problem is of necessity. In more philosophical terms there is a strong theory-ladenness to the investigation and understanding of unobservable causes. In the same context openness and flexibility of concepts is also desirable. In the case of the gene that was a strong context of hostility towards the postulation of unobservable particles as hereditary causal agents. But lack of knowledge was as much a justification as a warning. In combination the openness and epistemic values of the gene concept provided through the attitudes of researchers a way of openly and flexibly approaching the phenomena, providing resources and constraints for description and experimentation in the form of causal-explanatory relations, conceptual value as a foundation for theory building, and experimental value by representing something manipulable in experimentation.

3.4.1 The Gene Concept as an Unobservable Entity Concept

As mentioned one of the points of this dissertation is to make an historical claim about the use of unobservable theoretical entity concepts generally in the history of science which goes some way to understanding their contribution to scientific practice. I expressed that I thought there were some unifying elements regarding the use of this class that comes through their nature as theoretical or unobservable. In the case of the gene concept we have an illustration of the roles this factor has played. The gene concept was constructed despite the lack of direct epistemic access to it, and also to the causal mechanisms and structure of heredity and biochemical systems in general. The access that existed was grounded on causal-explanatory relations and certain experimental contexts, and the ability to manipulate those. In such a situation early geneticists and other scientists that used the concept, relied on its explanatory value and other epistemic functions together with a realist attitude in support of its open-endedness, to justify their investigation of biological phenomena as though the gene entity existed, which included the investigation of the gene itself. It’s thus fair to suspect that in general unobservability (or degree of theoretical dependence) and lack of epistemic access has played a role in what scientists have been willing to accept of a concept, as far as description and re-description of the entity are concerned, and has shaped their use of the concept accordingly. In retrospect openness and flexibility is itself epistemically valuable, as it was in the case of the gene concept, allowing through new description and redescription the wider application of the gene concept to give more complex explanations as the evidence of the gene's role and the complexity of heredity became more apparent. It was thus accepted that not knowing what the gene was or how it worked such additions and alterations to the gene concept would be an acceptable part of its use as principally a conceptual and explanatory starting point for investigation and research into the causes of heredity and development, within which the concept itself could be developed to meet its evolving conceptual and explanatory demands.

As an historical point then, we have with the gene case evidence of the historical value of unobservable postulates to scientific practice which was posed in chapter 2. In the case of heredity and development, the gene concept provided a starting point for research and to structure investigation into what was essentially complex and uncertain. It provided resources and constraints in this respect that weren't otherwise available. At the same time the postulation and treatment of the concept openly allowed scientists to manage their lack of knowledge while taking advantage of the epistemic benefits of the concept in this regard to help provide that very knowledge. Needless to say the treatment of the gene concept as an epistemic frame in this way I think is something shared to a greater or lesser extent by other theoretical entities historically. In this sense when we say an entity concept was used as an epistemic frame we imply to some degree that elements of this frame have an independence from descriptions and representations, and rather in fact acted as the structures in which and with which such descriptions and representations were composed. This is in fact the starting point for our observations on conceptual continuity and referential practices in chapter 5.

3.5 Conclusion

What I've tried to then in this chapter is show why we might historically refer to the gene concept, in the context of an ongoing process of research towards understanding heredity and development, as an epistemic frame for that research, thus taking account of both the epistemic roles driving it, and its open-endedness in the context of an ever changing landscape of information and phenomena. In terms of these roles I identified these as historically significant;

1. Explanatory role: ascribing as the cause of traits (or of their differences at least) a stable causal unit of gametic material.

2. Conceptual role: as a stable unit for developing an understanding or conceptualising the causes of various and diverse hereditary phenomena.
3. Experimental role: as a unit for producing hereditary phenomena and exploring them.
4. Conceptual role: as a basis for conceptualising the composition and properties of gametic material and its operation, through particularly its self-replication (stability).
5. Experimental role: as a unit of manipulation for testing and probing the properties and structure of underlying gene entities themselves and their causal action.

These together constituted the basis for the investigative role of the gene concept exploring the material basis of heredity and development, and the elements geneticists relied on to structure and resource this investigation. In this way we begin to form a broader picture of just how important the gene concept was to this historical process of research through its epistemic roles, which gives us some insight into how important theoretical entities have been in giving researchers a grip on complex, uncertain and unobservable causes of specific phenomena.

Further I do agree with Falk that one of the historically interesting characteristics of the gene concept throughout its history has been its apparent adaptiveness and flexibility. Falk locates this in the back and forth shift between unit of function and unit of structure and diversification of uses that characterise the modern use. I think though that from the very outset the concept carried this flexibility and openness, which flowed from its use by classical geneticists and early cytologists and biochemists as an epistemic frame rather than as a bounded or fixed concept. As such many questions were left open and vague, but a structure was provided to investigate them and provide responses. Of course many theories could and did rise and fall within this structure and all sorts of explanations and conceptual schemes using elements of it could prove and did prove erroneous. And there were certain expectations that certain facts would be identified, such as the inner structure of the gene. But with the dual qualities of the gene as both an open concept and an epistemic frame there was built in a flexibility to deal with new issues and novel phenomena, and change one's mind, and this included different attempts to pin down the gene concept and describe its causal mechanism. Whether this kind of understanding of the gene concept helps interpret the post-50s concept (or concepts) I leave at this point open, but I suspect part of the reason the concept endures is precisely because it still provides something epistemic usefully to research in terms of these very roles.

Chapter 4: Case Studies – The Neutron and Nuclear Physics (1900–1939): intertwining a concept and a research process

One aspect of this dissertation is to show how under the rubric of an epistemic account of the history of certain classes of concept we can make assertions about the way such concepts have been used at a general level which reveal something substantial about scientific methodology with respect to their creation. In this respect the gene concept from 1900 to 1953 and the neutron concept from 1900 to 1939, although employed in utterly different fields with no direct theoretical relation, share strong epistemic similarities in the ways they were relied upon to structure research processes into complex phenomena, processes which at the same time by virtue of this complexity, uncertainty and lack of knowledge maintained the concept as open-ended and a subject itself of this research. Like the gene concept, the neutron concept provided the starting point for dealing with what otherwise were conceptually difficult circumstances. We can refer to the neutron concept in this vein as acting historically as an epistemic frame for physicists with which they dealt with radiation phenomena, and pursued research into nuclear structure and nuclear forces.

Of course these similarities between the neutron and gene concepts during these periods exist mostly on the broadest level of description of their research processes and only provide the skeleton of an historical account of their role as epistemic tools, which is filled in by contextual and background-theory dependent uses, and the research decisions that flowed from them. However these broad similarities stand to tell enough of significance about the way theoretical entity concepts, as unobservable (or indirectly observable) postulates about the world, are integrated epistemically into research processes, to shed light on the vexed subjects of conceptual and referential continuity. In the neutron case, like the gene's, the concept fulfilled certain explanatory, conceptual and experimental roles that provided a basis for investigation and research in circumstances that were otherwise problematic and uncertain. In this respect the methodologies towards the concept were similar, given that aspects of the concept were uncertain and the situation demanded and in fact benefitted from the open-endedness in their descriptions or representations. The neutron concept like the gene was both a driver of and subject of the research process, and thus maintained a complex interaction with it, but one governed as will see by these epistemic roles it was developed to provide.

My aim in this chapter is principally again to draw out these themes by giving an *epistemic history* of the neutron concept as a novel historical account from 1900 to 1939, with respect to a process of research that aimed at understanding nuclear structure and nuclear forces. In the last chapter I worked more towards fleshing out the meanings of the various epistemic roles (conceptual, experimental etc) in an historical case. Here I switch to the more historiographical motivation of these ideas of showing how a narrative history can be given through the aid of these epistemic roles, tracking and understanding the developments and interactions of a research process and a theoretical entity concept, where that concept has played a central and formative role. That means assembling an account of the events and interpreting them with respect to the way in which that concept was relied upon as resource and constraint in evolving that process: as a conceptual and explanatory tool for developing theoretical responses to phenomena, and as an experimental tool for probing and producing phenomena. We understand how the concept was relied upon in framing issues of investigation, while at the same time being itself with respect to the relevant research process a subject of investigation.

In the history of physics the neutron concept has an undoubtedly central, if not notorious, part. It was after all the particle concept that allowed the conceptualisation of the atomic bomb, as an uncontrolled chain reaction of neutron production and nuclear fission. While this stands out in popular history, the neutron was heavily engaged conceptually and experimentally in the investigation of nuclear structure, nuclear processes, and nuclear forces, and the discovery of fission emerged from these investigations in the late 1930's. The research process associated with the neutron which I'm pursuing here and which is principal in the 1930's is just this one, for which the neutron itself as an accepted component of the nucleus was a subject of these investigations as well as conceptually an object for their understanding. More broadly my attention will range from the early 1900's (and a little earlier) when the first ideas of a neutral particle were being hypothesised, until 1932 when Chadwick first successfully argued for the existence of a neutron, through the major period of its central epistemic use up till 1939. I will not consider the years of the bomb's production. But 1939 itself has significance however not only because of political and social disjuncture that was the war, but also because it was the year in which fission began to be experimented with by Enrico Fermi's team in Chicago, the nuclear reactions involved having been first understood by Hahn and Strassmann right at the end of 1938.[210] 1939 is thus in some respects the last of a stream of significant developments involving the neutron concept, such as nuclear forces, artificial radioactivity, the efficiency of slow neutrons and slow neutron resonance, that drove the experimental program. During the war years attention shifted in both the experimental and theoretical contexts to the production of atomic weaponry and nuclear power applications, and solving the technical problems of making such processes stable and efficient. It is in fact at this point that many histories of the neutron find a degree of resolution in their discussions of the concept. At a symposium on the history of the neutron in 1962 Emilio Segré (a former colleague of Fermi's) was moved to put it that after the discovery of fission "With the achievement of the chain reaction, the history of the neutron is practically complete….The neutron has ceased to be an object of study in itself, it has mainly become a tool for other work, following in the destiny of many other fundamental objects of study, which after being the centre of attention for a long period, later become instruments for further development."[211] This presents an admirably epistemic understanding of the value of the neutron concept and its productivity in driving and motivating research in this period. However to some extent at least it gives a misleading impression of the concept, treating it primarily as the end product of a research process. In fact the neutron wasn't just the subject of investigation of the research process but also a prime resource in those investigations. Its role was two-fold and we must be careful of passive accounts. Nor has the neutron ceased to be itself a subject of investigation. Not long after this symposium, the quark model came into existence and the nature of the neutron, as well as many other subatomic particles were reformulated and the subject of even deeper investigation. There remains an open-endedness to all these particle concepts even today, as physicists and philosophers debate their interpretation and understanding in the context of quantum field theories. These research processes have thus carried on to deeper and more comprehensive levels of explanation but in the face of the sheer enormity of the complexity revealed have methodologically resisted fixing their fundamental ontological concepts.

Most histories of the neutron concept have come from scientists themselves, including personal reminiscences from participants in the 30's, particularly from the main laboratories

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[210] O. Hahn and F. Strassmann, *Über den Nachweis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen entstehenden Erdalkalimetalle* (*On the detection and characteristics of the alkaline earth metals formed by irradiation of uranium with neutrons*), *Naturwissenschaften* (1939), Volume 27, Number 1, pp. 11-15 (1939)

[211] E. Segré, 'The Consequences of the Discovery of the Neutron,' *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), pp. 149-154, p.154.

operational in neutron and nuclear research at the time. They almost all emphasise the critical importance of the neutron concept to the development of nuclear physics. Devon at the 1962 symposium labelled it "…a focal point from which subsequent developments proceeded."[212] Or in Pais' famous text: "…the discovery of the neutron made for the first time a rational theory of nuclear phenomena possible."[213] Principally these accounts are aware of the fact that the ontological discovery itself was barely the issue, but much more the fact that the neutron concept made possible a research process, built around its use and its own development, into nuclear phenomena, the nature of the nucleus and nuclear forces that wouldn't have transpired without it. It is common knowledge that the neutron itself inspired a tremendous amount of research. Entire laboratories were re-organised around its use and investigation. Its absence of charge gave it causal properties of investigation that were extremely useful. These histories as such tend to skew towards the future successes that flowed from the neutron's introduction, evaluating its impact in these terms. Segré for instance lists the import of the neutron as allowing the foundations for a nuclear model, producing a long chain of experimental discoveries and opening the gate to the practical application of nuclear energy.[214] These accounts are not so useful to us in giving epistemic accounts because they don't, except for the first observation, help us understand what the neutron concept provided to scientists in the state of uncertainty and complexity they were in, unaware of what would follow from their use of the concept. Nevertheless the value of the neutron is not simply an artefact of appreciating retrospectively its successes nor can this charge simply be levelled at these histories some of which do understand the importance of the neutron from its context. Some which focus on personal reminiscences often detail how reliance on the concept produced erroneous steps and dead ends. Chadwick in his letter to the 1962 symposium for instance tells of many of the unsuccessful attempts of the Cavendish laboratories to find evidence of the neutron in the 1920's, unsuccessful in fact because the model of the neutron they were relying upon turned out to be wrong. But they relied on it nonetheless, and this is the important point. Scientists were at the time aware that in the neutron concept that had something that could resolve many of the problems that had prevented any real clear articulation or investigation of the nucleus and they presented their early papers in this manner, and reemphasised it often with the new discoveries that flowed from its use.

[212] S. Devon, 'Comments to the paper of E. Segré', *Ibid*, pp. 155-158, p.155.
[213] Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 401.
[214] E. Segré, 'The Consequences of the Discovery of the Neutron,' *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), pp. 149-154, p.149.

What we'll do here is in this vein, giving a history of these events in terms of the contribution of this concept as recognised and evaluated in its context for scientists engaged at the frontier of research. Overall it might be surprising to hear that the history of theoretical nuclear physics in the 1930's is principally a story of erroneous expectations and assumptions, by experimenters and theoreticians. Experimenters often had wrong expectations about what they would find. Sometimes this unwittingly led to new discoveries such as the effectiveness of slow neutrons in causing artificial radioactivity. Most of theoretical work of the 30's on nuclear forces and processes has been completely rewritten and generalised to a point that only loose connections can now be made conceptually between then and now. The atomic nucleus turned out to be much more complex than anyone thought, new experiments brought forth new unexpected information consistently, and all that scientists were able to do was create starting points for developing research by resolving small problems or giving heavily simplified or idealised models of circumscribed aspects of the phenomena, like the model of the deuteron (heavy hydrogen). Mostly this was acknowledged as all that was aspired too. As a result the story of the neutron concept as an epistemic frame cannot be told, just as for the gene concept, simply in terms of successes and important events as judged from our standpoint, but only from the perspective of scientists at the time approaching unknown, uncertain and extremely complex phenomena.

The work of professional historians and philosophers on the neutron and its discovery (or construction) is rather limited. Most interest with respect to physics in the 1930's has been directed towards the positron concept. The positron (or positive electron) concept attracts attention because it rarefies the early boundaries between theoretical and experimental physics that were in the process of being reformulated with the increasing success and application of quantum physics. The experimental results exhibiting positron paths and the theoretical basis for their description existed (perhaps a year) before their existence was widely accepted. The way scholars interpret this varies. Many of course use it to belie the idea of 'discovery' in physics as opposed to 'construction'. Kuhn and Hanson for instance see it as a case of theory-ladenness of observation, emphasising that the existence of a new particle could only be accepted once the theory used to interpret the experimental data (here Dirac's field theory) had been accepted and justifiably applied (usually first attributed to Blackett and Occiahlini's application of the theory to their data in February 1933)[215]. Roque more recently counters this

[215] N.R. Hanson, *The Concept of the Positron* (Cambridge, Cambridge University Press, 1963); T.S. Kuhn, 'The Historical Structure of Scientific Discovery', *Science* (1962) 136, pp. 760-764.

somewhat with the claim that in fact the application of theory itself was not enough to convince most while the positrons remained cosmic ray, rather than laboratory, phenomena.[216] Rather scientists had to show that they could manufacture the positron in the laboratory by means of radioactive sources and manipulate it experimentally according to the theory before general acceptance was had. In sympathy with Hacking perhaps, the theory had to prove its worth through experiment by giving scientists control over it before scientists would employ the theory in justification of a new particle.

The discussion of the positron has some relevance for the neutron concept, especially since it too came into being in the context of a revision of the experimental and theoretical boundaries in physics and played its role in merging the two much more closely than they had been before. The neutron had as much a part in this as the positron, if not more so. Unlike the positron, it was an experimental result that more or less convinced the majority of physicists that they were dealing with a new entity in the case of the neutron. It may well be this was because in the context of this result the epistemic value of the concept and its ability to be redeployed, reproduced and investigated in accordance with its theoretical understanding was clear at the outset, which supports Roque's contention that these kinds of factors were important at the time to the way physicists reacted to critical new ontological suppositions. The neutron concept was embedded in experimental setups from the beginning, and these could be controlled to gain information about the neutron (such as its mass) or to use the neutron to causally affect other phenomena and test their properties (such as the stability of nuclei). Epistemic value in this sense has probably been in the context of particle physics at least a prerequisite to ontological acceptance and is worth a more dedicated study in terms of how it shaped the commitments of researchers.

The best work specifically about the neutron concept comes from Bromberg which we will rely on here.[217] It details in the theoretical context the significant impact the neutron concept had on resolving many of the conceptual difficulties theorists were experiencing with respect to the nucleus. These developments can be understood as part of the epistemic value of neutron concept to theorists in the 30's. Brown (with Moyer and Rechenberg) has also written extensively about other activities going on at around the same time, including the formation of

[216] Xavier Roqué, 'The Manufacture of the Positron', *Studies in History and Philosophy of Modern Physics* (1997), 28 (1), pp. 73-129.
[217] J. Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, pp. 307-341.

a neutrino concept, and the problem of beta-decay, both which intersect with the history of the neutron concept.[218] Otherwise the most important text for this type of approach with respect to the history of particle physics comes from Arabatzis, who we extensively discussed in earlier chapters. Many of his observations about the electron concept apply to the neutron; namely the resistance of the concept to anyone's or group's individual designs for it, as well as its independence from its representations and experimental results, the partially independent double lives as entities of experiment and theory, and the constraints on its representation by its past history, particularly its experimental history, which set limits on its reinterpretation. These similarities of course are not surprising given that the neutron fit into the historical context and research practices already developed around the electron and other earlier particle concepts. On other hand these reflect more general properties of the association between the theoretical entity concepts and research processes which is the subject of this dissertation.

While I claim that to understand the roles the neutron concept was playing it is important to understand that these themselves are identified relative to the epistemic goals of the research process, namely the investigation of the nucleus, it is of course important to recognise that the neutron concept was not the only concept playing important epistemic roles in this research. In the 30's and earlier a number of other concepts also played epistemic roles to a significant degree: namely protons, electrons, neutrinos, positrons, photons, mesons and so forth. This separates it to an extent from the early gene concept which was worked with originally without many other theoretical entity concepts. Our ambition here is not of course to use the neutron to completely explain the developments in the history of the nucleus. The reality is that all these concepts played epistemic roles at one time or another in the theorising and experimentation of the physicists concerned. Often the proton and the neutron were used in similar ways and conjointly. The point is rather to elaborate how the research process depended particularly on the neutron concept in terms of the way the concept was relied upon at the research frontier, not excluding that at the same time the process depended on others as well. Reciprocally, we are interested in how the neutron concept was itself elaborated by this process. In this sense we have some correspondence with the idea of Arabatzis, Daston and others of writing histories from the 'object's' perspective. Our entity provides one perspective with which to organise a history and tell a coherent and continuous story around its own

[218] L.M. Brown and D.F. Moyer, 'Lady or Tiger? The Meitner-Hupfeld Effect and Heisenberg's Neutron Theory', *American Journal of Physics* (1984) 52, pp. 130-136; L.M. Brown and H. Rechenberg, 'Nuclear Structure and Beta Decay (1932-1933)', American Journal of Physics (1988) 56, pp. 982-988; L.M. Brown, 'The Idea of the Neutrino', *Physics Today* (1978) September, pp.23-28.

contribution and development, without of course excluding other perspectives. Of course the centrality of the concept to these events weights towards the worth of its perspective. It goes really without saying however that in many cases the value of neutron concept itself derived from its relations with other entity concepts, and the fact that it could be used in conjunction with them as part of a conceptual framework. In these cases the parts it plays in respect of these is also an issue of our investigation.

With this overall aim then of investigating historically the epistemic roles of the neutron concept until 1939 I've divided this chapter into three sections. The first concerns the rather long history of the neutron concept before 1932 and its experimental 'discovery' (I don't use that term uncritically) by James Chadwick. The idea of a neutral particle was employed for different epistemic reasons to solve a range of problems many having nothing to do with its later purposes. This provides a good ground for studying the identification of a concept with the tasks set for it. There is a continuity from some of these older conceptions to Chadwick's which comes through these epistemic links with a particle meant to explain certain phenomena, rather than in terms of its description. As Pais expresses Rutherford's neutron concept is not ours, which has half-integral spin.[219] Yet Rutherford's neutron concept was developed with certain epistemic roles in mind (explanatory, conceptual, and experimental), and these persisted in Chadwick's notion and throughout the 30's. Indeed the neutron's association with nuclear research was partially formulated in these pre-stages through these associations and carried on through the 'discovery event' in direct connection with the epistemic tasks identified with it. In essence then this part is an illustration of the way theoretical entity concepts can come to be associated with their epistemic roles, and also the way in which they are posed to fulfil them. It shows the beginning of the neutron's entanglement with the investigation of the nucleus.

The second concerns the original deployment of the neutron concept at the hands of Chadwick experimentally with his generally accepted claim to have discovered it, and not long after, Heisenberg's use of it theoretically to ground a quantum mechanical treatment of nuclear forces and nuclear structure. The discovery process itself with its various mistakes and presuppositions, is indication of the way in which the neutron filled a conceptual and explanatory role that made the experimental evidence interpretable. Something similar can be said about the theoretical context too, where the neutron had in fact a critical historical role

[219] Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 398.

providing a way out of an extremely problematic conceptual dilemma that was leading people like Bohr and Heisenberg to pose giving up the law of energy conservation at distances smaller than a nucleus. As such the neutron concept opened up immediately a wide range of investigative, conceptual, experimental and explanatory opportunities, relying on critical property assumptions, such as its neutrality and its structural role as an element of the nucleus, and thus situated itself in the centre of research into the nucleus.

The third investigates the neutron concept and the evolving investigation into the nucleus up until the first fission experiments in 1939. We will pursue these investigations in the separate frameworks of experiment and theory which were to some extent autonomous in their use of the neutron concept. In the experimental context the prime use of the neutron was naturally enough as an experimental tool to probe nuclear phenomena and to produce new nuclear phenomena. In the theoretical context generally the neutron concept provided conceptual and explanatory roles and in this respect. But these contexts cannot be rigidly enforced uncritically, as the boundaries between the two were evolving in this period and the neutron concept was clearly a participant in these transformations, providing a conceptual route for theoretical practice to influence and guide experimental practice. The significant aspects of this investigation for us are however that they reveal the association between the epistemic roles of the neutron concept and this research (into the nucleus) which depended on both the paths of investigation and research that the neutron concept generated, not least as an object of investigation itself. Both its openness and its investigative roles will be highlighted in this respect. None of these accounts are complete descriptions, but rather aim at drawing out salient parts of the history.

The end result of this will be the knowledge that the neutron was built in to the research process in a way that maintained and took advantage of the flexibility and open-endedness in its conception. As is in the gene case the causal agency of the concept and its theoretical nature contribute to our description of its use and its part in the research process as that of an ‘epistemic frame’.

4.1 The Prehistory of the Neutron Concept

Surprising as it might seem, the neutron, in the sense of a neutral subatomic particle, has a rather long history. A few scientists have gone to the trouble of assembling this history and while most of the ideas that emerged were simply speculative and of no lasting consequence, and certainly of no help establishing the neutron concept as part of a research program, what they reveal is a diversity of concepts associated with certain epistemic goals.[220]

The earliest conceptions arose while the aether was the governing ontology of light and matter, and for these physicists the problem was how to physically account for x-rays (discovered 1895) and cathode rays (accounted for with electrons in 1897). The earliest reference to the word 'neutron' that Feather finds is in the work of an Australian William Sutherland working in the late 19th century[221]. In 1898 he proposed, relying on the developing electron concept of Thompson and Stoney, that both x-rays and cathode rays could be explained by the movements of free electrons in the aether. Of course the aether itself was neutral, and thus he required both positive and negative electron concepts, which would bind under electric attraction "to form the analogue of a material molecule, which might be conveniently called a neutron." Cathode rays then arose from the splitting of neutrons and x-rays from the vibrations of electrons in the aether. Sutherland's use of the concept was mostly purely explanatory: to allow him to source electrons from the aether while accounting for its neutrality. But he took the concept and began to apply it in a conceptual way to other phenomena, developing a symbolism to represent the neutron, and then using it as part of theoretical system to help deal with the problem of valency and atomic chemical binding relations. Feather labels his speculative work 'romantic' and a 'fantasy', freely posing new concepts and relations but never using them to develop any quantitative predictions or experimental tests, and it seems that Sutherland never took the neutron concept very far. While of course relying on hindsight a little too much perhaps for critical ears, Feather does at least acknowledge the importance of context at this time in physics which could be characterised by the extreme uncertainty with regard the micro-physical causes of new phenomena like x-rays and electrons and their origins. This promoted just as it had in the gene case the search for ideas and concepts which seem speculative from today's perspective (especially since they turned out to be far from the mark) to provide something to rely on given the aether concept itself was failing to directly provide sufficient resources to address these new phenomena and needed to be augmented. A similar notion of a neutron as a bound positive and negative electron system in the aether occurs in editions of a text by Walther

220 See particularly N. Feather, 'A History of Neutrons and Nuclei', *Contemporary Physics* (1960), pp.428-453; B. Kröger, 'On the History of the Neutron', *Physis* (1980) 22, pp. 175-190. Also E. Amaldi, 'From the Discovery of the Neutron to the Discovery of Nuclear Fission', Physics Reports (1984) 111 (1 to 4), pp. 1-332. Almadi relies mainly on Feather and Kröger.

221 W. Sutherland, 'Cathode, Lenard and Röntgen rays', *Philosophical Magazine* (1906) 47, p. 269.

Nernst on theoretical chemistry calling the particle again the 'neutron' but not referencing Sutherland.[222] This idea did not excite physicists substantially except that Thompson (1907) used the idea of binding positive and negative particles to give a prediction of the mass of the ether. The idea of a neutral particle was merely meant to simulate atoms and not anything novel. In any case Amaldi speculates that the neutron of Sutherland and Nernst may have been the source of Rutherford's concept even though his use of it and purpose for it was quite distant from theirs.[223]

4.1.1 William Bragg and 'Neutral Pair' Theory

While the first concept of the neutron we can epistemically associate with resolving problems of cathode rays, x-rays and the aether, the second concept was directed towards the problem of γ radiation. In 1900 Villard had discovered that radioactive atoms give out not just α and β rays (found out later to be helium nuclei and electrons respectively), but also another type ionising of radiation that was far more penetrating but not deflected by electric or magnetic fields. Rutherford reviewed the problem himself in 1904 confronting the question whether these were particles (with mass), in which case they would clearly have no charge, or rays like x-rays. His note of significance, which gives evidence that at this point Rutherford was himself thinking with the concept in mind and tracing its ramifications, was that such a neutral particle moving through matter "would probably not be absorbed as rapidly as a charged particle of the same mass or velocity."[224] Rutherford did not pursue this idea further however.

William Bragg did however by proposing what he called his 'neutral pair' theory of x-ray and γ ray radiation. Bragg is an interesting case for us since he used this concept to establish a research programme which competed with other possible explanations, but also he, more consciously than most, emphasised both the epistemic reasons for his postulations and the need to treat it with a degree of open-endedness given the 'puzzling' nature of the phenomena at hand. Bragg between 1907 and 1914 accepted the accumulating evidence for the similarity of x-rays and γ rays, but rejected the association between the x rays and the aether, proposing

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[222] These ideas occurred in the fourth edition of a text entitled *Theoretical Chemistry* (Stuttgart, Enke, 4th Ed., 1903).
[223] E. Amaldi, 'From the Discovery of the Neutron to the Discovery of Nuclear Fission', Physics Reports (1984) 111 (1 to 4), p. 27.
[224] E. Rutherford, *Radioactivity* (Cambridge, Cambridge University Press, 1904)

instead these rays to be material combinations of electrons and positive particles.[225] His ideas about what these positive particles consisted of changed, from originally α particles, to positive electrons and then to a more amorphous concept of charge picked up from atoms by an electron on its path through matter, but they kept to the same explanatory purpose to explain the properties of γ rays. Their neutrality explained their undeflected path through electric and magnetic fields and their penetrative power. Bragg particularly had taken note of the fact that x-rays, often caused by electrons falling on the anticathode of a cathode tube, passed on almost identical energy to the electrons leaving at the other end upon collision with these rays, suggesting a possible continuity of electron-sized particles throughout. Certainly intensity of the radiation didn't seem to affect the velocity of the emitted electrons only the number emitted, suggesting again, a continuity of the electrons (although with varying charge) throughout.

Stuewer documents the controversy that arose between Bragg and Barkla from 1907 on as both pushed respectively the opposite theories.[226] Bragg developed his theory as a research programme in the effort to find experimental proof and greater explanatory success for the neutral pair idea, relying on it epistemically. Bragg for instance reasoned from his corpuscular hypothesis that the secondary electrons emerged from a material exposed to γ radiation would be independent of the material (being the same electrons and thus bound by conservation of energy). This turned out largely a correct prediction. In his objections to Barkla he argued that pulse theory was comatose in so far as it could provide resources for addressing the problem of γ rays.

"It seems better to me to put it aside provisionally…To take such a step is no denial of all connection between x-rays and electro-magnetic phenomena: it is but to put down one tool and to take up another better fitted for the moment to work in hand."[227]

Bragg referred to his hypothesis as a 'working model which correlates all the known facts and has led to further discovery', in a letter to Sommerfield dated 1910[228]. Elaborating further, "I am only trying to state an alternative view, which seems to me to possess great advantages in

[225] For instance W.H. Bragg, *Studies in Radioactivity* (London, Macmillan, 1912).
[226] R.H. Stuewer, 'William H. Bragg's Corpuscular Theory of X-rays and γ-rays', *The British Journal for the Philosophy of Science* (1971) 5(19), pp.258-281.
[227] W.H. Bragg, *Studies in Radioactivity* (London, Macmillan, 1912).
[228] Reproduced in R.H. Stuewer, 'William H. Bragg's Corpuscular Theory of X-rays and γ-rays', *The British Journal for the Philosophy of Science* (1971) 5(19), p 269.

certain directions; it may perhaps be only an approximation to the truth which we are all seeking."[229] Chiefly Bragg relied on the idea that turned out later to be partially right, that the operation of x and γ rays were principally through a concentrated quantum of mass and energy, rather than the dispersion of a wave, and he took the cloud chamber evidence of Wilson to support this. This is how he relied on his particle concept. At the same time he kept his neutral pair concept open to adaptation transforming the positive aspect from an α particle to a positive electron to a 'quantity of positive' electricity over a 3 year period. At same time he recognised that there were explanatory insufficiencies to his model. He acknowledged this from the outset by virtue of the fact the x-rays as proved by Barkla could be polarised.

What's interesting about Bragg then is that he himself emphasises and shows awareness of many of the points that underlie I think the later use of the neutron concept (or neutral particle concepts) given the context of research at the time. He used his neutral pair concept relative to the state of uncertainty and the difficulties and complexity of the phenomena to pose a conceptual starting point that he felt had research potential and could guide experiment even if the concept itself could only be considered to represent a loose approximation to the truth. The story of the neutron concept, if not always expressed explicitly by the scientists involved so clearly in these terms, displays generally the search for useful notions that could work as resources and starting points in the face of the complexity the phenomena which characterised what physicists were witnessing experimentally in the laboratory. If fruitful they could be incorporated as part of the conceptual basis of an ongoing research process. Bragg's idea however couldn't be made to account for the wave properties which x-rays and γ rays were displaying and it was surpassed by the concept of the photon (relating momentum and wavelength) and Compton's theory of photon scattering.

4.1.2 The Neutron and the Nucleus: Rutherford's proposal

However Chadwick's conception of the neutron is not associated with either of these, and can only loosely be connected to it through Rutherford's thinking about the experimental possibilities of a neutral particle. The purpose and research process with which the neutron concept is associated is that of the investigation and interpretation of the nucleus. This can be contrasted with the earlier ideas above.

[229] *Ibid.*

The neutron concept that arose with Chadwick's experiments in the 1932 can in fact be traced back almost 20 years before these, and indeed its prior emergence was in fact essential to Chadwick's very development of these experiments. It arose because of the advent of the nuclear model of the atom, which itself arose in the context of the investigation of the atom and formulation of atomic models. In 1911 Rutherford famously published his model of atom, relying on the results he had been producing since 1909 firing fast α particles at gold foil, which were inconsistent with the then dominant models of the atom which spread out the negative and positive charge evenly.[230] Most α particles penetrated the foil but a few surprisingly bounced right back as though they had encountered something quite hard. Rutherford after thinking about this concluded that the positive charge must be concentrated at the centre of the atom with the negative charge moving around the outside, thus explaining with the nucleus concept these rebounding α particles.

The nucleus concept quickly became a central concept in the understanding of the atom, and attention turned subsequently to the question of the structure of the nucleus and how the nuclei of various atoms differed given the patterns of the periodic table. This is the context in which the early neutron concept which we associated with Chadwick and the 1930's arose. This context involved, through the 1910's and onwards, the increasing realisation that there was an inner complexity to the nucleus including the nature of the forces binding it. The well-defined energies of γ and α particle emission hinted towards this structure. Radioactive transmutation experiments and observations had made clear a relation between atomic mass and nuclear charge (of 2:1 in smaller elements). By 1920 Rutherford had discovered that α particles obeyed Coulomb scattering only to within 3 x 10^{-13} cm of the nucleus.[231] Further in 1919 he had discovered that firing fast α particles at nitrogen atoms, caused the nitrogen atom to disintegrate releasing a proton.[232] Experimenters took this as an essential cue that a way to understand the nucleus could well lie in being able to break it apart. Many experiments proceeded then on just this basis.

[230] E. Rutherford, 'The Scattering of α and β Particles by Matter and the Structure of the Atom,' *Philosophical Magazine* (1911) 21, pp. 669.
[231] See E. Rutherford, 'Collision of α-particles with light atoms. I. Hydrogen', *Philosophical Magazine* (1919) 37, pp. 537. Rutherford himself tried to fashion an explanation using electromagnetic forces and structural properties of the α particle. But upon further Cavendish experiments, Chadwick and Bieler wrote an article ruling out the involvement of the inverse square law given the accepted structure of the α particle, thus opening the way the postulation of a new nuclear force. J. Chadwick and E.S. Bieler, 'The Collisions of Particles with Hydrogen Nuclei', *Philosophical Magazine* (1921) 42, pp. 923.
[232] E. Rutherford, 'Collision of α-particles with light atoms. I. Hydrogen', *Philosophical Magazine* (1919) 37, pp. 581.

The neutron's earliest development in this respect pertained directly to the question of how nuclei are built up or 'nuclear systematisation'. In 1913 a Dutch physicist Antonius Johannes van den Broek concluded that there needed to be electrons in the nucleus to explain why the atomic mass of an atom seemed to be twice the charge (implying twice as many as protons in the nucleus as the charge).[233] This excess charge could be neutralised by electrons. Systemisation gained momentum with Aston's results from his mass-spectrometer system, measuring the weight of nuclei and in February 1920 William D. Harkins used some of these results to hypothesise that in order to explain the stability of heavy nuclei it should contain particles, called μ particle, which were combinations of two hydrogen nuclei and two electrons.[234] He did conceive that such particles should be able to pass closer to a nucleus than an α particle, signalling that he was thinking about the ramifications of the concept and how the particle should behave.

According to Feather, Harkins had been openly speculating about these neutron ideas since 1915.[235] It's not clear though whether Rutherford was influenced by these or not, and his own critical work on the subject doesn't mention Harkin's or μ particles, an oversight which he later corrected, but before he could, Harkin's had written to Rutherford expressing his disappointment that Rutherford gave him no credit in his paper of June 1920[236] (from his influential Bakerian lecture published in the *Proceedings of the Royal Society*[237]). However Rutherford exposes his thinking perhaps when he wrote to a colleague at Yale that "most of the ideas….have been common property in this country and especially to myself for the last five years. It is exceedingly easy to write about these matters, but exceedingly difficult to get experimental evidence…"[238] This suggests of course that these ideas about a neutron concept were flowing backwards and forwards, but there was a hesitancy to publish them given the lack of any solid empirical basis.

Rutherford however did in the Bakerian lecture of 1920. What marks this lecture out from the ideas coming before is that Rutherford does apply himself to using the concept not just to give explanations of various properties of the nucleus, but to consider how the concept could

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233 Reviewed in B. Kröger, 'On the History of the Neutron', *Physis* (1980) 22, pp. 175-190.
234 W.D. Harkins, 'The Nuclei of Atoms and the New Periodic System', *Physical Review* (1920) 15, pp. 73-94.
235 N. Feather, 'A History of Neutrons and Nuclei', *Contemporary Physics* (1960), pp.428-453.
236 *Ibid*, p261.
237 E. Rutherford, 'Bakerian Lecture: Nuclear Constitution of Atoms', *Proceedings of the Royal Society* (1920) 97(686), pp. 374-400.
238 N. Feather, 'A History of Neutrons and Nuclei', *Contemporary Physics* (1960), p. 261.

provide a basis for investigating the nucleus, and would manifest itself experimentally. It's with Rutherford then that we can argue that the neutron concept begins to be incorporated even if only weakly into the research process of nuclear investigation in the experimental context. Rutherford himself was famous for his scepticism of speculations in theoretical physics and thus consideration of the neutron was always likely to be formulated in the context of how it would manifest itself experimentally, giving it a life in his laboratories that it couldn't have had in the speculations of theoretical chemists and physicists.

Early nuclear physics was in fact experimentally owned, and shared particularly by the Cavendish Laboratories under Rutherford in Cambridge, Lise Meitner and Otto Hahn in Vienna, Marie Curie and Pierre Joliot in Paris, to name the most prominent. The nucleus had arisen out of an experimental context, at the hands of Rutherford, and its investigation had proceeded through the use of proton, or α, β, γ particle, bombardment, observing the resulting composition of the products (protons, or α, β and γ, particles) and their energies. Transmutation had been discovered in 1901 by Rutherford and was now being studied as a nuclear process. For good reasons as we'll see in the next section theoretical physicists were unable to gain any real traction dealing with the nucleus concept. Thus experimenters proceeded, more or less using basic physical relations, such as the conservation of energy and momentum, and the laws of electro-magnetic interactions (Coulomb's law for instance). On this basis Rutherford for instance had built up his original scattering theory of the hydrogen nucleus and estimated its radius.[239]

In the Bakerian Lecture (to the Royal Society, held the 3rd of June 1920) Rutherford posed the concept of the neutron in the context of the question of nuclear constitution. For a start he acknowledged the general lack of information of nuclei and their nature. The conceptual resources available with which to construct nuclei were protons and electrons, both assumed to be part of the nucleus given the observed emission of hydrogen ions and β rays. It had also been noted that helium nuclei (α particles) had much lower mass (thus much higher binding energy) than the mass of their component particles individually. This suggested to Sommerfeld, an idea that was relied on significantly, that α particles are central nuclei components.[240] Rutherford didn't differ in relying on these ideas including with respect to the formulation of his neutron conception. He was in fact relying on observations of the emissions

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[239] E. Rutherford , "The Scattering of α and β Particles by Matter and the Structure of the Atom", *Philosophical Magazine* (1909) 6, pp.21.
[240] A. Sommerfeld, *Atombau und Spektrallinien* (Braunschweig, Vieweg, 1919).

of nitrogen and oxygen atoms under bombardment of α particles. He took these to be, mistakenly as it turned out, neutral particles. After studying the deflection properties of these entities Rutherford calculated a mass of about 3.1 and a charge of 2, suggesting a particle composed of 3 protons and 1 electron (electrons having negligible mass). He then considered;

"If we are correct in this assumption its seems very likely that one electron can also bind two H nuclei and possibly one H nucleus…..In the [latter] case, it involves the idea of the possible existence of an atom of mass 1 which has zero nucleus charge."[241] (396)

He proposed this particle to be more or less a condensed hydrogen atom, speculating that "under some conditions,…,it may be possible for an electron to combine much more closely with the H nucleus."[242] Rutherford then proceeded to use the concept to evaluate how it might be witnessed. Such a particle if it existed would have zero external electric field except very close to the nucleus, thus enabling it to move relatively freely through matter, and enter nuclei possibly uniting with the nucleus or disintegration under the action of its intense positive field resulting in the escape of the electron (presumably then a β particle) or a hydrogen nuclei (proton). However one should expect to produce such particles by running discharges through hydrogen, where both electrons and H nuclei are present. He declared his intention then to test for these particles under these conditions.

While admitting that both nuclear forces and nuclear structure were extremely uncertain, Rutherford composed his neutron concept in this context as an idea that had research consequences and whose ramifications could be investigated. Certainly it seemed to Rutherford that neutral particles of various sizes were needed to explain the 'building up' of heavy nuclei, "for unless we suppose the production of charged particles of very high velocities it is difficult to see how any positively charged particle can reach the nucleus of a heavy atom against its intense repulsive force".[243] Rutherford probably had the production of heavier elements in stars in mind, when thinking about the neutron in this respect, and thus the very the existence in nature of these elements.

[241] E. Rutherford, 'Bakerian Lecture: Nuclear Constitution of Atoms', *Proceedings of the Royal Society* (1920) 97(686), p. 396.
[242] Although it is a little puzzling and worth investigating why Rutherford here didn't feel bound in his speculations by Bohr's quantum mechanical description of the nucleus here, which prevented electrons binding more closely than the ground state of a hydrogen atom.
[243] *Ibid,* p. 397.

Rutherford didn't in fact apply this idea significantly in a conceptual way for putting together nuclear structure, using mainly the mass 3 and 4 neutral particles instead for a few elements. The role of his neutron was left open in this respect, referring to it only as a 'probable unit of nuclear structure'. He did however associate the neutron concept strongly with a proton and electron combination (the *pe* – model). It's this model which Rutherford relied upon to speculate about how his neutral particle might emerge and behave in certain contexts, and thus for him served to elaborate its conceptual role as unit of element construction. This model was in fact relied on essentially in early uses of the neutron concept, as a basis for experimental manipulation. In this regard Rutherford lived up to his promise to devote laboratory time to searching for a neutral particle through the agency of its binding processes by putting members of his laboratory to work by virtue of the assumed *pe*-model and its role in nuclear structure. Both papers reveal that Rutherford was thinking about the neutron as a potential solution to the problem of element formation and energy production in stars. In the former case it had been observed that heavier elements accumulate in stars while hydrogen and lighter elements are consumed. Glasson (1921) posed that the neutron, lacking charge, would be capable of entering a nucleus and adding to the atomic mass, with the release of its electron, much more easily than positive nuclei could combine.[244] Roberts (1922) notes that in a star violent collisions between electrons and hydrogen nuclei could be happening all the time leading to the production of energy through the creation of these extremely stable neutrons (with higher binding energy than an ordinary hydrogen nuclei).[245] Both in effect tested for this energy release by pushing electrical discharges through hydrogen as Rutherford had suggested, looking for the energy released by neutron formation. Both hence relied on the *pe* – model. Neither found anything. But these experiments do indicate that Rutherford was thinking in terms of conceptual and experimental roles for the neutron, relying on its presumed properties, and was treating the neutron concept as something that could sustain and structure a research process and guide further investigation of the nucleus and nuclear phenomena, including its own examination, through these very properties and roles. It was not simply an idle postulation.

However that the neutron concept was nonetheless oscillating around the Cavendish halls throughout the 20's is clear from Chadwick's own reminiscences. He records conversations

[244] J.L. Glasson, *Philosophical Magazine* (1921) 42, p.596.
[245] J.K. Roberts, 'The Relation between the Evolution of Heat and the Supply of Energy during the Passage of an Electric Discharge through Hydrogen', *Proceedings of the Royal Society* (1922) 102, p. 72.

between himself and Rutherford regarding the neutron during the lost sessions counting scintillations in dark rooms from disintegrations during experiments.

"It was during these periods of waiting to begin counting that [Rutherford] expounded to me at length his views on the problems of nuclear structure, and in particular the difficulty in seeing how complex nuclei could possibly build up if the only elementary particles were the proton and the electron, and the need therefore to involve the aid of the neutron."[246]

Again the reference to 'building up' should probably be considered with stellar processes in mind, as a natural process where transmutations were required, and not simply with respect to the possibility of producing artificial transmutations in the laboratory. The neutron seemed to be necessitated by natural processes, and not simply an imaginative hypothesis by experimenters of what could be used to construct elements. In any case Chadwick claims to have been 'completely converted' by these discussions with Rutherford, and despite this lack of success reports that, "From time to time in the course of the following years sometimes together, sometimes myself alone, we made experiments to find evidence of the neutron, both its formation and its emission from atomic nuclei."[247] He likens some of these attempts 'so far-fetched as to belong to the days of alchemy.' Of the more credible attempts according to him, in 1923 with Rutherford's approval he tried to detect the emission of gamma-radiation from the formation of neutrons in a large mass of hydrogenous material. Once again Chadwick was relying on hypotheses about neutron formation. After the development of the Geiger counter by Geiger and Müller in 1928 (which had a much sharper ability to detect gamma rays) the same experiments were tried again with Chadwick and Rutherford going to 'all manner of tricks in the hope of finding some trace….'

The neutron concept was thus being used to motivate and direct research through hypothesised experimental and conceptual roles even at this early stage, albeit weakly, as part of the investigation of the nucleus and nuclear processes. In this respect it was providing certain epistemic tools for experiment formation. It is clear that Chadwick was thinking conceptually with the neutron concept; relying on its assumed properties (particularly the *pe*-model) for thinking about how it might behave in varying circumstances. For instance some experimental ideas towards the end of the 1920's arose from Chadwick speculating (and thus

[246] J. Chadwick, 'Some Personal Notes on the Discovery of the Neutron,' *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), pp. 159-162, p. 159.
[247] *Ibid,* p.159.

explaining perhaps why they hadn't witnessed it with their other experiments) that a neutron could neither be formed nor exist without the presence of a strong electric field. Thus he calculated that some evidence of them might be found by firing protons into atoms where electrons were tightly bound. These thoughts remained conceptual however (as he reports in these Personal Notes)[248], because Cavendish did not have the money to finance the kinds of accelerators required to produce protons with enough energy to penetrate deeply into the atom.

Likewise as a component of the nucleus, there was an expectation on the part of Rutherford and Chadwick that neutrons might be witnessed emerging after disintegration experiments. Scintillations from particles undeflected by electric and magnetic fields were continuously looked for in this respect although most of this research was not reported publicly according to Chadwick. Nonetheless what all this activity indicates is that in the Cavendish laboratory, particularly for Rutherford and Chadwick, the neutron had become (even if somewhat outside the mainstream activities of the lab) an active concept and part of their experimental research process with respect the nucleus. It seemed to them to fulfil an essential explanatory role regarding the structure and construction of nuclei, which gave them a strong motivation to rely on the concept conceptually to think about nuclear processes and structure experimental investigations. In this sense it provided a resource for extending their research regarding the nucleus and nuclear phenomena. Their conviction in this respect was later to lead them to an expectation of its existence and to interpret experimental results from beryllium in terms of them, while others not familiar with the neutron concept had to rely on other available physical entity concepts that couldn't be made to fit the circumstances. As such this research process and Rutherford's original ideas about the neutron can eventually be connected with the actual discovery of the neutron, as the epistemic frame through which it emerged at Chadwick's hand in 1932 but through a different agency than the combination or separation of protons and electrons. As Pais was moved to exuberate, "Rutherford's vision of the neutron…illustrates strikingly how the pursuit of a conjecture based on very good sense…can open new vistas not anticipated by the best physicists."[249] Pais himself although not in these terms acknowledges that the neutron concept during the 20's was more for the scientists of the time than simply a new proposed element of the world, but was playing investigative roles directing aspects of their research.

[248] *Ibid,* p.160.
[249] Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 409.

4.1.3 The early theoretical life of the neutron concept

Apart from its life in the experimental context, the neutron concept did have some sporadic existence as an object of theoretical physics before 1930, by which I mean theoretical physicists in their context saw it as something of theoretical value. This use was much more limited than the use that was being made of it as an hypothesis by Cavendish experimenters, but it is related to their concept, having similar explanatory purpose and roles, and thus is part of its history. Some of the ideas developed in this context carried over into the way it was employed theoretically from 1932. There were however very good reasons why it was extremely difficult dealing with the neutron concept as it was then instantiated, and why it seemed theoretically not a particularly helpful hypothesis. We will discuss this below.

Of these ideas the most significant was Wolfgang Pauli's. Pauli is mostly associated in this respect for first proposing the neutrino, but in some sense this is anachronistic while ideas relevant to the later neutrino concept and neutron concept are present in his original concept. This occurred in the form of a letter dated the 1st of December 1930 to a gathering of experts on Radioactivity in Tübingen on the 4th of that month.[250] Two things principally concerned Pauli. First the problem of the continuous β ray spectrum in β decay. This fact first observed in 1914 by Chadwick[251] and confirmed in 1927 by Ellis and Wooster[252] after a fairly heated controversy between Lise Meitner and Cavendish experimenters (including Chadwick)[253] seated the continuity to the nucleus itself and not to any external or secondary effect. This challenged conventional thinking about the quantised energy states of the nucleus which expected that like α and γ rays, β rays would emerge with discrete energy levels. This energy was less than that expected by the conservation law and fell away around an average energy of emission. A possible mechanism by which this could be understood, which seemed like it could not be provided by quantum theory, was lacking. As such it contributed to a crisis in theoretical physics over how to account for the nucleus in general. Bohr proposed that in the distances of the size of the nuclei, fundamental physical laws had to be re-evaluated,

[250] Reprinted in Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 315.
[251] J. Chadwick, 'Distribution in Intensity in the Magnetic Spectrum of the β rays of Radium (B + C)', *Verhandlungen der Deutschen Physikalischen Gesellschaft* (1914) 16, pp. 383-391.
[252] C.D. Ellis and W.A. Wooster, 'The Average Energy of the Disintegration of Radium D', *Proceedings of the Royal Society* (1927) A117, pp. 109-123.
[253] Documented in many places. See particularly Andrew Brown, *The Neutron and the Bomb* (Oxford, Oxford University Press, 1997).

particularly the energy conservation law.[254] He thus proposed that lack of energy conservation was responsible for the energy emissions beneath that expected by conservation principles, suggesting that the missing energy had simply vanished. Conservation of energy could hold at best only statistically.

Another significant problem was the spin statistics of the nucleus, or at least of the generally hypothesised nucleus model of protons and electrons. In 1928 Rasetti had proved that N^{14} obeyed Bose-Einstein statistics. But on the *pe*-model of the nucleus (not the neutron), a nitrogen atom was composed of 14 protons and 7 electrons.[255] Each obeyed Fermi-Dirac statistics with spin ½. The sum of these spins produced a particle with at least half-integer spin, thus following Fermi-Dirac statistics. Similar problems were measured in the case of Li-6. Some such as Bohr took this again as evidence of the failure of quantum mechanics at nuclear distances.

Pauli on the other hand staunchly defended the conversation of energy principle and objected quite strenuously to Bohr's proposals. In the letter to the Tübingen meeting he proposed instead 'a desperate way out' of both the spin statistical problem and the continuous beta ray spectrum by posing a new particle in the nucleus (which he called a neutron) which would have spin ½ , a mass of the same order as the electron, and no charge. It would be emitted with an electron in beta decay and account for the missing energy. The sum of the electron and neutron energies would then amount always to a constant. He notes that this particle would have great penetrative power, perhaps 10 times a γ ray. Pauli was naturally reticent about publishing these ideas, but did nonetheless seriously entertain them. As he wrote a month later to Klein, "I do not believe the existence of neutrons is definite, but do think that this hypothesis should be seriously checked or definitely disproved…."[256]

It's clear that Pauli meant these particles as constituents of the nucleus thus solving the spin statistics problem. In 1931 however in his first public talk raising his ideas he dropped its role as a constituent, and by the 7th Solvay conference (1933) it was clear that his neutron had become the neutrino and was no longer playing any role in nuclear structure and resolving the spin statistics problem which was now clearly the providence of Chadwick's neutron. He

[254] Niels Bohr, 'Faraday Lecture: Chemistry and the Quantum Theory of Atomic Constitution', *Journal of the Chemical Society* (1932), Part I, pp. 349-384.
[255] R. Rassetti, 'Selection Rules in the Raman Effect', *Nature* (1929) 123, pp. 757-759.
[256] Quoted in Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 316.

claimed later to have dropped his use of the term 'neutron' precisely because of a link between the concept of the 'neutron' then in currency and nuclear structure, and it's dedication to dealing with β decay.[257]

Carlson and Oppenheimer in a letter to the editor of 1931 picked up Pauli's concept and attempted to theorise about its ramifications, particularly to give an estimate for the range of such particles through a gas, while pointing to cosmic ray observations of particles with low ionisation.[258] These were later to be identified by Anderson with positrons. However Carlson and Oppenheimer thought they could apply the neutron concept to these tracks. Having excluded theoretically that these tracks could be caused by protons and electrons, they proposed, "we have therefore thought it of interest to investigate the ionizing power of the neutrons, which were suggested by Pauli to salvage the theory of the nucleus." To do so they advocated experiments which would test the expectations that thin tracks should be present in beta decay pictures. The results were published after Chadwick's announcements of the neutron, but it is interesting for us that Carlson and Oppenheimer both thought through the idea, enough to think about how it could be applied to other phenomena, but also that they associated it too primarily with the "building up of nuclei".

Pauli's idea is connected to that at Cavendish by its role in constructing the nuclei, and as we will see later, in dealing with spin statistics, but otherwise varies from that of other theorists who were engaging with the neutron as a proton-electron system, and not a fundamental particle. The mass of Pauli's neutron was to be on the order of an electron and clearly the idea was not compatible. Theorists Langer and Rosen however published a paper in May 1931 under the title 'The Neutron' where they explored the concept as a combination of a proton and electron on the basis that 'it would be useful in explaining a number of atomic and cosmic phenomena.'[259] Emphasising the great theoretical uncertainty surrounding the nucleus, their plan was to engage the reader with what could be done with a neutron concept, it was not to persuade the reader directly that it exists. As such they repeated the ideas of Rutherford and his laboratory that a neutron might penetrate the outer shells of electrons and build up atomic

[257] See E. Amaldi, 'From the Discovery of the Neutron to the Discovery of Nuclear Fission', *Physics Reports* (1984) 111 (1 to 4), p. 75. Pais disputes this, *Ibid* p.317. However Pais relies on a newspaper report of the talk in which the journalist might have interpolated the term, especially as it seemed to have some popular currency at the time.
[258] J. F. Carlson and J. R. Oppenheimer, 'On the Range of Fast Electrons and Neutrons', *Physical Review Letters* (1931) 41, pp. 1787-1788.
[259] R.M. Langer and N. Rosen, 'The Neutron', *Physical Review* (1931) 37, pp.1579-1582.

structures as part of a cosmological history. They posed the formation of helium as much better explained by the 'gradual gathering' of four neutrons releasing energy than the rather haphazard multiple collision of hydrogen nuclei. High density matter in stars could also be explained by gravitational pressure pulling neutrons together, particles that without repelling charge could be packed much more closely. Lastly they made a few suggestive calculations with the, at that time, completely open idea of a nuclear force, that could give rise to energy states that could accommodate the electron at such small distances in the nucleus. As such the neutron provided something of a loose epistemic frame, which although speculative, gave Langer and Rosen some suggestive explanatory and conceptual uses on dealing with nuclear forces and various cosmic phenomena such as the construction of nuclei.

These theoretical hypotheses would have generally struck physicists at the time as speculative. Certainly little is really made of them before Chadwick's experiments. The way we should understand them is not as simple ontological suppositions, which was almost secondary to the physicists involved, but as posing a way forward in thinking about the nucleus. Clearly we must understand their situation, confronted in the case of the nucleus with something extremely uncertain, about which little information was available and for which there was not much guide about what information was needed or could be important. Added to this the nucleus was a font of problems and contradictions for the traditional theoretical approaches and available concepts (such as the proton and electron), and theorists were increasingly prepared to accept that new principles or postulations were going to be required.[260] The theoretical tools available were being realised to be insufficient. In this context Heisenberg's conceptual use of the neutron, which we discuss in the next section, has immense historical significance.

4.2 The Deployment of the Neutron

Part of the story of the neutron concept is the story of its prehistory. At this stage (the beginning of 1932) for the physicists involved the concept has no real momentum. It has no real experimental manifestation. Its properties are limited and mostly qualitative. As such its use theoretically and conceptually is restricted to hypothetical explanations, some mostly

[260] See Bromberg's description of the prevalent mood among theorists at the time that another 'major transformation' would be necessary 'as radical with respect to quantum mechanics as the latter had been with respect to the physics of the early twenties.' Joan Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, p. 308.

qualitative conceptual thinking (generally relying on its perceived proton/electron composition) and experimental proposals making use of it that were either unsuccessful or beyond current technology. Nonetheless what we see during the 20's and very early 30's are the first suggestive links being formed between the epistemic uses of such a concept in terms of certain roles directed towards the understanding and investigation of the nucleus and nuclear phenomena: i.e. with an extant but open research process. The fact is that the neutron was not introduced in a vacuum, and, as we will see, was only recognised by Chadwick because he was already thinking with the neutron in mind and it acted as an epistemic frame with which he could try to successfully interpret certain experimental observations. It provided him an essential resource for understanding otherwise baffling phenomena. As such its epistemic uses, as a conceptual, explanatory and experimental resource, playing particular roles in these regards for research into the nucleus, carried to an extent through these events.

With its 'discovery' and identification with particular ray phenomena however, it could not only now be directly manipulated from an available and acknowledged source, and thus studied, measured or fired at nuclei through it causal properties and relations, but it could be confidently relied upon conceptually and in explanatory contexts as something that wasn't simply speculative or for which one had no real access. There was now the possibility of testing directly claims related to it. This is not to say that before this the neutron concept could not be characterised as an epistemic tool. It was, and Chadwick's interpretation of his experiments as we'll see depended on this. Rather it's the basis by which already hypothesised epistemic roles gained efficacy and promise, thus helping integrate the concept quickly into the centre of nuclear research.

4.2.1 Framing its own discovery: Chadwick gives the experimental neutron reality!

Chadwick's announcement in 1932 is not a starting point for the neutron concept, but rather the beginning of a significant shift in the degree to which it was part of research into the nucleus.[261] What is interesting about Chadwick's 'discovery' of it was in fact that he was looking for it and was prepared to find it. For him it was a resource in the back of his (and Rutherford's) mind, that came to the fore when the more expected and standard explanations for a particular phenomena of radioactive disintegration failed. Indeed Chadwick was to later assert that he would have found the neutron much earlier had he not 'failed to think deeply

[261] J. Chadwick, 'Possible Existence of a Neutron', *Nature* (1932) 129, p.312.

enough about the properties of the neutron, especially those properties which would most clearly furnish evidence.'[262] As such the neutron concept provided for Chadwick an epistemic frame through its hypothesised roles with which to interpret experimental results, and had he only applied it earlier, he might have understood where to look for and find the neutron earlier than he did.

We should add one more thing before revealing how these events took place. Above I put discovery in quotation marks. The usual characterisation of this historical episode is to say that Chadwick 'discovered' the neutron. Almost all accounts express it in these terms, including Chadwick himself in his personal reminiscences. However the actual text of the main 1932 paper presenting his experimental work does not take the form of a sharp observation.[263] After all the phenomena itself had already been observed. The issue was how to interpret it. As such in this paper, Chadwick weighs up competing hypothesis and provides what later turned out to be convincing argumentation that the neutron hypothesis was the better explanation. Thus there is no discovery moment here in any traditional sense, but rather a quick acceptance based on argument that the neutron was the best explanation. Why the term 'discovery' is used in this context is interesting but says much about the way scientists interpret events in retrospect.[264] To call it a discovery ignores the degree of interpretation and argumentation that went into establishing these theoretical objects as the relevant causal agents involved and puts an objectivist spin on their acceptance. The fact is the neutron was 'recognised' rather than discovered, and this recognition of course depended on assumptions associated with the concept that provided a frame of properties and expectations through which it could be recognised.

The *recognition* of the neutron then emerged from a series of experiments studying the energy spectrum of α particle emission. It had been realised in the early 1920's through measurements that α particles were emitted with sometimes less than the energy between the nuclei's starting and final state. This led physicists such as Gamow to assume that when this happened the nucleus remained in an excited state before releasing a γ ray to make up the

[262] J. Chadwick, 'Some Personal Notes on the Discovery of the Neutron,' *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), p. 162.
[263] J. Chadwick, 'The Existence of a Neutron', *Proceedings of the Royal Society* (1932) A136, pp. 692-708.
[264] Which has been widely noticed. See for instance Arabatzis, *Representing Electrons* (Chicago, University of Chicago Press, 2006), pp.18-26, for a good account of this. He makes similar observations with regard the electron's own 'discovery'.

difference.[265] It had also been noted that protons pushed out of light nuclei by α collisions were not of the same energy, implying the release of γ rays too.[266] Hence the γ ray concept was being employed almost universally to make up the energy differences. Experimenters set out to find them and test their energy levels, and confirm their presumptions about nuclear processes. For these experiments Polonium (rather than Radium) had become the chosen α particle source given that it provided a strong source, but without giving out so many γ rays that would confound the counters meant to test for these rays from the disintegrated source. Both the Cavendish and Bothe and Becker in Berlin began experimenting in this vein.

In August 1930 Bothe and Becker published a systematic survey of radiation detection for elements across a wide spectrum of the periodic table, having bombarded samples with α particles from Polonium.[267] The surprising element of the results arose when they bombarded Beryllium which didn't behave at all as anticipated. Beryllium gave out intensely penetrative radiation nearly ten times that of any other element. They took this radiation nonetheless to be simply a highly energetic form of γ radiation. In December they gave a more dedicated study of this effect fitting it, as Feather points out, into the γ ray picture, by assuming that the energy of the emitted radiation was less than the maximum energy of the incoming α particles.[268] In fact energy of the radiation was often more (often much more) than the α particle's, which only in September 1931 did Bothe make clear.[269] To make this work it had to be reasoned that the α particle was completely captured by the nucleus (producing carbon) causing an increase in nuclei binding energy.

From this point on problems with the γ ray assumption arose, but at the time experimenters stuck with their conceptual basis, and tried to reason out a solution. The Curie laboratory which had the largest supply of Polonium began experimenting on the penetrability and properties of this radiation. A paper of Curie presented by Perrin to the *Academie des Sciences* in December 1931 reported that the radiation was more penetrative than Bothe and Becker had discovered, and on assumption of its nature as light and its penetration in lead estimated its energy between 15 and 20 MeV, which was at least three times the energy of the

[265] G. Gamow, 'Artificial Disintegration by α particles', *Nature* (1930) 126, p.397.
[266] E. Rutherford and J. Chadwick, 'Energy Relations in Artificial Disintegration', *Proceedings of the Cambridge Philosophical Society* (1929) 25(2), p.186.
[267] W. Bothe and H. Becker, 'Eine Polonium-γ-Strahlung', *Naturwissenschaften* (1930) 18(43), pp. 894-895.
[268] N. Feather, 'The Experimental Discovery of the Neutron', *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), pp. 135-144; p.138.
[269] W. Bothe *Physiks Zeitung* (1931), p. 661.

incident α particles from Polonium.[270] She also dispelled any idea that α scattering alone, rather than capture, could be responsible on the γ ray assumption.

At the same time as this was all unfolding, Webster at the Cavendish under Chadwick's guidance had begun to test also the radiation, again on the assumption that it was γ radiation. He found only 7 MeV on penetration results, but made a significant and surprising observation that the radiation from Beryllium emitted in the direction of motion of the α particles upon collision was significantly more penetrative than in the opposite direction.[271] On the theory of γ ray emission the energy from the collision should be absorbed resulting in a nucleus in high energy state that then dropped to a stable state by emitting energy in any direction equally. The observation was however consistent with the transfer of kinetic energy directly through a collision process between masses. Webster thus reported considering that the radiation might not be γ radiation at all but instead "high speed corpuscles, e.g. of a proton and electron in close combination." However on that theoretical assumption, as we have already noted, such a particle should give rise to thin tracks in a cloud chamber via their small magnetic dipole moment. When photos were taken of the radiation none were witnessed and Webster fell back on the assumption that what they were witnessing were electromagnetic in nature. Chadwick reports "we were very puzzled at the time; we did not know how to reconcile the observations."[272]

However the most telling experiment came almost straight after. Curie and Joliot in a paper of January 1932 which was reported before the *Academie des Sciences* by Jean Perrin, had tested the radiation for its ability to produce transmutations but discovered that through paraffin the radiation emitted protons on the order of 4.5 MeV (where Beryllium was the source of γ particles).[273] Curie and Joliot employed the theoretical architecture for photonic (light particle) scattering, modelling the collisions in paraffin as between a γ ray and a proton, or Compton scattering, to get an energy for the γ rays of 50 MeV. This evidently clashed with the early measurement of 15 to 20, and such energies for γ was far in extreme of what had been encountered before. Nor as they noticed later could the Compton effect give a complete

[270] I. Curie, 'Sur le rayonnement γ nucléaires du bore par le particules α', *Comptes Rendus de l'Academie des Sciences* (1932) 193, pp1412-1414.
[271] H.C. Webster, 'The Artificial Production of Nuclear γ-Radiation', *Proceedings of the Royal Society* (1932) A136, pp. 428-453.
[272] J. Chadwick, 'Some Personal Notes on the Discovery of the Neutron,' *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), p. 161.
[273] I. Curie and F. Joliot, 'Emission de protons de grande vitesses par les substances hydrogenées sous l'influence de rayons γ très pénetrants,' *Comptes Rendus de l'Academie des Sciences* (1932) 194, pp. 273-275.

account since it predicted a scattering cross-section that didn't match the experimental information. In a note a month later they speculated again sticking closely to the γ ray hypothesis that the Compton effect could not be in operation here and there must exist a new mode of interaction between radiation and matter. [274]

Following the Webster results, on Chadwick's account he and Rutherford were primed to interpret results in terms of the neutron, which they keenly suspected was the explanation for these experimental results, despite the lack of photographs. When Curie and Joliot's January paper came through, it struck them as the final conclusive bit of evidence. That Curie and Joliot seemed to stick nonetheless to the γ ray hypothesis meant, according to Feather who was there at the time, that they lost all credibility on the issue at Cavendish.[275]

This is then the context for Chadwick's much recorded experiment, which set out, on the basis that the neutron concept was the right explanation for these explanatory results, to show how the concept could provide a better consistency with the data than the γ ray hypothesis. The entire experimentation was guided and constructed by virtue of the neutron concept, on the assumption that this radiation from Beryllium was composed of a neutral particle.[276] The experiments were set up to show elastic collisions were in operation between these particles and various materials as would be expected from a massive particle, and in order to measure the mass of this particle. Chadwick constructed an ionisation chamber filled with different gases connected to a linear amplifier (a system called a valve counter) to measure current pulses in the chamber. Ejected ions or recoil nuclei from the collisions would generate ionised pairs on further collisions with the air molecules also in the chamber, the energy of which could be measured by the detector. With this Chadwick showed that the penetrating radiation produced recoil atoms not only from hydrogenous substances (like paraffin) but also from helium, lithium, beryllium, air and argon. Measuring the effect on Nitrogen he found that the recoil energies of nitrogen atoms were 2 to 3 times larger than that predicted by the Compton effect for a γ rays of 50 MeV. All this was reported in a letter to the editor of *Nature* on the

[274] I. Curie and F. Joliot, 'Effet d'absorption des rayons y de trés haut frequence par projection de noyaux légers, *Comptes Rendus de l'Academie des Sciences* (1932) 194, pp. 708—711.
[275] N. Feather, 'The Experimental Discovery of the Neutron', *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), pp. 135-144; p.142.
[276] See accounts from N. Feather, 'The Experimental Discovery of the Neutron', *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), pp. 141-142; J. Chadwick, 'Some Personal Notes on the Discovery of the Neutron,' *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), p. 162 and E. Amaldi, 'From the Discovery of the Neutron to the Discovery of Nuclear Fission', *Physics Reports* (1984) 111 (1 to 4),pp. 9-12.

27th of February 1932, entitled the 'Possible Existence of Slow Neutrons.' In it Chadwick states "the difficulties disappear, however, if it be assumed that radiation consists of particles of mass 1 and charge 0, or neutrons."[277] He then proceeds to use the concept to build up a picture of the exact processes involved assuming the neutron. The nuclear reaction producing neutrons would involve the capture of the α particles ($Be^9 + He^4 \rightarrow C^{12} + n^1$). The neutron while passing through the gases produces recoil nuclei, and provides thus a simple reason for why protons from collisions with hydrogen are emitted with higher energies in the direction of the collision rather than reverse. However at the end he states that despite this, the information here is not decisively in favour of the neutron hypothesis, but makes only the point that to accept the γ ray hypothesis would require giving up the conservation of energy and momentum at some point.

Not long after Curie and Joliot performed similar experiments on Helium gas but persisted in interpreting the radiation as a high energy quantum, posing a new mode of interaction of such radiation with nuclei.[278] Thus it is easy to understand that the context was one where justification and argumentation was demanded to back up an interpretation,....and it explains why Chadwick structured his paper and the one that followed in May in this way. What's important is the way in which Chadwick puts the neutron concept to work in these experiments using it to generate experiments to explicitly look for the neutron, by choosing an apparatus that could credibly show effects of its causal agency (given assumptions about its properties such as its massiveness and neutrality) and that could not be attributed to γ particles. The neutron didn't fall out of these experiments by chance. It was explicitly looked for by relying on its conceptual role as a neutral component of nuclei that could be released in certain circumstances. As such the concept of the neutron had an epistemic role in its own investigation.

In May Chadwick published a full article in Nature, entitled "The Existence of the Neutron", where he developed his arguments but at the same time began to incorporate the concept into the investigation of the nucleus and nuclear forces.[279] It had in fact already become clear that neutrons could be used as experimental tools themselves. Around the same time as Chadwick's results were being published, Feather had fired neutrons at nitrogen nuclei and

[277] J. Chadwick, 'Possible Existence of a Neutron', *Nature* (1932) 129, p.312
[278] I. Curie and F. Joliot, 'Emission de protons de grande vitesses par les substances hydrogenées sous l'influence de rayons γ très pénetrants,' *Comptes Rendus de l'Academie des Sciences* (1932) 194, pp. 273-275
[279] J. Chadwick, 'The Existence of a Neutron', *Proceedings of the Royal Society* (1932) A136, pp. 692-708.

observed transmutation.[280] Neutrons could thus act as disintegrating agents sometimes being captured, sometimes not, suggesting an unknown and uncertain complexity to their interaction with the nucleus. Investigative paths probing the causal powers of the neutron and the forces of the nucleus were thus starting to be conceived. The neutral concept was now filling those roles earlier hypothesised of it.

In the May article Chadwick assumed a position of neutrality, describing the results of recoil free of any hypothesis, and detailing its properties, but showing that the photon hypothesis could not work without giving up the conservation laws. He also began to apply the neutron concept quantitatively on the basis of its particle nature (possessing mass, velocity, kinetic energy, and obeying classical elastic collision laws). He was able to show for instance using its assumed role as an element of the nucleus, and conservation of energy and momentum calculations from simultaneously comparing hydrogen and nitrogen collisions, that a neutron must have a mass close to that of a proton to have energy consistent in terms of conservation with the energies of the products and reactants in the original disintegration of Beryllium. The mass of Beryllium had unfortunately not been measured, but Boron had, and thus relying on the similar reaction with Boron ($B^{11} + He^4 \rightarrow N^{14} + n^1$) Chadwick gave the first measurement of neutron mass at 1.0067 u (or 1.0067 times the mass of a proton).

It's at this point that Chadwick began to flesh out the concept more. This mass calculation was in fact consistent with the idea of the neutron being composed of a proton and electron. Unbound the two have a combined mass of 1.0078 u. With an estimated mass less than that, it was clear that this missing mass could provide binding energy holding the two in close combination. This model was thus still very much in Chadwick's mind, calling it 'the simplest hypothesis one can make about the nature of the particle…'[281]. Reasoning further "we may suppose that the proton and electron form a small dipole, or we may take the more attractive picture of a proton embedded in an electron." Both the mass and the structure of the neutron turned out later to be issues for which the concept should be understood in this historical context as ''open-ended' to account for its changes and developments in this respect. Neither of these statements by Chadwick turned out to be correct but merely became pertinent matters of research and conjecture in the life of the concept through the 30's. Both were the subject of the epistemic roles of the neutron concept with respect to the nucleus and developed as these

[280] N. Feather, 'The Collisions of Neutrons with Nitrogen Nuclei', *Proceedings of the Royal Society* (1932) A136, pp. 709 – 727.
[281] J. Chadwick, 'The Existence of a Neutron', *Proceedings of the Royal Society* (1932) A136, p. 700.

roles were further pursued. The *pe*-model served to elaborate these epistemic roles of the neutron concept towards understanding the nucleus, and give them expression, without being fixed necessarily to them.

As such Chadwick also began fleshing out the kinds of details and observations that led to the concept directly becoming an integral part of the investigation of nuclei. Here was the first use of the neutron concept to set up an experiment on the basis of a prediction from its properties. As he points out, as a neutral particle (composed of charged particles) the electrical field of the neutron must be extremely small except at distances of the order of 10^{-12} cm (having given an estimate for its radius on the order of that of an electron at 10^{-13} cm). On this basis he reasoned an expectation that the neutron "should be able to penetrate the nucleus easily, and it may be that the scattering of the neutrons will be largely due to the internal field of the nucleus….On these views we should expect the collisions of a nucleus with a neutron to occur very seldom and that scattering will be roughly in all directions…"[282] This was confirmed by taking the Beryllium neutrons and pushing them through an inch of lead, and observing the amount scattered. At the same time Chadwick mentioned that experiments colliding neutrons with protons to examine more directly the field of the neutron, were already under way, and collision of neutrons with electrons had already been explored, confirming that such encounters were very rare.[283]

It was obvious of course that having added a new particle to the nucleus, that nuclear structure and forces was now going to need to be investigated and understood with the neutron concept in mind. He had no firm postulates in this regard but was clearly anticipating their conceptual, and in turn investigative, role. "Such a neutron would appear to be the first step in the combination of the elementary particles towards the formation of a nucleus. It is obvious that this neutron may help us visualise the building up of more complex structures."[284]

Chadwick's article is thus already witness to a developing process of incorporating the neutron into the experimental research processes and practices already engaged in the investigation of the nucleus, such as disintegration, scattering, transmutation and so on. This had been set in motion by the February report, and Cavendish physicists were working full

[282] *Ibid*, p.702.
[283] P. Dee, 'Attempts to detect the Interaction of Neutrons with Electrons', *Proceedings of the Royal Society* (1932) A136, pp. 727-734.
[284] J. Chadwick, 'The Existence of a Neutron', *Proceedings of the Royal Society* (1932) A136, p. 706.

time on the neutron by the time Chadwick published his article, which is clear from what he was already able to report. This incorporation was framed by its various epistemic roles as both an object for investigation and of investigation. It was clear that the neutron was something about which more information could be had like its mass, scattering cross sections and its ability to interact with various particles and nuclei. At the same time the role of the concept in the investigating the nucleus was being assembled, through its conceptual and explanatory roles building nuclei and seating nuclear forces and nuclear processes, but also in its experimental roles in breaking nuclei apart. Much of these weren't novel, but were anticipated in the original conception of Rutherford and elaborated by others as we've already seen. There is thus a continuity through the 'discovery' event of the epistemic roles of the neutron concept. The difference is that Chadwick's explanatory use of the concept made these uses justifiable, such that experimenters could believe that their use of it conceptually, experimentally and as an object for and of investigation, had grounding in reality and was not just idle speculation. As we expressed in the case of the gene concept, for such theoretical causal agent concepts, the assumption of reality undoubtedly played an important part in how the concept was evaluated epistemically and relied upon. In the experimental context the fact that the rays emerging from Beryllium was something directly manipulable which could be measured and fired at other particles and nuclei gave it significant epistemic potential in the experimental realm that the neutron didn't otherwise have. It became thus a credible subject of investigation, and an object for investigating the nucleus. This explains its integration into the centre of experimental research, which we document in the third part of this chapter.

What I want to emphasise however to end this section is the point I've been making already that the neutron concept's use as 'an epistemic frame' was necessary in this case to its own 'discovery'. The radiation from Beryllium was only understood as neutrons and then investigated with the neutron's properties in mind as Chadwick did, because he had the neutron concept already as a tool for framing this radiation and then investigating it in alignment with that hypothesis. Other physicists who did not have the neutron concept at the forefront of their thinking relied instead on the most available neutral concept, the γ ray, to the extent that they were willing to create new laws to make it work. This is of course not to paint this as a drastic step…creating new forces or interactions for established concepts is a common part of the history of physics and are sometimes right, but only to show how scientific episodes may be pinned to their concepts, in terms of how individuals respond to challenging situations by making use of the resources they have. Frédéric Joliot was famously to utter many years after the events of the early 1930's that had he actually read Rutherford's Bakerian lecture in 1920 he and his wife may have identified the neutron in place of Chadwick, but lacking the concept they missed out.[285] In this sense describing a theoretical entity concept like the neutron concept as an epistemic frame seems to me most appropriate for understanding its 'coming into being'.

4.2.2 Bringing theory to the nucleus: The neutron concept provides Heisenberg a way out of a theoretical crisis!

The neutron concept is rightly said to be the creation and original concern of experimenters. This is to some extent because the nucleus itself at that time was the providence of experiment. The reasons for this were enough if not to precipitate an actual crisis, certainly to create an immense amount of uncertainty among theoretical physicists about the limitations of their presumptions and principles regarding the microscopic world. The problems stemmed principally in hindsight from a limitation of their conceptual framework and conceptual tools, which it took experimenters armed with the neutron concept to give a solution. As such theoretical physicists struggled to come up even with a viable starting point with which to approach the nucleus. Theoretical physics was dealing confidently with the atom as a whole, and orbiting electrons, but then failed completely when it came the nucleus. As such one of the neutron concept's most significant historical epistemic roles was to provide theorists a conceptual way out of these problems.

The problem not just as we see it today, but also to scientists at the time, arose from the assumed proton-electron model of the nucleus and its seeming necessity to any description of the nucleus. The relevant theoretical context in the 1920's can be described in terms of the success of quantum mechanics describing the behaviour of bound electrons and the phenomena they produced, like spectral lines, and also on describing the statistics of groups of identical particles according to their spin. De Broglie's assignment of wave-lengths to electrons had drawn light and matter together within the same possible explanatory system, leading late in the 20's to the nascent quantum field theories from which positrons gained theoretical recognition.[286] The power of the quantisation to electrons outside the nucleus and

[285] Quoted in Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 399.
[286] Louis de Broglie, *Recherches sur la théorie des quanta* (Researches on the quantum theory), Thesis (Paris, 1924); On quantum field theory, the works of Dirac, Born, Heisenberg and Jordan are historically the most important. P.A.M. Dirac, "The quantum theory of emission and absorption of radiation," *Proceedings of the*

nuclei as a whole had thus produced an expectation that quantum mechanics should be applicable to the nuclei elements. But as became obvious to physicists at the time, doing so simply produced contradictions with the experimental evidence and sometimes simple mathematical inconsistencies that the theory couldn't handle. Physicists acknowledged that what created the problem were the electrons in the nucleus. These had been posited in the nucleus early, and seemingly had to be there to explain β decay which was considered a nuclear process. There were four principal problems. First nuclear radii had already been estimated by Rutherford and others to be of the order of 10^{-13}. For an electron then to be bound within the nuclei, the uncertainty principle demanded it possess a wave-length not greater than these dimensions. However ascribing an electron such a small wavelength implied a tremendous kinetic energy for the electron of the order of 23 MeV. At such energies it was hard to conceive of any potential barrier or force that could confine the electron, given quantum tunnelling. Electrons would simply leak out of the nucleus. In addition the relativistic quantum theories available at the time, which would be necessary for such a high energy particle, predicted paradoxically the opaqueness of barriers of height greater than $2mc^2$ (where m is the electron mass).[287]

The second to be realised (by Kronig, 1926)[288] was that of the resulting magnetic moments of the nucleus given the electron's presence. The relative largeness of an electron's magnetic moment would have produced, when accumulated in a nucleus, large hyperfine splitting of spectral lines due to the interaction between nucleus electrons and those orbiting in the neighbourhood. None of these had been observed.

This observation led Kronig to the issue of spin, which we have already mentioned. Kronig had noticed from Nitrogen spectra that on the basis of quantum mechanics the spin of the nucleus had to be 1. But this conflicted with the sum of the spins of its nuclear components. Related to this Rasetti, as we also noted, had in 1928 revealed that Nitrogen must obey Bose-Einstein statistics. Again this was troubling since on the basis of quantum mechanics, odd collection of fermions such as N^{14} when electrons were added, should behave like fermions in total and follow Fermi-Dirac statistics.

Royal Society of London (1927) A114, pp. 243-256; M. Born, W. Heisenberg, P. Jordan, "Zur Quantenmechanik II", *Zeitschrift für Physik* (1926) 35, p. 557.

[287] Discovered by Klein in 1929 on investigation of the Dirac equation. Known as the 'Klein Paradox'. O. Klein "Die reflexion von elektronen an einem potentialsprung nach der relativischen dynamik von Dirac" *Zeitschrift für Physik* (1929) 53, pp. 157-165.

[288] R. de L. Kronig, 'Der Drehimpuls der Stickstoffkerne', *Naturwisssenschaften* (1928) 16, p.335.

All these inconsistencies were derived from applying quantum mechanics to the nucleus on the basis of the *pe*-model of the nucleus.[289] Additionally there was the problem of the continuity of β decay spectra which seemed again to deny the applicability of quantum rules to nuclei elements. This placed theorists in the invidious position of not really having any basis upon which to even begin to talk about the nucleus, since it was not clear at all to what extent quantum mechanics applied. As Gamow wrote in 1929 "The usual ideas of Quantum Mechanics absolutely fail in describing the behaviour of nuclear electrons; it seems they may not even be treated as individual particles."[290] This was the kind of viewpoint that began to emerge. Kronig felt that the components of the nucleus do not retain their identity in the way they do outside the nucleus.[291] As such protons and electrons lose their spin identity, and also its participation in the spin statistics of the nuclei as a whole. Bohr expressed a similar belief in an address in 1931. "The extent to which those constituents of nuclear structure can be treated as independent mechanical entities is, however, far more limited than the possibility of considering nuclei and electrons as separate particles in ordinary problems of atomic constitution."[292] Others however began to suspect that there was something wrong with the *pe*-model of the nucleus. Gamow for instance in his text on nuclear reactions published in 1929, famously sent the editor a manuscript of the text with skulls and crossbones next to every mention of it.[293] Pais believes a Russian by the name of Yakov Gregorievitch Dorfman in 1930 can be cited as the first to directly propose the absence of electrons in the nucleus, but probably on the basis of suggestions flying around from Iwanenko among others that implied that quantum field theory might provide a way to have β decay without permanent electrons in the nucleus.[294]

Otherwise however the physics at work in the nucleus was left mysterious leading theorists like Bohr and Heisenberg began to think more radically about how to proceed. Bohr for instance was concerned by both β decay and nuclear electrons positing that the later "seem to fall entirely outside the field of consistent application of ordinary mechanical concepts, even

[289] Note that the *pe*-model of the nucleus and of the neutron were distinct.
[290] G. Gamow, *Constitution of Atomic Nulcei* (Oxford, Clarendon Press, 1931), p. 5.
[291] R. de L. Kronig, 'Der Drehimpuls der Stickstoffkerne', *Naturwisssenschaften* (1928) 16, p.335.
[292] From a speech given in Rome in October quoted in E. Amaldi, 'From the Discovery of the Neutron to the Discovery of Nuclear Fission', *Physics Reports* (1984) 111 (1 to 4),p. 38.
[293] H. Casimir, *Haphazard Reality* (New York, Harper and Row, 1983), p. 117.
[294] See Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 303.

in their quantum theoretical modification."[295] He posed thus that the disintegration of the nucleus involved the creation of an electron, not the expulsion of something pre-existent, and the conservation of energy criterion had to be abandoned. On the electrons he wrote, "Strictly speaking, we are not even justified in saying that a nucleus contains a definite number of electrons, but only that its negative electrification is equal to a whole number of elementary units, and, in this sense the expulsion of a β ray from a nucleus may be regarded as the creation of an electron as a mechanical entity." As Bromberg notes there was a sense then in which Bohr was also removing electrons from the nucleus.[296] They were certainly not to be treated in any sense as separate dynamical entities with their original properties.[297]

There was however one limited but confusing success. Gamow in 1929 provided a theory of α decay relying on quantum tunnelling which gave correct predictions.[298] This was in fact the first successful application of quantum mechanics to the nucleus. Heisenberg reported though that at the time this was nothing more than 'a lucky case', which gave no real understanding of the nucleus.[299] Conceptually then, the resources available such as quantum mechanical laws, and electron and proton concepts were not providing suitable resources for the description of nuclear structure. In fact they seemed to be constraining it. Hence physicists looked to generate new conceptual frameworks to find a way out of impasse.

However Chadwick's revelation of the neutron changed this. With the neutron concept suddenly theoretical physics had not only a revised ontology for the nucleus, but a new conceptual resource with which to construct it in line with established principles. Soon after Iwanenko wrote a letter to Nature emphasising that the hypothesis should be "very attractive to theoretical physicists," while pointing to a lack of theory of the nuclei in general.[300]

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[295] From a note to the 1927 Como Conference. Quoted in J. Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, p.313.

[296] J. Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, p.307-341.

[297] Heisenberg on the other hand took the classical electron radius, to provide a fundamental length, less than which physical laws as known did not apply. Heisenberg was partially motivated by the problem of electron self-energy which suggested that the energy stored by an electron if treated as a point particle would be infinite. This in turn suggested to him that inside the classical radius of an electron ordinary laws did not apply. But it was also the same dimension of the wavelength of a proton, intimating that length to be the smallest within which the uncertainty relations allowed a particle to be localised. He thus created a lattice theory using this length where systems large with respect to the lattice cells obeyed quantum mechanics, but smaller didn't. W. Heisenberg, 'Die Selbstenergie des Elektrons,' *Zeitschrift für Physik* (1930) 65, pp.4-5

[298] G. Gamow,'Zur Quantentheorie der Atomzertrümmerung', *Zeitschrift für Physik* (1929) 52(7-8), pp.510-515.

[299] Interview with Werner Heisenberg by Joan Bromberg June 16, 1970, quoted in J. Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, p.307-341.

[300] D. Iwanenko, 'The Neutron Hypothesis', *Nature* (1932) 129, pp. 798-798.

Iwanenko in fact posed, probably the first to publicly do so, that the neutron might be fundamental and not composed of electrons and protons, although physicists in general were still working with the *pe*-model at the forefront of their minds. The essential value and conceptual role of the neutron concept regardless of the model of its structure, was that it seemed to provide a platform to bring quantum mechanics into the understanding of the nucleus even if it did contain electrons that were hidden away, and as such the underlying particle could be treated externally like a hydrogen atom as a quantum mechanical particle. For this reason it was picked up as a new conceptual tool for theorising about the nucleus and the forces that operated within it.

Heisenberg, in a series of three papers published in 1932 in the *Zeitschrift für Physik*, employed the neutron concept in just this way.[301] As Bromberg speculates, Heisenberg probably realised right away that with neutron concept one could account for nuclear spins and statistics by assigning it spin ½ .[302] In his first paper Heisenberg emphasises at the start that if the neutron hypothesis is true, 'it means a very considerable simplification of nuclear theory'. His entire three papers then proceed to show how the neutron can be used in conjunction with hypotheses about interactions and quantum mechanical principles to reproduce some of the phenomena of nuclei, if only in a loose qualitative sense. These papers were not intended as complete or final theories. He acknowledges the inconsistencies and tenuous assumptions he has to make to get his results. They were meant merely as starting points for further exploration, detailing in effect how the neutron concept could be used in combination with quantum mechanics to erect a theoretical position on the nuclei. In this sense he illustrates the potential epistemic value of the neutron concept and its usefulness to a theoretical programme of research.

In the first Heisenberg attempts to show that with a proton and neutron model of the hydrogen nucleus, nuclear forces can be accounted for in quantum mechanical terms. Using the analogy of the H_2^+ molecule, Heisenberg poses an exchange force between the proton and the neutron. Exchange forces were seen to arise as a quantum effect from the symmetry or anti-symmetry of wave states for the orbiting electron. If the electron was available to both particles then the electron state would with certain assumptions be linear combinations of the individual states

[301] W. Heisenberg , 'Über den Bau der Atomkerne. I', *Zeitschrift für Physik* (1932) 77(1-2), pp.1-11; Über den Bau der Atomkerne. II', *Zeitschrift für Physik* (1932) 78(3-4) ; Über den Bau der Atomkerne. III', *Zeitschrift für Physik* (1932) 80(9-10).
[302] J. Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, p.332.

for each particle. This implied interaction terms in the Hamiltonian for the average energy of the superpositioned state and thus a force dependent for its direction on the sign (or symmetry) of these interactions. With Heisenberg's assumptions a symmetric superposition, $\varphi = c\ (\varphi_A + \varphi_B)$ (where *A* and *B* are the eigenstates for the two proton-electron systems respectively, and *c* a normalising constant), implies an increase in binding energy, written *J(r)*, which in turn implies an attractive force. Otherwise for an antisymmetric combination the mean energy of the system is reduced by the interaction term implying a repulsion. In terms of an electron, this force can be interpreted as the tunnelling of the electron from the neutron to the proton, swapping both particles. The strength of the interaction is determined by the probability of the electron jumping the barrier. Heisenberg considered however that "it may be more correct to interpret the exchange-integral *J(r)* as a fundamental property of the neutron-proton pair, without reducing it to the movement of electrons."[303] In this regard he built his formalism around a treatment of the neutron as fundamental while suggesting that both protons and neutrons might be simply states of the same particle represented by what was later know as an isospin coordinate $\rho^{\zeta} = \pm 1$ with a set of matrices representing transformation operators. It's not clear whether Heisenberg really took this idea seriously, rather than simply trying to elaborate an efficient formalism for his theory.

Otherwise Heisenberg posited a weaker attractive force between neutrons and just the Coulomb repulsive force between protons. With these he produces a Hamiltonian for a multi-nucleon system from which he deduces qualitative information about the stability of various combinations of protons and neutrons. He has for instance the beginnings of some explanation of when β decay occurs by noting that the energy of a nuclear-system may increase if a neutron is replaced by a proton. He uses his assumptions about the forces in the nucleus thus to give estimates of the form of the mathematical functions that bound stable ratios of protons to neutrons. He fills in the details with the data from experimentation.

What's important to note about this paper is the way in which Heisenberg remains ambiguous or open-ended about what he takes the neutron to be, leaving it open whether it is composed of electrons or protons or not, and thus raising the possibility that the proton-neutron force is something fundamental, not the effect of neutron exchange. As such he builds up a formalism which treats the neutron and proton as particles of different states. At the end of the paper

[303]W. Heisenberg , 'Über den Bau der Atomkerne. I', *Zeitschrift für Physik* (1932) 77(1-2), pp.2. Translated in D.M. Brink, *Nuclear Forces* (Oxford, Pergamon Press, 1965), pp.144-154.

however he prefers to call this an approximation, given that other phenomena like β decay and the Meitner-Hupfeld effect seem to depend on the existence of an electron in the nucleus and thus the *pe*-model of the neutron.[304]

The second article follows in a similar manner to the first, exploring qualitatively using these assumptions the stability of nuclei, but the third makes more direct use of the Hamiltonian structure. Again Heisenberg emphasises that his goal is only to put an hypothesis to "investigate in what way it could put some order in the empirical results." At that he admits the great uncertainty in the state of the data causes great uncertainty in the worth of any theoretical interpretation. Heisenberg calculates using his apparatus and various assumptions the expression of the energy of the nuclear system in its lowest state for a given number of nucleons using the Thomas-Fermi approximation of treating the group as a Fermi gas. However the inevitable collapse of nuclei (i.e an ever increasing density) under this method, prompted Heisenberg to impose a strongly repulsive force between neutron-proton pairs at short range which seemed arbitrary to others (see Majorana below).

The effect of these three papers, although they were mostly speculative and qualitative in many ways, was to immediately raise the status of the neutron concept giving it conceptual roles for understanding the nucleus from the perspective of non-relativistic quantum mechanics. Heisenberg tentatively used the neutron in just this way to erect a nuclear Hamiltonian, and seed various nuclear forces. To an extent he relied on an accepted open-endedness in the concept, switching between a proton-electron model of its structure and a fundamental one to achieve his various goals. This will be important for our discussion of the concept in the next part.

As such the epistemic value of the neutron concept conceptually was widely apparent to theorists at the time. Bohr, who reacted favourably to Heisenberg's work, summed up the situation in a letter to Heisenberg as akin to 'shoving' all the difficulties over onto the neutrons.[305] This was a very pertinent observation, because the problematics of electrons in

[304] The Meitner-Hupfeld was the unexpected large scattering of γ rays upon collision with heavy elements. This originally found its best explanation through bound electrons in nuclei. For a discussion of the apparent need for an electron concept in this respect and the effect on the elaboration of the neutron concept see L.M. Brown and D.F. Moyer, 'Lady or tiger?-The Meitner – Hupfeld effect and Heisenberg's neutron theory', *American Journal of Physics* (1984), 54(2), pp.130-136.
[305] Letter from Bohr to Heisenberg quoted in J. Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, p.336.

the nucleus had been hidden in the structure of the neutron and could be neglected, or rather put-off, generating an open-endedness in terms of the interpretation of its structure which Heisenberg certainly relied upon to use the neutron concept in the way he did. After all if this structure was strictly asserted then it generated problems itself, since the binding energy for the neutron seemed too weak for a proton-electron combination given the masses of each and the mass measured by Chadwick, and of course the spin of the neutron should have been 1, not ½ as Heisenberg assumed.[306] In this sense Heisenberg reserved the neutron concept as an object itself for investigation but which had tremendous conceptual and explanatory value despite the uncertainties and inconsistencies that arose from treating it this way.

Heisenberg didn't though achieve any major prediction or successes with his use of the neutron concept, although his systematics impressed Bohr. But he did through his use of it help build the neutron concept into a research program applying quantum mechanics to the nucleus. This becomes its significant and substantial epistemic use in the 30's as we'll see and perhaps defines the importance of the neutron concept historically. It is indeed how scientists themselves look back on its discovery when they express sentiments of the kind that 'the discovery of the neutron made for the first time a rational theory of nuclear phenomena possible'[307], or the neutron was '..a focal point from which subsequent developments proceeded.'[308] Subsequent theoretical investigations of the nucleus follow Heisenberg's use of the neutron, but refine and readjust their conception of it, and of the nuclear forces involved. As such Heisenberg's use of the neutron should be understood as setting down the conceptual and explanatory roles for the neutron concept that resourced the theoretical research process for the purpose of understanding nuclear structure and nuclear forces. From this point on physicists had a conceptual resource to develop theories of the nucleus. Exploring these roles and how they were relied upon in its historical development is the subject of the next section.

4.3 Framing a Research Process: The Neutron Concept's role in Nuclear Research of the 30's

It's important to emphasise at this point the mere advent of the neutron concept did not in itself solve all problems or explain huge numbers of unexplained phenomena. These types of

[306] According to Bromberg, Heisenberg was aware of both these problems. *Ibid,* p.337.
[307] Quoted above.
[308] Devons 'Comments to the Paper of E. Segré', *Proceedings of the Xth International Congress of the History of Science* (Itaca, 1962), pp. 155-156.

developments are relatively rare in science (perhaps theories of relativity might be good examples). Rather it solved immediately a few problems, notably the composition of radiation from Beryllium and the problem of the electrons in the nucleus, when applied in the experimental and theoretical context respectively, but the advent and role of the neutron concept cannot be evaluated strictly in such terms. Its ontological assertion did not immediately change the situation confronting physicists, that of the complexity and uncertainty of the nucleus and its properties. As already noted the neutron concept created its own problems, such as that of its own structure. In this sense one can't by any means characterise it for scientists as some kind of ontological revelation that signalled an end-point of their investigations. That's not how it was in context appreciated. As a discovery it was simply a partial piece of a complex picture that physicists knew they were far from understanding. Thus it was recognised by physicists at the time principally as a resource towards furthering the goal of understanding the nature of matter and the laws at work at the quantum level. This is why histories of neutron concept tend almost always, even if overloaded with hindsight, to emphasise the epistemic value of it, rather than ontological success itself. They discuss the history as though the neutron was more or less a critical stepping stone to understanding the nucleus and indeed particle physics itself. To the physicists at the time of course like Chadwick, Iwanenko and Heisenberg, they didn't conceive that the concept would of course necessarily give them such insight, what was clear however is that they had a concept in their possession that could be used experimentally and conceptually to approach the nucleus and nuclear phenomena, which given its assumed basis in reality, could provide them with a reliable starting point to producing more nuclear phenomena through its causal-experimental uses and to bettering their understanding with the aid of this new phenomena and the neutron concept's own conceptual and explanatory possibilities.

The result is that the neutron concept through its various epistemic roles became quickly essential to the developing field of nuclear research. That's what we want to document here in the last part of this chapter; exploring the way in which research choices and research developments were dependent upon it, but at the same time how the concept itself as a subject of this research depended on the research process, this seeming inconsistency reconciled by scientists' methodologically interpretation of the neutron concept and its relations in an open-ended way, treating it as an epistemic frame for the research process, rather than tying it essentially to one description or set of beliefs about it.

It becomes somewhat easier to countenance such ideas when it is realised that in the theorising of the 1930's many of the principles developed and postulates asserted were wrong, or only approximate at best and quickly were realised as flawed. As such the lifetime of viewpoints about nuclear forces and nuclear processes were short. At the same time new phenomena and data kept arising in experimentation as the process of research carried on that constantly demanded new theoretical approaches. In this environment, where the object under study was apprehended as complex the system had to be adaptive in its use of concepts and thus physicists had to rely on their central concepts (like particle concepts) in an open-way that allowed their reformulation. The methodology applied to the use of the neutron concept is strongly indicative of this.

4.3.1 The Solvay Conference 1933

The attachment of the neutron concept to the context of nuclear research is first apparent from a discussion at the Royal Society involving Chadwick, Rutherford, Ellis and others. The discussion was entitled 'Discussion on the Structure of Atomic Nuclei', and within this context the neutron was discussed, i.e. with respect to nuclear structure.[309] Rutherford announced at the start that 'so far it has not been found possible on the theory to give a detailed picture of the structure of a nucleus,' putting it that, "If the neutron hypothesis be confirmed by experiment, it will obviously much influence our conception of the formation and constitution of nuclei." The discussion coming not long after Chadwick's results doesn't do much more than this with the neutron concept except in this way to suggest its future roles in any theory of the nucleus. But the discussion did extend to the problematic elements of invoking the neutron concept, with Lindemann questioning how it could exist at all given quantum theory on the basis of its presumed proton-electron combination.

However by the time of the 7th Solvay conference of 1933 it's clear that the neutron concept had become strongly associated with the task of understanding the nucleus.[310] The conference itself had been given the designated topic of 'The Structure and Properties of Atomic Nuclei' partially as a result of the neutron's discovery. It covered however a broad range of topics,

[309] Rutherford et al., 'Discussion on the Structure of Atomic Nuclei', Proceedings of the Royal Society (1932) 136, pp. 735-762.
[310] Paper and discussions published, W. Heisenberg, *Structure et Proprietes de Noyaux Atomiques: Rapports et Discussions du Septième Conseil de Physique* (Gauthier-Villars, Paris, 1934). Also summarised in English J. Mehra, *The Solvay Conferences on Physics* (Reidel, Dordrecht, 1975), pp.207-226.

summarising more or less the research frontier of nuclear physics. In this context Chadwick put his arguments for the neutron and the measurement of its mass, and then his achievements with it so far, including his experimental results on proton-neutron collisions, which were acknowledged as vital for the understanding of nuclear structure. Chadwick gave himself on the basis of wave mechanics a value for the cross-section *Q* of this collision.[311] Gamow discussed the origin of γ rays and nuclear energy levels incorporating the neutron into his modelling, citing nuclear spin and the problems of the proton-electron model as a reason. On the basis of this new assumption he was able to produce a binding energy curve with respect to atomic mass and the general shape of the nuclear potential. Gamow presented his liquid drop model of the nucleus to explain the independence of the average density of nuclei from their radii and the saturation of nuclei as that of a closed liquid drop. Heisenberg gave a report and directed discussion regarding the understanding of nuclear forces, applying the neutron concept as he had in his original papers, but also considering the results of Majorana we mention below.

In this context different models of the nucleus, and the structure of the neutron, were open to discussion and consideration (e.g. either as a proton-electron combination or as fundamental), as well as the types of forces that could be operating and the interactions between neutron and neutron, and proton and proton. These were all considered on a hypothetical open basis as aspects of future research and consideration. In fact the structure of the neutron itself was discussed extensively with Chadwick giving arguments both for and against the *pe*-model of the neutron, while Heisenberg tended towards an elementary account on the basis of the small mass defect of the neutron so far observed. Heisenberg's report concluded by discussing the application of the proton-neutron nuclear model to questions of mass defect and stability, diffusion. As such in these ways the neutron concept was linked by these physicists to the issue of the nucleus, as an ongoing investigation, for which the existing concepts like the neutron concept were open to discussion in terms of its structure and relations. As Brown and Rechenberg put it, with Heisenberg's report 'the subject of nuclear structure physics was fairly launched,' the major outstanding question being, which itself concerned neutron structure, the problem of β decay.[312]

[311] In nuclear and particle physics, the concept of a cross section is used to express the likelihood of interaction between particles.
[312] L.M. Brown and H. Rechenberg, 'Nuclear Structure and Beta Decay (1932-1933)', *American Journal of Physics* (1988) 56(11), pp. 982- 988; p. 986.

This conference is important for us, because it shows us that the neutron concept had become quickly embedded in a research process concerning the 'structure and properties of atomic nuclei', and this was the over-arching purpose and context which guided the use and discussion the neutron. Indeed one of the motivating principles listed for the conference stated that "One of the first tasks of theory in the nuclear domain is to determine as precisely as possible a limit to the possibility of applying quantum mechanics."[313] In this context the neutron was as already mentioned extremely important and the discussion of nuclear structure was dominated by this question of how neutrons could facilitate a quantum mechanical treatment.

4.3.2 The Experimental Context

The degree to which the neutron concept influenced the development of experimental nuclear physics after 1932 is highly significant. It was through the 30's in many ways the chief epistemic tool for the fundamental goal of understanding nuclear phenomena, nuclear reactions and nuclear structure itself. As Gregorio puts it, "all the main laboratories then active in nuclear physics were reorganised after the discovery of the neutron."[314] These included the laboratories in Cavendish, Joliot and Curie's, and Bothe and Becker's. Further new laboratories arose in part motivated by the neutron (Fermi's for instance).

The neutron concept itself promoted its use in this respect by having properties that suggested its value in providing insight into the nucleus, and that were in fact constitutive of its experimental roles. The first was that as part of the nucleus itself its use in experiments should be able to generate information about nuclear forces. Scattering experiments, particularly of neutron against protons, had this in mind. To quote Chadwick, "The interaction between a neutron and a proton is the most significant for the structure of a nucleus and govern its stability".[315] At this time short-range forces between neutrons themselves and between protons, were considered likely to be different from that between a neutron and proton. Secondly, as Chadwick reemphasised in his Bakerian lecture on the neutron in 1933, on the basis of its neutrality, "the most obvious properties of the neutron are its ability to set in

[313] W. Heisenberg, *Structure et Proprietes de Noyaux Atomiques: Rapports et Discussions du Septième Conseil de Physique* (Gauthier-Villars, Paris, 1934), p.289.
[314] Alberto G. De Gregorio, 'Neutron Physics in the Early 1930's', *Historical Studies in the Physical and Biological Sciences* (2005) 35(2), pp. 293–340; p.3.
[315] J. Chadwick, 'The Bakerian Lecture: The Neutron', *Proceedings of the Royal Society* (1933) A142, pp.1-25; p.21.

motion the atoms of matter through which it passes and its great penetrating power."[316] Of course these properties were precisely the ones of interest to experimenters, who saw the neutron as a tool capable through its properties of producing new phenomena through its interactions with matter. Such phenomena of course would be themselves essential to an eventual understanding of the nucleus and nuclear reactions. At the same time the neutron itself was a puzzle to experimenters. Calculations of its mass varied, and in fact generated a controversy, with different pictures of its structure resulting (see below). Working out the properties of the neutron was thus itself a subject of investigation. As such the neutron concept played an epistemic role in experiment as a tool of investigation, including into the nature of its underlying entity. Precisely how these roles manifested themselves in the experimental programs that developed, we discuss here.

As mentioned already much of what experimenters set out to use the neutron concept to do, was to generate novel phenomena by testing out the causal powers and interactions of the neutron to the end of understanding nuclear structure. As Chadwick expresses it, "the nature and properties of the neutron are of interest not only because of their novelty but because the neutron is probably a very important unit in the structure of matter."[317] Hence they were guided in their use of the neutron concept by what seemed germane to this purpose.

The first experiments in this regard were happening at the same time Chadwick was announcing his crucial ones in 1932. As already mentioned Feather was testing out the ability of neutrons to disintegrate a nucleus. Feather's experiments showed that a neutron could cause the ejection of an α particle from the nucleus.[318] Disintegration produced this way proved extremely complex however and independent of the only other known type, α particle disintegration. As such Feather scheduled for himself an investigative process with the neutron with regard disintegration experiments declaring "it is the writer's hope to undertake in the near future, the necessary investigations which will enable us to study as many cases as possible of the artificial disintegration of nuclei under neutron bombardment."

[316] *Ibid*, p1.
[317] *Ibid*, p2.
[318] N. Feather, 'The Collisions of Neutrons with Nitrogen Nuclei', *Proceedings of the Royal Society* (1932) A136, pp. 709 – 727.

Likewise the scattering of neutrons by protons was part of the very first work taken up by Chadwick himself.[319] Such experiments had shown that the forces acting between the two could not be basic coulomb forces (assuming the neutron a small electric dipole) but had to be a novel kind of force. The collision radius was greater than expected on the Coloumb prediction and strangely seemed to increase as neutrons slowed down. Dunning expanded on this work in thorough detail with the express aim, 'to investigate the emission and properties of the neutrons.'[320] He reached the same result on the basis of his experimental work that the forces at work in the nucleus couldn't be described by any known force.

At the *Institut du radium* the Curies applied their laboratory intensively to investigate neutron interactions. They showed quite early that they had been partially right about Beryllium disintegration. Neutrons released were accompanied by γ rays and didn't just emerge alone. By February 1933 Curie and Joliot were beginning to observe symmetries in transmutation processes. For instance they discovered a collection of elements of 4n + 3 nucleons that produced protons or neutrons and positrons in α particle disintegrations. [321] This led them to suppose that in fact it was protons that were complex, not neutrons, being composed of a neutron and positron, an assertion which later fed into the uncertainty over neutron mass which we discuss later. Symmetries were also being assembled by Americans Harkins, Gans and Newson at the University of Chicago.[322] They followed Feather applying the neutron to disintegrate fluorine, neon and oxygen. The products of these were known, except for that of fluorine. F^{19} had been discovered to be involved in (α,p), (α,n) and (p,α) reactions. It was hypothesised on the basis of these that an (n,α) relation ought to exist as well, even though it would involve an end product N^{16} thought not to stably exist, and which remained hypothetical. But symmetry considerations implied it did, and Fermi in fact relied on this reaction to explain his results on neutron-induced artificial radioactivity.

This discovery of neutron-caused artificial radioactivity was in fact the most important use of the neutron concept in the 30's for in fact generating enormous amounts of research. The discovery of artificial radioactivity by α particles had already been made in 1934 by Curie and

[319] J. Chadwick, 'The Bakerian Lecture: The Neutron', *Proceedings of the Royal Society* (1933) A142, pp.1-25.
[320] J. R. Dunning, ‚The Emission and Scattering of Neutrons,' *Physical Review* (1934) 45, pp.586- 600, p. 586.
[321] I. Curie and F. Joliot, 'Sur les conditions d'emission des neutrons par action des particules α sur les éléments légers', *Comptes Rendus de l'Academie des Sciences* (1933) 194, pp. 397-399.
[322] W.D. Harkins, D.M. Gans, and H.W: Newson, 'The Disintegration of the Nuclei of Nitrogen and other light Atoms by Neutrons. I', *Physical Review* (1933) 44, pp.529-537.

Joliot, a discovery for which they received the Nobel Prize for Chemistry.[323] About a month later, it was observed also for protons by Cockcroft, Gilbert and Watson.[324] Fermi however made the discovery that neutrons could produce artificial radioactivity, which significantly embedded their value as experimental tools, neutrons having distinct advantages as nuclear projectiles over other particles.[325] However at the time this wasn't realised because the neutron sources available were extremely weak. Fermi after testing many elements with negative results, finally had success with fluorine and aluminium, from which he worked to assemble positive findings on a huge range of other elements.[326] This causal property of neutrons seemed immediately to have enormous potential application, which physicists foresaw by virtue of the simple effectiveness of neutrons in causing such reactions (over α's and protons). As neutral non-ionising particles they were better able to reach nuclei and not lose energy through ionisation, and their cross-section as already noted was larger given its immunity to the electric-potential barrier of nuclei. Immediately Fermi's group began to systemically assemble results for all elements of the periodic table. As Amaldi, a member of Fermi's group at the time expresses, "we had found that all elements, whatever their atomic weight, can be activated by neutrons. The nucleus product is sometimes an isotope of the target nucleus, on other occasions, it has an atomic number lower by one or two units."[327] Light elements differed from heavy elements in this respect, likely to have the later rather than the former outcome. This was explained by heavier nuclei having a high potential from which particles would have to escape, thus retaining their contents and absorbing the additional neutron.

There's a degree of dispute over why Fermi suddenly decided to try to produce the same effects with neutrons, since his original plans were simply to repeat the experiments of Joliot and Curie. Segré puts it that straight after Fermi heard of the discovery of artificial radioactivity he 'saw that their work could be expanded tremendously by using neutrons as

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[323] I. Curie and F. Joliot, 'Production artificielle d'éléments radioactifs. Preuve chimique de la transmutation des elements', *J. Phys. Radium* (1934) 5(4), pp.153-156.
[324] J.D.Cockcroft, C.W. Gilbert, and E.T.S. Walton, 'Production of Induced Radioactivity by Protons,' *Nature* (1934) 133, p.328.
[325] E. Fermi, 'Radioattività indotta da bombardamento di neutroni', *La Ricera Scientifica* (1934) 5(1), p.283.
[326] An account of Fermi's discovery process is given by Alberto G. De Gregorio, 'Neutron Physics in the Early 1930's', *Historical Studies in the Physical and Biological Sciences* (2005) 35(2), pp. 293–340 and E. Amaldi, 'From the Discovery of the Neutron to the Discovery of Nuclear Fission', *Physics Reports* (1984) 111 (1 to 4).
[327] E. Amaldi, 'From the Discovery of the Neutron to the Discovery of Nuclear Fission', *Physics Reports* (1984) 111 (1 to 4), p. 128.

projectiles.'[328] In other words he was thinking epistemically about the potential applications of the neutron concept. But others doubt that the decision was really made so suddenly. Guerra, Leone, and Robotto, believe he was motivated by his own β decay theory, itself based on Landé's theory of nuclear structure, which predicted that an excess of neutrons in the nucleus would decay.[329] Hence overloading a nucleus should naturally cause a decay reaction of this kind. In any case it is clear Fermi was thinking deeply about the neutron concept and how it could be employed experimentally. In his Nobel lecture in 1938 Fermi emphasised that he had been thinking about the ability of the neutron to overcome the potential barrier, despite the low numbers of the term emitted by sources, and their long range, making the probability of nuclear collision very high.[330] Plus the experiments of 1932 and 33 had already indicated that the neutron was 'an efficient agent for producing some nuclear reactions.' Others had doubted the experiments because of the relative sparseness of neutrons.[331] Fermi had overcome this problem with a relatively intense source of neutrons, replacing the traditional Polonium-Beryllium source with a Radium-Beryllium source.

As a result of these experiments Fermi's entire laboratory in Rome was organised around the neutron concept on the essential basis that neutrons would prove the most efficient nuclear reaction agents which would thus produce phenomena relevant for understanding the nucleus but also the properties of neutrons themselves.[332] Fermi's intuition was on display again not long after when he identified a class of neutrons, that were henceforth known as 'slow neutrons', to be responsible for strong radioactivity produced in silver when the apparatus was

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[328] Quoted in Alberto G. De Gregorio, 'Neutron Physics in the Early 1930's', *Historical Studies in the Physical and Biological Sciences* (2005) 35(2), p 325.

[329] G. Acocella, F. Guerra, and N. Robotti, 'Enrico Fermi's Discovery of Neutron-Induced Radioactivity: the Recovery of his first Laboratory Notebook', *Physics in Perspective* (2004) 6, pp.29-41.

[330] E. Fermi, *Les Prix Nobel* (1938).

[331] For instance Frisch recalled, "I remember that my reaction and probably that of many others was that Fermi's was really a silly experiment because neutrons were much fewer than alpha particles. What that simple argument overlooked was that they are much more effective." Quoted in Gerald Holton, 'Striking Gold in Science: Fermi's Group and the Recapture of Italy's Place in Physics', in G. Holton, *The Scientific Imagination* (Cambridge, Cambridge University Press, Part, 1978), p.173.

[332] Corbino the director of Physics Institute at the University of Rome gave a speech in 1929 announcing the shift in the direction of research in Rome to be governed by Fermi. The general goal was the re-establishment of Italian science at the forefront of empirical research. But it's clear the epistemic goals driving this program were guiding Fermi. As Corbino puts it "the only possibility of great new discoveries lies in the chance that one might be able to identify the internal nucleus of the atom." In fact Corbino asserts that it is only in experimental research into the nucleus and nuclear transformations that there is still the possibility of the real discovery of novel phenomena. "One can therefore conclude that while great progress in experimental physics in its ordinary domain is unlikely, many possibilities are open in attacking the atomic nucleus. This is the most attractive field for future physicists." Quoted in Gerald Holton, 'Striking Gold in Science: Fermi's Group and the Recapture of Italy's Place in Physics', in G. Holton, *The Scientific Imagination* (Cambridge, Cambridge University Press, Part, 1978), p.168.

placed on a wooden table rather than marble one.[333] Fermi did the same experiment but placed paraffin in front of the neutron source witnessing the same effect. He and his laboratory members thus realised that neutrons, when slowed drastically to almost thermal temperatures, raised their cross-sections of interaction extremely significantly. This ran counter of course to established preconceptions, which took cross-sections to be proportional to velocity, as in the case of protons, deuterons and α particles. The neutron concept was thus at the centre of the production of a surprising new phenomenon, and became the starting point of an entire new direction of research into nuclear structure, serving to develop and elaborate its experimental role in this regard, with a set of new experimental uses.

Again for the laboratory members the immediate issues were epistemic: how could this be used? As Amaldi puts it, ‘Immediately after the discovery of the effect of hydrogeneous substances, we started to talk about possible practical application’.[334] Theoretically, Fermi’s group began to thoroughly investigate the properties of slow neutrons and their reactions, including their cross-sections, scattering lengths, diffusions, energy, absorption and so on across different elements. From this work in 1935 and 1936 Fermi’s team (and others around the world) became aware of the selective absorption of neutrons of different energy levels and resonances.[335] Groups of neutrons were labelled by their ability to be absorbed by particular elements. These groups were then more closely analysed and experimented with. It seemed that this phenomenon could not be explained by scattering laws in themselves. It was given an explanation in 1936 by Bohr as due to resonance within a liquid drop model of the nucleus, caused by the neutron creating stable oscillations until eventually one of the particles emerged or a photon carried the energy away.[336] At the same time Breit and Wigner provided the theoretical apparatus for neutron cross sections as a function of neutron energy for single resonance levels.[337] Thus out of these neutron studies the first successful theories of nuclear energy levels emerged. “The ideas of Bohr and the one-level formula of Breit and Wigner

[333] This is an historical episode that has been thoroughly elaborated in many accounts. See for instance G. Acocella, F. Guerra, and N. Robotti, ‘Enrico Fermi’s Discovery of Neutron-Induced Radioactivity: the Recovery of his first Laboratory Notebook’, *Physics in Perspective* (2004) 6, pp.29-41; Gerald Holton, ‘Striking Gold in Science: Fermi’s Group and the Recapture of Italy’s Place in Physics’, in G. Holton, *The Scientific Imagination* (Cambridge, Cambridge University Press, Part, 1978); E. Amaldi, ‘From the discovery of the neutron to the discovery of nuclear fission’, *Physics Report* (1984) 111(1-4), pp. 1 – 331; Alberto G. De Gregorio, ‘Neutron Physics in the Early 1930’s’, *Historical Studies in the Physical and Biological Sciences* (2005) 35(2), pp. 293–340; E. Segré, ‘Fermi and Neutron Physics’, *Reviews of Modern Physics* (1955) 27(3), pp.257-263,

[334] E. Amaldi, ‘From the discovery of the neutron to the discovery of nuclear fission’, *Physics Report* (1984) 111(1-4), p. 154.

[335] See *Ibid*, chapter 5 and 6.

[336] N. Bohr, ‘Neutron Capture and Nuclear Constitution’, *Nature* (1936) pp.344-348.

[337] G. Breit and E. Wigner, ‘Capture of Slow Neutrons’, *Physical Review* (1936) 49, pp. 519-531.

were immediately accepted by all the physicists and remained for a number of years the guiding principles on which all nuclear processes were interpreted."[338] Experimenters themselves took up the task of finding the parameter values for the equations of these models.

At the same time the systematic study of radioactivity produced by neutrons by Fermi's groups had revealed strange but complex behaviour in Uranium and Thorium.[339] The products from these reactions were highly energetic but were difficult to identify, and didn't seem to be other Uranium or Thorium isotopes. An immense amount of work involving Fermi's group, Curie, and Hahn, Meitner and Strassman, among others, worked on identifying chemically the products and via their decay rates. The assumption was that the products should be radium, which would be the end result of two α decays from Uranium. Hahn however found that one of the Uranium products was Barium, an element much lighter than Uranium but much larger than an α particle, implying that the nucleus had actually been split. This was an extremely surprising since until then no one had witnessed any products emerging from a nuclear reaction larger than an α particle.[340] Hahn and Strassman published these results at the end of 1938.[341] However there was a theoretical basis available to support this hypothesis in the form of the liquid drop model which was realised not long after.[342] The neutron could cause a vibration in the atoms which then led to motions that eventually split it apart. The positive charge on the uranium nucleus was powerful enough it seemed to break the considerable surface tension holding the nucleus together. The energy released could be accounted for by mass deficit calculations. The nuclei products were in fact lighter than the nucleus itself. The significance of the result was immediately widely realised and in January 1939 a Columbia group involving Fermi ran the first nuclear fission experiment. When Paris scientists (Joliot and Curie's group) identified that secondary neutrons were released also in the nuclear

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[338] E. Amaldi, 'From the discovery of the neutron to the discovery of nuclear fission', *Physics Report* (1984) 111(1-4), p. 196.

[339] Fermi's group reported this in a letter to the Editor of La Ricerca Scientifica on the 10 May 1934. E. Amaldi, 0. D'Agostino, E. Fermi, F. Rasetti and E. Segrè, 'Radioattività beta provocata da bombardamento di neutroni III,' Ric. Scient. (1934) 5(1), pp.452—453.

[340] The German chemist had Ida Noddack had originally proposed the idea of nuclear splitting in 1934 in the journal *Angewandte Chemie* (1934) 47, stating; "It is conceivable that when heavy nuclei are bombarded with neutrons these nuclei could break down into several fairly large fragments, which are certainly isotopes of known elements, but not neighbours of the irradiated elements." However as Amaldi reports there was a reluctance to take this idea seriously by anyone in the field since it involved a completely new type of nuclear reaction. We might also speculate that they weren't convinced enough by the lack of a causal mechanism for such a phenomenon. It seemed too speculative. E. Amaldi, 'From the discovery of the neutron to the discovery of nuclear fission', *Physics Report* (1984) 111(1-4), p. 277.

[341] O. Hahn and F. Strassmann, ‚*Über den Nachweis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen entstehenden Erdalkalimetalle*', *Naturwissenschaften* (1939) 27(1), pp. 11-15.

[342] N. Bohr and J.A. Wheeler, 'The mechanism of nuclear fission', Physical Review (1939) 56, pp. 426—450.

reaction, they introduced for the first time into nuclear physics the idea of a nuclear chain reaction and the idea of a nuclear reactor to control the process began to be developed.[343]

In some sense then the neutron concept did what was expected of it in the experimental context. It enabled the production of new phenomena, that themselves created investigative pathways and opened-up new windows onto the nucleus and nuclear processes. No experimenter anticipated what would happen of course, but they did use the neutron concept by virtue of the penetrative properties stemming from its neutrality with the aim of producing responses and phenomena from the nucleus and nuclear forces which would it was hoped clarify the nature of the nucleus and the neutron itself. In fact it exposed more complexity than was anticipated. As Arabatzis would put it the concepts such as the neutron and nucleus were not completely under the control of the scientists using them and had a life of their own. But the experimenters nonetheless were prepared to find new things, and used the neutron concept as an experimental tool with this explicit aim, as an investigative tool. In this sense the epistemic role of the neutron in this research comes to the fore as an essential part of experimental investigation into the nucleus.

Part of this as we've seen in the events detailed above, was the motivation of gaining more information about the neutron itself and its behaviours (scattering, dispersion etc). Of course building up a more sophisticated picture of the neutron was a prerequisite for fathoming nuclear processes, and nuclear structure, which the neutron was inherently part of. Questions about the neutron translated generally to fathoming some aspect like its field structure which would inform on the construction of the nucleus itself. Testing the resonance properties of slow neutrons for instance was interrelated with the question of nuclear structure and interactions. One important aspect of the understanding of the neutron itself was the measurement of its mass, a task that was confined to experimentation. As it turned out the question of its mass was interrelated with the question of its structure, which was itself related to ways in which explanations of phenomena like the Meitner-Hupfeld effect and β decay could be conceived. The neutron's mass as such became an investigative issue tied in with the investigation of nuclear processes. It in fact proved controversial and remained an open issue in the mid 30's. We already saw that Chadwick gave an estimate of the neutron's mass in his original paper to *Nature* at 1.0067 u, with an error between 1.005 and 1.008, using principally

[343] H. von Halban, F. Joliot and L. Kowarski, 'Liberation of Neutrons in the Nuclear Explosion of Uranium', *Nature* (1939) 143, pp. 470—71.

the measured masses of the products and reactants of Boron disintegration. This fit with a proton/electron model of the nucleus reserving some difference in mass for binding energy, although a decidedly low value. Nonetheless as Stuewer asserts Chadwick's mass measurement was generally accepted by the middle of 1933.[344]

However in June 1933 it was challenged by Ernest Lawrence who fired deuterons (a proton, neutron nucleus) at various elements producing protons as a result.[345] Measuring the energies gave a mass of neutron at about 1.0006 u, lower than Chadwick's. This relied on what turned out later to be a false assumption that the deuteron was splitting up into a neutron and proton. In July, Jean Perrin presented a note from Curie and Joliot to the Académie des Sciences questioning Chadwick's measurement by positing that the α's were not interacting with B^{11} in Chadwick's Boron sample, but with B^{10}.[346] On this assumption the products would be different, either a proton, or if a neutron had emerged as measured, a neutron and a positron. This gave on comparison of the mass-energy balances a mass for the neutron of 1.011 (later corrected to 1.012), and clearly posited that the proton, not the neutron was the complex particle. In favour of this result, was the fact that it produced a strong binding energy for the proton.[347] At the Solvay conference in 1933 these different values manifested themselves in a debate which served to open up the whole issue. The Curie and Joliot questioned whether Chadwick had taken into account energy lost in the form of γ rays during the nuclear reaction, presenting photographic evidence of positron-electron pairs.[348]

In the context of this debate neutron mass remained an unsettled question, and an issue of investigation. By the middle of 1934 however Lawrence recognised that his results were due to contamination in his experiment, and the neutrons he found had come from a different reaction than he thought. Scientists running the same tests under more controlled environments had in fact failed to find any evidence of neutrons at all emerging from the reaction.[349] But the high value of Joliot and Curie, and Chadwick's own, remained with experimental evidence on both sides. Certainly the issue prompted experimentation to try to

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344 R.H. Stuewer, 'Mass-Energy and the Neutron in the Early Thirties', Science in Context (1993) 16, pp.195-238.

345 M.S. Livingston, M.C. Henderson, and E. O. Lawrence, 'Neutrons from Deutons and the Mass of the Neutron', *Physical Review* (1933) 44, pp. 781 – 782.

346 Cited in R.H. Stuewer, 'Mass-Energy and the Neutron in the Early Thirties', Science in Context (1993) 16, p. 208.

347 In addition the revised neutron mass gave a stable mass for the Be^9 isotope, which seemed unstable on the old calculation.

348 J. Mehra, *The Solvay Conferences* (Reidel, Dordrecht, 1975), p.217.

349 See R.H. Stuewer, 'Mass-Energy and the Neutron in the Early Thirties', Science in Context (1993) 16, pp.195-238.

decide the issue. The decisive resolution came from an idea of a Cavendish experimenter Maurice Goldhaber, that photo-disintegrating the deuteron would give a highly accurate measurement. Chadwick took up this idea and measured the mass at 1.0081 u. The results were published in August 1934.[350] The result in fact was completely surprising to Chadwick and his expectations but he immediately changed his viewpoint. It ruled out the complex nature of the neutron. With Fermi's theory of β decay relying on a fundamental notion of the neutron, the need for a complex neutron dissipated. In fact a higher mass for the neutron than the hydrogen atom explained why a neutron decayed in a proton and electron. Measurements nonetheless continued to be made throughout the rest of the 30s to greater precision eventually settling on 1.00893 u from Bethe.[351] All these measurements relied on the neutron's conceptual role as an element of the nucleus for composing nuclear structure, by being able to use this starting point, to work backwards to deduce the neutron's mass from known prior masses and energies.

When we envisage then the epistemic roles of the neutron concept, we see that the measurements of its own mass and identification of its own structure were thus a significant part of the issues for experimental investigation the concept threw up in the 30's. Employing a particle concept implied the need for a mass measurement, which implied the need for experimental means of deriving it by relying on what was known of the concept, the explanatory roles that identified it, and in what contexts it appeared. In such a way the concept could be said to have played a part in its own investigation. As it turned the structure of the neutron itself was dependent also upon this investigation.

In sum then what we have here is some insight into the role the neutron concept played in the experimental side of nuclear research. These notes here don't encompass everything that was going on with the concept by any means, but are merely meant to illustrate the point that in the experimental context it played central epistemic roles as an experimental tool and by virtue, as an investigative tool, for which the investigation of its own properties was an active and interrelated part. Hence understanding how experimenters approached the uncertainty and complexity of nuclear processes as understood in the 30's is dependent upon understanding the epistemic roles the neutron concept was providing them. In this respect the perceived properties of the neutron made it the principal tool for producing novel phenomena through its neutrality and subjection to nuclear forces that would themselves open up fruitful

[350] J. Chadwick and M.Goldhaber, 'A'Nuclear Photo-effect': Disintegration of the Diplon by γ-Rays', *Nature* (1934) 134, pp.237-38
[351] H.A. Bethe, 'Masses of Light Atoms from Transmutation Data', *Physical Review* (1935) 47, pp. 633-34.

investigative paths towards a deeper understanding of the laws at work in nuclear reactions and nuclear structure. These are the expectations that guided experimenters in the use of the neutron concept and thus how it was built in to the centre of a continuing and developing research process. As it was this process led to starting points for many practical applications after 1939 that themselves demanded a better understanding of nuclear processes in order to perfect…most notably of course nuclear fission reactors and atomic weapons.

4.3.3 The Theoretical Context

At the hands of theoretical physicists different epistemic uses of the neutron concept were emphasised, notably its explanatory and conceptual possibilities. Theory itself responded to the advent of the neutron more slowly at least at the outset. Some of this was no doubt due to the complexity at hand. As already mentioned the history of theory in this period with respect to nuclear process and nuclear forces is a history of error and approximation. Most theoretical pictures presented aimed simply at giving models for limited circumscribed phenomena. What is characteristic of it is the underlying methodology that accompanied it of the postulation and provisionality with which theorists presented their work and accepted its quick obsolescence. As such the various theoretical strictures applied to the neutron concept and other particle concepts changed significantly and continuously. Nevertheless by April 1936 enough theory had been produced on nuclear physics to fill a 200 page summary by Bethe and Bacher[352]. This summary provided a survey of theoretical attempts at describing nuclear forces and the quantum-mechanical scattering of protons and neutrons, as a theory of the states of isotopes of hydrogen and helium, the structure of heavier nuclei and the shell model, the application of quantum field theory to β decay and a discussion of the magnetic interactions between nuclei and their orbiting electrons. This work was of course extremely complex, but the complexities of it are not strictly necessary to my elaboration here. What I want discuss here is how the neutron concept itself provided through its epistemic roles a resource on which these variable theoretical processes relied.

What enabled Bethe and Bacher to fill these pages was principally Heisenberg’s use of the neutron concept to apply quantum mechanics to the nucleus and nuclear processes. Its properties such as its energy, mass, spin, and magnetic moment were applied to quantum mechanical equations. Conceptually it was used by theorists to think about how to construct

[352] H.A. Bethe and R.F. Bacher, ‘Nuclear Physics: Stationary States of Nuclei’, *Reviews of Modern Physics* (1936) 8, pp.82-260. This seems actually to have been the first part of the two part series, the second dealing with the dynamics of the nuclei and transmutations. As far as I know this was never published.

different sized nuclei, and to understand weak radioactive processes such as β decay. At the same time other properties associated with it, particularly relational ones, such as its cross-section, and the forces between it and other nucleons drove theoretical investigation. In all this thinking and investigation the neutron concept provided a relatively flexible resource for the imposition of new ideas that were arriving with quantum field theory and particle theories of force, on the basis of its conceptual and explanatory roles as a neutrally charged unit of nuclear structure and nuclear force subject to quantum mechanical laws.

4.3.3.1 Nuclear Forces and Quantum Mechanics

As already mentioned Heisenberg established his speculations on nuclear forces using the neutron concept in this way as a theoretical starting point, illustrating broadly a new way in which the nucleus could be dealt with. It was realised that this force not only had to have short range but that it probably didn't conform to any classical notion of force that was available. "The forces between neutron and proton are an entirely new phenomenon, not connected in any way with the forces familiar in atomic physics…"[353] As such the nature of these forces was an open theoretical issue, to be built necessarily with the help of the neutron concept and what was thought of the neutron itself. This path of research as it turned out involved continually changing ideas, in response to experimental evidence and the provisionary and approximate nature of the theoretical responses. Ideas that were rejected like Heisenberg's isospin, later re-emerged. Physicists were thus dealing with a complex phenomenon with new information challenging their preconceptions all time, such as for instance the generally pervasive assumption that the forces between neutron and neutron, neutron and proton, and proton and proton were different. This necessitated flexibility in their ideas and approach which manifested itself in the tentativeness and creativity of their proposals aiming to explore different conceptual possibilities about how these forces might operate. The neutron concept and its own flexible usage were central to this.

In March 1933 Majorana published a paper in which he provided just such a conceptual innovation, with aim of improving on the obvious faults of Heisenberg's model.[354] The chief problem with Heisenberg's nuclear exchange force was its saturation. As was already known the binding energy of nuclei rose proportionally to the mass number A. Heisenberg's exchange force had it rise as A^2. The problem was identified as its dependence on spin-

[353] *Ibid,* p106.
[354] E. Majorana, 'Über die Kerntheorie', *Zeitschrift für Physik* (1933) 82, p.137. Translated in D.M. Brink, *Nuclear Forces* (Oxford, Pergamon Press, 1965, pp.161-169.

exchange. This meant that two nucleons (a neutron and a proton) would fill a state making the most stable combination. This contradicted in addition the fact that the most stable nuclei seemed to be α particles. Thus the nature of the nuclear force remained a very open question. Majorana worked specifically to resolve these problems.

In his opening sentence he expresses that, "The discovery of the neutron, a heavy and uncharged elementary particle, mechanics without, however removing the fundamental difficulties that are connected with β decay," thus showing that he appreciated the role of the neutron concept in framing a quantum-mechanical approach to the nucleus that didn't specifically resolve all its problems. Majorana poses as his methodology simplicity in reproducing the nuclear force. In fact he sets out to represent 'in the simplest way the most general and obvious properties of the nucleus', not to go further and resolve any other deeper problems about the precise form of the force or the processes creating it. In the name of simplicity he poses the Coloumb force for interactions between protons but none between neutrons (none had yet been detected). He rejects however the repulsive force between neutrons and protons that Heisenberg found himself forced to hypothesise, labelling it 'aesthetically unsatisfactory.' Majorana poses that he can avoid this problem and that of saturation by posing an exchange force in position only (not spin), with a positive sign for $J(r)$ rather than negative. Only the spatial coordinates would be interchanged, allowing combinations between different spin varieties. Such a force would allow the interaction between a neutron and two protons, and vice versa, rather than one. As such the α particle would be saturated rather than the deuteron nucleus, forming a complete shell. Being completely symmetric in position coordinates there would be no need for a repulsive force to prevent degeneracy. Finally Majorana was pleased to waive the need for the 'troublesome' ρ spin coordinates. He treated the proton and neutron as essentially different particles. From these assumptions he was able to show that the binding energy of the nucleus (assuming Coulomb forces are neglected) would come out proportional to the number of nucleons.

At around the same time as Heisenberg, Wigner published two papers applying quantum mechanics to the nucleus. The first entitled 'On the Mass Defect of Helium' attempts to derive a formal basis to show solely that the binding energy of helium would be greater than that of the deuteron. Wigner avoids the exchange force hypothesis, and instead proposes that if we treat the neutron as an elementary particle then there should be an ordinary potential

V(r) in operation (analogous to the Coloumb force).[355] Setting this *V(r)* then as a variable to be empirically determined, he established aspects of its form by comparison with results from the deuteron and neutron-proton scattering. The deuteron results suggested a relation between the width of the potential well and its depth. The width was somewhat already known from scattering results, thus leaving Wigner to make an estimate of depth. With this he got his result on helium binding energies thus showing the possibilities of supposing a more traditional force without any sort of particle exchange. Wigner's force however did not saturate, not forming a closed shell for the α particle, and thus was ruled out for nuclear binding, as Heisenberg proved. But the idea that a nucleon in the α particle was subject to the force of two others, and not just one other as Heisenberg's theory implied, proved important to subsequent thinking on the topic.

Wigner's second paper concerned the scattering of neutrons by protons, and thus visited again the issue of the nuclear forces between the two.[356] Scattering (exploring the interaction between particles fired at one another) in fact became the principle method by which nuclear forces were explored, since actual detail on nuclei was difficult to accurately obtain to the required degrees. Experiment by Curie and Joliot, Meitner and Phillips and by Dunning and Pegram was already providing the general features of such scattering, and Wigner worked on the basis that the exact shape of the potential wouldn't affect the scattering results as opposed to general qualitative features such as the range and depth of it, and its binding energy. Wigner showed in fact that these in fact were related to the binding energy such that if the binding energy was kept constant, range and depth were inversely related by a function Wigner worked out. Bethe and Peierl soon clarified this point systematically showing the irrelevance of choice of force, whether a Majorana or Heisenberg exchange force or Wignerian potential.[357] As Amaldi reports these papers by Bethe and Peierl 'played a fundamental role in the development of nuclear physics in those years,' by exposing the limitations of potential wells and the Schrödinger wave equation for dealing with neutron-proton interactions.[358] Wigner had in fact given on this basis a calculation for the cross-section of the interaction which was significantly at variance with the experimental result. He

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[355] E. Wigner, 'On the Mass Defect of Helium', Physical Review (1933) 43, pp.252-257. Although of course Heisenberg had already suggested as we saw that the exchange force itself could be treated as something independent of the potential structure of the nuclei.
[356] E. Wigner, 'On the Scattering of Neutrons by Protons', *Zeitschrift für Physik* (1933) 83, p.253.
[357] H.A. Bethe and R. Peierls, 'The Scattering of Neutrons by Protons', *Proceedings of the Royal Society* (1934) A149, pp. 176-183.
[358] E. Amaldi, 'From the discovery of the neutron to the discovery of nuclear fission', *Physics Report* (1984) 111(1-4), p. 144.

thus realised that the interaction was not in fact independent of the relative orientation of the spins of the nucleons. This implied that instead of one state, via which the interaction happened, there would be four (an S = 0 singlet state for antiparallel spins and an S = 1 triplet state for parallel spins). It was clear from the theory and the experimental results that the triplet state existed as the ground state of deuteron and the singlet state would have weaker energy, but it was not clear whether it was a real or a virtual state, in the latter case being incapable of binding the particles in a stable state. This prompted an experimental program involving Fermi to test its stability, eventually finding that the state was unstable.[359]

In each of these cases the neutron concept (as well as the proton concept) was used as a conceptual platform as a unit of nuclear force and nuclear structure subject to quantum mechanical rules for the discussion of different variations of nuclear forces. Some such as Heisenberg's and to a lesser extent Majorana's raised the issue of neutron structure. Majorana treated it as a fundamental particle (as did Wigner or at least he ignored the question), while Heisenberg was more ambiguous. Majorana in fact dispensed with Heisenberg's isospin conjecture, which could be read to treat the proton and neutron as the same particle in different states. As such the use of the neutron concept involved refashioning of ideas about it, in the pursuit of a more effective theory of nuclear force.

In fact in 1936 experimental results began to emerge from Tuve, Heydenberg and Hafstad, and Breit, Condon and Present that suggested the strengths of neutron-proton and proton-proton interactions were approximately the same assuming the ranges the same.[360] Factoring the Coulomb repulsion between protons out seemed to leave the forces identical, so charge independence of this interaction was hypothesised (the force independent of the charges on the particles). This postulation had a significant effect on how nuclei and particle interactions were thought about, being the inspiration for many symmetry rules about particles interactions that followed. Interestingly it was the only real property of pairs of nucleon testable in the context of heavy nuclei, since it implied a symmetry in the equations of motion of nucleons and nuclear structure. In that same year Cassen and Condon redeveloped the neutron (and proton) concept to fit this result by reemploying Heisenberg's isospin conjecture to provide a general formalism for discussing any nucleon interactions with a five coordinate system.[361] In

[359] E. Fermi, *La Ricerca Scientifica* (1936) 7(2), p.13.
[360] M.A Tuve, N. Heydenberg, L.R: Hafstad, 'The Scattering of Protons by Protons', *Physical Review* (1936) 50, p.806; G. Breit, E.U. Condon and R.D. Present, 'The Theory of Scattering of Protons by Protons' , *Physical Review* (1936) 50, p.825.
[361] B. Cassen and E. U. Condon, 'On Nuclear Forces', *Physical Review* (1936) 50, pp. 846 – 849.

this language charge independence implied that nuclear forces were invariant under isospin rotations.

4.3.3.2 Nuclear Forces and Quantum Field Theory

Quantum field theory emerged from attempts to find a quantum mechanical theory for electromagnetic phenomena, in order to account for the interaction of these fields and matter. Quantum mechanics itself wasn't capable of dealing with a massless particle travelling at the speed of light like the photon. The original theory emerged in the late-1920's at the hands of Born, Heisenberg, Jordan and most successfully Dirac. [362] The basic driving principle was that fields should themselves be quantised and localised in space (as represented by spatially dependent field operators). Changes in field state amounted to the absorption or emission of photons.

As we've already noted Chadwick's proof of the neutron didn't solve the problem of β decay. It simply left it open while theorists carried on using the neutron concept in order to give a quantum mechanical account of nuclear structure and forces. β decay remained an unexplained nuclear process. As we also noted Pauli had in 1929 postulated that an extra particle called a neutrino could be employed to account for the missing energy that was lost in the decay, but he couldn't offer any explanation for why the electron emitted had a seemingly continuous distribution of energies. The resolution of these problems was provided by Fermi's application of Quantum Field Theory to the nucleus in combination with the neutron hypothesis. It is not often noted however that this itself depended upon the neutron and proton concepts and Fermi's willingness to work with them in a novel way.

At the start of his first paper entitled 'Attempts at a Theory of β Rays I' published in 1934 Fermi states, "The emission of electrons and neutrinos from a nucleus by β decay will be treated by a method similar to that for the emission of a light quantum from an excited atom in radiation theory."[363] To get his theory to work Fermi explicitly followed Heisenberg's use of the neutron concept, treating the proton and neutron as one particle of different states, but expanding on it. The creation of a neutrino and electron transpires with the transition of this particle from neutron to proton. Thus he hypothesised the transformation of a neutron to a

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[362] P.A.M. Dirac, "The quantum theory of emission and absorption of radiation," *Proceedings of the Royal Society of London* (1927) A114, pp. 243-256; M. Born, W. Heisenberg, P. Jordan, "Zur Quantenmechanik II", *Zeitschrift für Physik* (1926) 35, p. 557.

[363] E. Fermi, 'Versuch einer Theorie der β-Strahlen. I' , *Zeitschrift für Physik* (1934) 88, pp. 161-177. Translated in F.L. Wilson, 'Fermi's Theory of Beta Decay', *American Journal of Physics* (1968) 36(12), pp. 1150-1160.

proton. This transformation was then coupled to a quantised field representing numbers of neutrinos and electrons at each spatial location. An operator $Q^{*}a_s{*}b_\sigma^{*}$ represented both simultaneous processes (Q changing the state of the neutron to a proton, and $a_s{*}$ and b_σ^{*} representing operators that increase the number of electrons and neutrinos at a given spatial location in quantum state ψ_s^{*} and ψ_σ^{*}). As such the individual fields of electrons and neutrinos were represented by;

$\psi = \Sigma\, \psi_s^{*}\, a_s^{*}$, summing over the states s.

Writing down the Hamiltonian for this interaction between a neutron (or proton), gave Fermi a formal basis for expressing the energy differences between field and proton/neutron states (depending on the eigenfunctions of the neutron (or proton)), and a theory of transition probabilities and lifetimes could be developed based on the idea that such transitions depended on the energy difference between initial and final states (the transition matrix elements) and the relative density of the final states. Evaluating the various coupling constants in conjunction with experimental energy curves for the emitted electrons gave extremely good fits with that from experiment. Fermi had realised the continuity of this curve could be related to the density of final states for an unbound electron which were themselves continuous.

Fermi's theory is thus not just novel in its application of quantum field theory, but in its use of the neutron and proton concepts in order to implement that theory. He relied on them conceptually as elements of nuclear structure to posit a process explaining β decay by taking advantage of quantum field theoretical ideas. But the neutron, like the proton, concept was a starting point for his solution, and conceptual tool for constructing those processes. As a result Fermi found a theoretical way to do without the electron in the nucleus, by postulating theory creation and annihilation in the nucleus. In 1936 Yukawa picked up these ideas and those of Heisenberg, using the neutron and proton concepts as alterable states.[364] He states at the outset that, "At the present stage of quantum theory little is known about the nature of interaction of elementary particles." He notes that the interactions energies given by Fermi's theory were too small to account for neutrons and protons in the nucleus. Tamm and Iwanenko had separately in 1934 investigated whether the interaction Fermi had posed might be responsible for the binding force between the two on the basis that the neutrino and electron pair might be

[364] H. Yukawa, 'On the Interaction of Elementary Particles', *Proceedings of the Mathematical Society of Japan* (1935), 17, p.48

exchanged together, working backwards to calculate the exchange potential.[365] They found it to be too weak. Yukawa posed instead that if a neutron transformed to a proton with much larger probability it would produce a much stronger interaction, implying the existence of a much heavier (heavier than the photon that is) exchange particle. As such Yukawa introduced a new field and a new particle governing the interaction. Nucleons interacted with the field by changing states, releasing or absorbing this heavy particle in the process. Yukawa formalised this by picking a potential of the same form as Heisenberg's original for the exchange force. To give the correct range for the nuclear force, the mass of the heavy particle would have to be about 200 times that of an electron.

Yukawa's theory for the later called 'strong-nuclear force' (to distinguish it from the weak one involved in Fermi's β decay), was not immediately taken up, until a particle in 1937 was discovered in cosmic showers having the same mass as his 'heavy particle'.[366] This meson was not in fact the right particle for the nuclear force, but it did drive the theory in the 30's and 40's nevertheless and was the basis for the prediction of the π-meson. Yukawa's theory became the meson theory of nuclear forces, and was the source of a conceptual framework for associating forces with exchange particles.

This excursion into the history of the field theoretical studies of nuclear forces was principally to highlight the conceptual role the neutron concept was playing throughout these events, and how it was providing scientists a flexible resource and a basis point on which to construct and reconstruct their thinking, reapplying it as a particle state in a quantum field theory formalism. This was fundamentally a novel move by theorists making novel use of both neutron and proton concepts, and relying on an open-endedness in the understanding of their structure, behaviours and properties. As many philosophers will tell you this was the beginning of a fundamental and confusing shift away from the classical idea of a 'particle' towards a 'field' conception.[367] What framed these shifts epistemically then for the neutron were its essential explanatory role in the explanation of β-decay and a conceptual role as a unit of nuclear force and nuclear structure. These facilitated these conceptual developments, while the roles themselves were elaborated and given content in the process. Of course our tendency to focus on the novelty of these approaches and their postulation of new particles hides the important

[365] I. Tamm, 'Exchange forces Between neutrons and protons' Protons', *Nature* (1934) 133, p. 981; D. Iwanenko, 'Interaction of Neutrons and Protons', *Nature* (1934) 133, pp. 981-982.
[366] C. D. Anderson and S. H. Neddermeyer, 'Note on the Nature of Cosmic-Ray Particles', *Physical Review* (1937) 51, pp. 884-886.
[367] See for instance Meinard Kuhlmann, 'Quantum Field Theory', *Stanford Encyclopedia of Philosophy* (2006), http://plato.stanford.edu/entries/quantum-field-theory/.

fact that theorists were pursuing this conceptual development by virtue of already established roles.

4.3.3.3 Building the Nucleus

The last ongoing theoretical application of the neutron concept I want to mention here, as illustration of the neutron's role in this research process, was the construction of the nucleus itself. From the days of Rutherford's speculation the neutron had been principally accepted as fundamentally a nuclear constituent and even before Chadwick's experiments the concept assumed an epistemic role in theorising about nuclear structure. Bothe emphasised that of all the issues surrounding the neutron this was its most certain role, pointing out epistemically that with the neutron concept the stability of the atom is not only now much simpler to deal with (without the presence of electrons) but also to interpret unconstrained (*zwangloser*) by these old issues.[368] The neutron concept in other words provided space for the reconsideration and reframing of nuclear stability. These thoughts from Bothe capture fairly how the neutron concept was used in this context as *Baustein*.

The most immediate of these was to reinterpret mass numbers in terms of proton + neutrons, and then to use this framework to study the patterns of existing isotopes. Theories tried to identify stability with numbers of alpha particles, and with numbers of neutrons above equality and how this varied as mass number increased. Heisenberg himself as we saw explained several facts about the existence and stability of isotopes according to neutron-proton ratios in qualitative terms. One of the first to publish on this, was Alfred Landé. In two papers of January 1933 he applied the neutron concept as a neutral unit of nuclear structure more precisely than Heisenberg to explain the patterns of isotopes and other patterns in the periodic table.[369] These patterns had begun to be identified by Aston from the development of his mass spectrograph onwards, and were put in the language of the nuclear model of protons and neutrons. These included 1) the atomic number of any atomic species is very close to a whole number on the basis of 16 (atomic weight of oxygen) 2) Abundance of atoms and isotopes of any type much higher for even than odd value of atomic weight 3) even numbered elements are much more abundant than odd numbered ones and in every case more abundant than adjacent odd numbered ones.

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[368] W. Bothe, 'Das Neutron und das Positron', *Die Naturwissenschaften* (1933) 47, pp. 825-831; p831. Bothe cites Heisenberg as having began the investigation theoretically of the consequences of the neutrons involvement in nuclear stability.
[369] A. Landé, 'Neutrons in the Nucleus. I and II.,' *Physical Review* (1933) 43, pp. 620-626.

His emphasis was on showing what could be done with the neutral neutron concept towards explaining these patterns. For this his tools were more or less just calculations of binding energies and mass defects, rather than any detailed considerations about the forces involved. He noted for instance that binding energies in an isotope seemed to suggest that each additional neutron added provided a fairly constant amount of binding energy. Studying Xenon for instance he found this to be 0.009 u per neutron. This constancy applied throughout the periodic table except for Neon. Landé further compared results when the neutron was taken into account, as against the old model with α particles, protons and electrons. On the old theory elements above 50 should be unstable when they are in fact not until element 80. This is because as Landé noted, in the old theory the number of α particles included is larger than in neutron theory. In the old model of Gamow (1931)[370] α particles were packed into the nucleus (mass value of 4) with two electrons to a maximum according to the mass number A, and then extra protons were added with electrons to make up the difference, and finally more electrons added to suppress any excess charge. On Landé's model α's were added according to the atomic number Z on the basis of providing the charge. Neutrons were then added to make up the isotopes. On this basis on the old model Xe^{125} (with Z = 54) would have 124/4 = 31 α's at least. This would give a charge of 62 but the nucleus has a charge of only 54, so an extra 8 electrons would be added, plus the one proton and electron to get to 125. On the new model 54/2 = 26 α's were required giving a total mass of 108, thus requiring an extra 17 neutrons. Hence Landé could point to a genuine explanatory advantage of the neutron model.

Starting again with the neutron concept as a basic unit of nuclear structure, Landé was also able to interpret more phenomena into rules. For example it seemed that evenly charged elements tended to have more frequently even numbers of neutrons. Missing isotopes in most cases belonged to odd neutron arrangements. This preference for an even number wasn't explicable by virtue of energy balances. Further it seemed that no odd elements have isotopes with an odd number of neutrons. This could be explained by β decay transforming an odd arrangement into an even one given the observed preference for even states, without explaining this preference itself. Landé was in fact the first to propose this idea we saw picked up later by Fermi that a neutron could transform itself to a proton (rather than splitting) creating an electron in the process. However the main content of Landé's paper, which again arose directly from the use of the neutron concept, was the application of quantum mechanics to the nucleus in his shell model. As he puts it;

[370] G.Gamow, *Atomic Nuclei and Radioactivity* (Oxford, Oxford University Press, 1931).

"It is very tempting but precarious to conclude from the table of the isotopes known up to now details of the arrangement of the neutrons in the nucleus. The existence of closed shells is suggested by the fact….of similar isotope sequences repeated in different elements."[371]

Landé was not in fact the first to re-raise the question of a shell model for the nucleus. Bartlett had about 6 months earlier, doing a similar analysis although relying on less information and more on the analogy of electron shells, rather than the α particle as a basic component.[372] Either way both saw the potential of the neutron concept for giving them a capacity to refine element and isotopic patterns into a shell structure, and thereby explain them. Bartlett was however employing an electron concept in the nucleus, which marks out Landé's novelty.

Having collated these isotopic sequences Landé himself built up a model of neutron shells for those neutrons remaining outside of α particles. He pursued this theme in his second paper providing more detail on this shell structure and mapping it to the recorded binding energies. He used this model to explain some peculiarities of these energies from $Z = 36$ to 40 and $Z = 50$ to 54. As he puts it with such a model at hand he hoped 'to give a more rational basis to the art of predicting missing isotopes.'

Work on systematics and the shell model of the nucleus continued in this vein after Landé, applying the proton-neutron model. Mattauch added in 1934 the isobar rule that there are no pairs of isobars with charges (Z) that differ by one unit.[373] Bethe and Bacher in their 1936 paper give an extensive discussion of the issue. They clearly presented their postulations using the neutron-proton model tentatively, as a basis for investigation. "Not many definite results concerning details of the structure of heavier nuclei have yet been obtained. We shall discuss in this chapter only a few of the attempts to obtain such a theory, and only those likely to become starting points for future research."[374] They argued against the α particle model as essential subunits, by resting their theory of the nucleus on the 'overwhelming strength' of the neutron-proton interaction which for small nuclei makes those elements most stable which contain equally many of each, and secondly on the 'even-odd' rule making even numbered nuclei stable because of the Pauli exclusion principle. In any case for particles greater than mass number 30, α particles would repel each other too strongly to be contained as unit.

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[371] *Ibid,* p. 623.
[372] James Bartlett, 'Structure of Atomic Nuclei I', *Physical Review* (1932) 41, p.370; 'Structure of Atomic Nuclei II', *Physical Review* (1932) 42, pp. 145-146.
[373] Isobars have the same atomic mass but different atomic number. J. Mattauch, ' Zur Systematik der Isotope', *Zeitschrift für Physik* (1934) 91, pp.361-371.
[374] H.A. Bethe and R.F. Bacher, 'Nuclear Physics: Stationary States of Nuclei', *Reviews of Modern Physics* (1936) 8, p168.

Rather we need the neutron concept in order to hold onto individual protons more actively as 'mortar'. Binding energy measurements of neutrons supported this idea better than that of conjoined α's, which would probably be dissociated by such energies. Bethe and Bacher's main focus is as a result on putting together a general shell model applying to all the protons and neutrons in the nucleus (not just outer neutrons). They claimed this gave a better basis for prediction of periodicities in nuclear structure and the best hope for some development of a rational theory of nuclear spins.

Weizsäcker in 1937[375] (relying on Mattauch, Harkins and Aston) was able to state the rules in proton-neutron terms as follows: 1) nuclei with even proton and neutron numbers are more frequent than those with one or two odd particle numbers. Odd-odd cases are only found amongst light elements 2) there are no pairs of isobars with odd mass or even mass with atomic numbers that differ by one unit 3) elements with odd atomic number have never more than two isotopes. Weizsäcker himself in 1935 had created a semi-empirical mass formula trying to trace stability curves in terms of an equation in N (number of neutrons) and Z, relying on binding energy approximation from nucleon interaction and approximate Coulomb repulsion, surface effects and a term representing a decrease in binding energy when differences in numbers of neutrons and protons.[376] Mattauch worked as well at expressing all these rules in terms of binding energies, which he'd succeeded at by 1942.[377]

As such the neutron concept played a conceptual role in the development of these theories, as a unit of nuclear structure which allowed the incorporation of quantum mechanical arguments, as a starting point for reinterpreting nuclear structure and developing richer theoretical accounts, that built on this role by bringing in more theoretical details and mathematical tools, but always in a tentative way given the everchanging informational context.

4.4 The Open-Endedness of the Neutron Concept

When we discussed the gene concept in the last chapter one of the main points to be made about its role in the genetic research process, was that it was maintained open-endedly, to the extent that it was itself adapted and shaped in terms of its structure, properties and behaviours (its representation in other words) to deal with the complexity of the phenomena it was being applied to explain. In this sense its epistemic roles in providing a basis for the theory of and

[375] C.F.v.Weizsäcker, *Die Atomkerne* (Leipzig, Akademische Verlagsgesellschaft M.B.H., 1937).
[376] C.F.v.Weizsäcker, ‚Zur Theorie der Kernmasse', *Zeitschrift für Physik* (1935) 96, pp.431-458.
[377] J. Mattauch, *Kernphysikalische Tabellen* (Berlin, Springer-Verlag, 1942).

experiment into genetic phenomena through its causal relations for instance provided space for the concept to be adapted with respect to its performance of those roles in response to new information and phenomena.

What can be said of the neutron concept in this respect has a strong parallel in the case of the gene concept. The concept was itself throughout this period open to investigation and open to redevelopment and reconsideration. Aspects of its properties and structure were part of the investigative landscape of research into the nucleus and nuclear processes. In other words the methodological approach of physicists as a group theoretically and experimentally alike was to treat the concept flexibly rather than pin it down to any description or set beliefs.

There are two aspects of the concept that stand out in this respect: the discussions of its structure and the measurement of its mass. As we've seen already from Rutherford's original conception, the neutron was pictured as a combination of a proton and electron. This explained its neutrality out of available concepts but rejected the idea that it was fundamental. The idea informed the use of it conceptually in some respects before it was established to exist by Chadwick and its early years thereafter. But while some historians have a tendency to paint the acceptance of this structure as almost unanimous I think the real story is actually quite different.[378] The historical record shows that physicists in fact kept an open-mind about the issue, even when tending strongly one way or another and weren't committed absolutely. Thus viewpoints had space to develop through the 30's on these questions of mass and structure.

In Chadwick's original paper published in 1932 he presents a section entitled 'The Nature of the Neutron' in which he puts forward as 'the simplest hypothesis one can make about the nature of the neutron' the assumption that it consists of a proton and neutron in close combination, taking his estimate of the mass as support.[379] He recognised however the possibility that the neutron might also in fact be fundamental although stating that, "This view has little to recommend it at present, except the possibility of explaining the statistics of such nuclei as N^{14}."[380]

[378] L.M. Brown and D.F. Moyer, 'Lady or Tiger? The Meitner-Hupfeld Effect and Heisenberg's Neutron Theory', *American Journal of Physics* (1984) 52, p. 134.
[379] J. Chadwick, 'The Existence of a Neutron', *Proceedings of the Royal Society* (1932) A136, pp. 692-708.
[380] *Ibid,* p.706.

In his Bakerian lecture of mid-1933 he was more open to the possibility that the neutron might be fundamental. He quotes the mass of the neutron again as supported its complexity, but acknowledging that ‘this is by no means conclusive’[381] . The best proof would be observation of its splitting. But, ‘On the other hand, certain arguments can be advanced to support the idea that the neutron is an elementary particle. He notes spin, and also the quantum model of the hydrogen atom as seemingly incompatible with the complex model of the neutron. “If the neutron is a proton and an electron why does not the hydrogen atom transform into a neutron with the release of energy?...This consideration seems to me to argue strongly for the elementary nature of the neutron.”[382] As such Chadwick preferred to keep the neutron completely open, stating that it seemed necessary to recognise these difficulties and, ‘while retaining the hypothesis that the neutron is complex for some purposes’, to regard it as an elementary particle for others, particularly with respect to composing the structure of the atom (taking into account spin and statistics).

This was of course the essential issue that compelled physicists to remain open with regard the neutron concept. One of its chief epistemic functions was the construction of the nucleus and the application of quantum physics to it, which seemed to demand the neutron be treated as fundamental, especially given the spin and spin statistics of N^{14}. In mass measurements, β decay, the Meitner-Hupfield effect and some collision experiments however it seemed more reasonable that the neutron was complex, especially since it provided the basis for an exchange force in proton-neutron scattering, which was proving a useful explanatory hypothesis. As such either hypothesis on the structure had epistemic value, but this dual use compelled an open mind about the structure of the neutron itself for which there was no firm evidence either way. In any case it’s not right to say that Chadwick was fixed in his beliefs about neutron structure. Of course Chadwick’s views may well have modified somewhat with the success of the neutron in its application to the problem of nuclear structure, but not due to the advent of new mass measurements which occurred later. Rather it seems Chadwick varied his views or at least found the evidence inconclusive, demonstrating in either case he considered the concept open-endedly, as disposed to change his views.

As we have already recounted, between 1933 and 1934, mass became an open issue and thus one of investigation for the research process. It motivated debate and new experiments such as

[381] J. Chadwick, ‘The Bakerian Lecture: The Neutron’, *Proceedings of the Royal Society* (1933) A142, p. 14.
[382] *Ibid*, p. 14.

Chadwick and Goldhaber's in order to precisely measure it. The question of neutron structure seemed to hinge on its measurement. Experimenters, even those backing particular experimental results, perceived the question as inconclusively settled during this period, and thus considered mass an open aspect of the neutron concept. When Chadwick and Goldhaber performed their experiment and got a result that disagreed with everybody else's including their own, they adjusted their beliefs. Research nonetheless continued in 1935 because even their measurements conflicted with some emerging from transmutation data and mass spectrograph data. This in the end prompted Aston to have to revise his reading of his mass-spectroscopic results.[383] Further, work progressed seeking the neutron's mass to greater and greater accuracy.

Theorists especially treated these aspects of the neutron concept as open-ended, and thus a flexible conceptual platform upon which they could erect new ideas and put forward new propositions, even if they contradicted other ideas about the neutron. The history of theories about nuclear forces is intimately connected with this fact. Heisenberg treated neutron structure essentially in this way, making the assumption it was fundamental in some contexts and built out of a proton and electron in others, leaving the question itself open for future investigation. In his first text 'On the Structure of Atomic Nuclei' he states that, "The neutron will be taken as an independent fundamental particle, which, however, can be split under favourable conditions, into a proton and an electron."[384] One could also read Heisenberg proposing the idea that the neutron and proton are simply states of the one particle with his introduction of isospin, by which the neutron and proton are completely symmetrical. As such he remained ambiguous about how to interpret his exchange forces: as fundamental or due to the movement of an electron from the neutron to the proton. He was partially ambiguous in this way because he realised enforcing the proton-electron model just created other problems. "If we wanted to describe the neutron as composed of protons and electrons, the electron would have to obey Bose statistics and have spin zero…" β decay itself, instead of being explained by electrons in the neutron, is simply reserved to some unknown mechanism. However at the end of this text he acknowledged that treating the neutron this way left explanatory holes that seemed to demand the need for the electron, particular the Meitner-Hupfeld effect. This phenomenon seemed to require that an electron be part of the neutron.

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383 According to R.H. Stuewer, 'Mass-Energy and the Neutron in the Early Thirties', Science in Context (1993) 16, p 229.

384 W. Heisenberg , 'Über den Bau der Atomkerne. I', *Zeitschrift für Physik* (1932) 77(1-2), pp.2. Translated in D.M. Brink, *Nuclear Forces* (Oxford, Pergamon Press, 1965), p.145.

As such Heisenberg's paper is a classic example of the use of a theoretical entity concept in a deliberately open-ended fashion, treating the concept not as some precise well-defined object, but rather as a conceptual starting point that could be flexibly used to solve some problems, remaining open-ended and thus an object of investigation through the performance of, and reliance upon, its epistemic roles, which formed a basis or starting point for the entity's own consideration.[385] In 1970 Heisenberg expressed his attitude, which took its idea of the neutron from thinking about its conceptual role constructing the nucleus, "At the time we had an unclear feeling that the neutron somehow can be considered as consisting of proton and electron, but also somehow not, because after all, the neutron seemed to be very similar to a proton. I had from the very beginning the idea that the neutron was a kind of brother to the proton….from the fact that an approximately equal number of protons and neutrons were in the nucleus…"[386] For Heisenberg, such epistemic roles, and their elaborations, gave him some basis for thinking about the neutron itself.

Of course different contemporaries interpreted this ambiguity in different ways. Wigner's text assumed that Heisenberg was in fact 'emphasising' that the only elementary particles were protons and electrons.[387] Wigner himself however having identified the different possibilities about the interpretation of the neutron's structure, including it's possible existence as fundamental, created a theoretical position using an ordinary potential for the force between the proton and neutron which didn't specifically depend on either, and thus could tolerate its uncertainty in this respect. His goal was the character of this force and scattering relations, and not the explanation of either β decay or the Meitner-Hupfeld effect. As such he didn't find it necessary to consider neutron structure beyond such a survey.

These different ideas about the neutron's structure had an earlier origin, and the openness of the concept in this respect was a critical element of the conceptual uses the concept was put

[385] Bromberg theorises that Heisenberg accepted an inconsistent model of the neutron as the basis upon which to do any physics in the nucleus at all. This seems right in the context given the different explanatory roles he was assigning to the neutron. He had to use the neutron inconsistently as a result. But of course this is consistent with the idea that Heisenberg was providing a starting point for further investigation into all these aspects, not explicitly resolving any of them. For others, given the investigative paths they pursue in this respect, the inconsistency doesn't arise. Majorana and Wigner for instance concentrate only on nuclear force, ignoring the β decay problem, and thus not needing to assert the *pe*-model. J. Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, p.333.
[386] From an interview by Joan Bromberg with Heisenberg quoted in J. Bromberg, 'The Impact of the Neutron: Bohr and Heisenberg', *Historical Studies in the Physical Sciences* (1971) 3, p.333.
[387] E. Wigner, 'On the Mass Defect of Helium', Physical Review (1933) 43, pp.252-257.

to. In 1932 Massey wrote a theoretical paper which Chadwick himself referred to, relying on the *pe*-model as what he refers to 'as the most likely model'.[388] Acknowledging the restrictions given by quantum mechanics on the bonding between electrons and protons, he assumes the existence of a nearly-zero quantum state and constructs a formula on the basis of it that generated predictions about collisions and scattering.[389] It was clear that Massey considered the structure of the neutron an investigative issue pertinent for the understanding of nuclear forces for which a theoretical approach could be used to generate the kinds of predictions against which different models could be tested, arguing as such that "it is therefore very necessary to obtain as much knowledge as possible of the field of force surrounding the particle. If this field be known a very strict test of any theory of the nature of the particle is provided."[390] By the time of the Solvay conference however it was clear that such field measurements, through scattering, did not support Massey's structural model, placing some question marks on the *pe*-model, but it was nonetheless historically an attempt to elaborate the neutron concept in order to give a better understanding of nuclear force and nuclear interactions by virtue of its participation in these.

On the other hand Iwanenko in his short note to nature put it as 'the chief point of interest…..how far the neutrons can be considered as elementary particles.' [391] He expanded on this shortly after, with a model of the nucleus of just protons and neutrons where those electrons involved in β decay were actually produced not by the splitting of a neutron but by a change in energy levels of the neutron, causing a particle emission in the similar way that a photon was emitted when charged particles shifted energy levels.[392] These ideas were incorporated mathematically by Fermi in 1934 again relying on the idea that neutrons and protons were simply states of the same particle. He thus too relied essentially on the openness to interpretation of the concept to apply it to the β decay problem. This idea was pursued to its extreme by Cassen and Condon who created a general symmetric formalism for the nuclear force by virtue of attaching protons and neutrons to isospin states.[393]

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[388] H.S.W Massey, 'The Passage of Neutrons through Matter', *Proceedings of the Royal Society of London* (1932) A138, pp. 460-469.
[389] Massey highlighted the fact that no relativistic theory of electron/proton interactions yet existed thus maintaining the open possibility that other states may exist. *Ibid,* p. 461.
[390] *Ibid*, p460.
[391] D. Iwanenko, 'The Neutron Hypothesis', *Nature* (1932) 129, p. 798.
[392] D. Iwanenko, 'Interaction of Neutrons and Protons', *Nature* (1934) 133, pp. 981-982
[393] B. Cassen and E. U. Condon, 'On Nuclear Forces', *Physical Review* (1936) 50, p. 846.

Majorana however preferred to take neither of these approaches, instead posing the neutron as fundamental but 'by distinguishing' between the proton and neutron and not treating them simply as states. He does so principally for formal reasons however, to avoid having to deal with 'the troublesome ρ-spin-coordinate.' But while Majorana's exchange forces were influential, Yukawa's nuclear force again relied upon this state presumption to build his model of proton-neutron interaction.

Thus during this theoretically productive time in the neutron concept's existence in the 30's, 1932-34, there were at least 3 interpretations of the neutron flying around: the *pe*-model, the fundamental model, and the state model. These composed a general openness of interpretation of the neutron concept that allowed physicists to play with different conceptions appropriate for their particular goals, whether creating a theory of nuclear forces, or dealing with β decay. Once β decay had been theoretically accounted for by Fermi the need for a *pe*-model diminished. But the other two potential interpretations remained. As such the neutron concept played important epistemic roles in this respect providing a conceptual platform that could be differently developed by physicists, as they themselves took conceptual advantage of the neutron concept to deal with aspects of nuclear forces and nuclear processes.

When we consider the history of the neutron from 1932 until 1939 we have to take into account that part of its epistemic role in the research process stemmed from the openness in its interpretation and formulation. These provided investigative issues for researchers, but also a flexible conceptual and explanatory tool for their theoretical understanding that could be developed and redeveloped to respond to different problems. It was in fact in such contexts, not independently of them, that ideas about the neutron took shape. The neutron's own development was an embedded part of this ongoing research as it grappled with its epistemic goals, research that was itself driven by the epistemic roles of the neutron concept. Underlying this was a methodological acceptance by physicists at least at the group level that the neutron concept could be used in this way. Given the complexity of the phenomena being dealt with and the evidence available nuclear physicists did not as a group pin themselves down to one interpretation or one measurement of its properties, but maintained the flexibility of the concept to be adjusted and adapted, and its properties to be re-measured and reassessed, to pursuit of its conceptual and explanatory roles. Individuals of course took particular positions and sometimes asserted them quite strongly, but these positions never came to constrain the group which is evidenced by the alternatives that were employed and jointly considered. For instance in Bethe and Bacher's summary of the state of nuclear physics in April 1936, all the alternative constructions of the nuclear force were considered together. As such physicists in general treated the concept and used it open-endedly, acknowledging and investigating those aspects that were uncertain, like its mass, spin, or structure, and displaying a willingness to change these aspects of the concept as the changing circumstances demanded.

This openness in more general respects guided the research process. The experimental testing with the neutron concept was prefaced on the idea that not everything was known about the neutron or its behaviour in various circumstances, particularly regarding its reaction with nucleus and the part it played in nuclear stability. It was felt that there was more to be said about it and found out about it, and this open-endedness motivated and guided particularly experimental research. Artificial radioactivity experiments with neutrons and slow neutron experiments can be seen in these terms. These of course didn't stem so much from the uncertainty of interpretations, but from a more genuine lack of knowledge about the neutron's properties and behaviours. Nonetheless the over-arching principle is the same. Namely, that the neutron concept was open-ended in various respects or became so as the research process developed, and thus presented possibilities for investigation and conceptual development in the course of this research.

With this in mind we can once again describe the neutron concept in this period, as we did with the gene concept, as broadly akin to an 'epistemic-frame'. This means that the concept itself resourced the research process as an explanatory, conceptual and experimental tool, and by providing relevant issues for investigations, many of these arising from the open-endedness of the concept, which we identify both with the uncertainty over the description of the underlying entity, and possible unknown properties and behaviours, but also the methodological willingness to accept its re-description in the context of the uncertainty and the complexity of the phenomena being studied. Through its uses in these respects one can talk of the neutron playing a role 'framing' investigation in nuclear physics in the 30's. It was relative to the neutron concept that problems for investigation emerged, and by virtue of its conceptual, explanatory and experimental roles, that they were investigated, and dealt with. Physicists approached the nucleus and nuclear processes strongly with the neutron concept in mind, which of course influenced what they identified as problems or research issues (like a theory of neutron-proton force) and the potential solutions they proposed which themselves relied on these conceptual explanatory roles and their constitutive properties (particularly their structural role in the nucleus, their neutrality, their subjection to quantum mechanical laws), and indeed the open-endedness with which new ideas could be developed and new interpretations given. Like theoretical entity concepts generally I believe, the neutron concept was tied to the research goals it was originally posed to address and developed flexibly in response to new phenomena in the performance of, or reliance on, its epistemic roles in this regard.

It is in fact rather a moot point to raise the realism issue that gene theorists had to deal with and argue for. There was little issue over the reality of the neutron concept throughout its life. Before Chadwick's experiments everyone accepted its existence as speculative. After it opinion quickly changed. While this process of change is not well documented, in April 1932 as part of a discussion on the Structure of Atomic Nuclei at the Royal Society Rutherford suggests that the neutron was still yet to be confirmed by experiment.[394] But by May 1933 Chadwick in his Bakerian lecture is able to report that it was generally assumed that the nucleus consisted of protons and neutrons. There was as such already a reasonable framework of particle concepts in physics to give a basis of interpretation of the concept even if its structure did remain open-ended. Nonetheless to an extent their realism in the situation was for this very reason an *open-concept* realism, since over the course of investigation it did allow the concept to be open-ended in aspects of its properties, and to be a genuine object of further investigation.

4.5 Conclusion

At the start of this chapter I set out to give an epistemic account of the history of the neutron concept to the investigation of the nucleus, associating it thereby with a particular developing research process. Having discussed this now, through the various historical stages, we have a clearer image of how this process depended on the neutron concept through its various conceptual, explanatory, and experimental roles, and more generally its role as a device for investigation of the nucleus (including into the underlying entity itself) through these roles. As such the neutron concept both resourced this process and influenced its direction through these epistemic roles, but also was the subject of the process itself, and developed within the framework of these roles.

[394] Rutherford et al., 'Discussion on the Structure of Atomic Nuclei', Proceedings of the Royal Society (1932) 136, pp. 735-762.

We can then characterise the use of the neutron concept in this period through these formative and non-determinative epistemic roles;

1. The explanatory and conceptual role explaining and describing the properties and composition of the nucleus as a unit of nuclear structure of neutral charge and subject to quantum mechanical principles.
2. Conceptual and experimental role constructing theories and producing information respectively about nuclear forces, as a unit of nuclear force subject to quantum mechanical principles.
3. Explanatory role explaining experimental nuclear reaction phenomena as a neutral unit of the nucleus.
4. Experimental role producing nuclear phenomena and probing nuclear reactions: relying on the neutrality of the neutron and its role as a unit of nuclear force.

In both the experimental and theoretical contexts then the neutron concept had a dynamic role in the research process, as both a subject of it and resource for it – roles which themselves overlapped. This gives us I believe a grasp on how to understand the way a concept like the neutron concept was being used when researchers were actually confronting the research frontier. It is at the 'research-frontier' where there is a lack of knowledge and uncertainty that researchers have relied on their theoretical entity concepts in these epistemic ways as resources to develop that research towards its goals. The roles themselves have formed an epistemic frame in this respect, underlying the response of scientists to the dynamic phenomena of typical research, but providing them nonetheless with purposes and properties to rely centrally on to structure their investigations and interpretations and direct that research towards its goals, where generally the investigation and interpretation of the underlying entity itself was pursued too as part of the performance of, or reliance on, these roles to develop better understandings and explanations of the relevant phenomena. I've tried to establish that we can understand the broad but central contribution of the neutron concept historically to research in the 30s in these terms. In this case those research goals were to understand the nucleus, and the neutron concept through its roles was a historically significant part of how scientists chose to pursue that research and what they relied on for means to pursue it. In this way the structure and other descriptive aspects of the neutron itself were not fixed by these roles, but developed and redeveloped with the aid of them in response to new phenomena as an integrated part of the research itself and the attempt to rely on the development of the neutron concept itself to better understand the nucleus and nuclear forces. As such the role of the concept representing a newly 'discovered' entity translates to only a small part of what the concept was being employed to do. The discovery itself rather justified a more extensive reliance on and use of the concept, and was in this sense a significant epistemic event, not just an ontological one.

It is worth pointing out the neutron concept continued playing epistemic roles in the nuclear investigations that followed and then into the analysis of particle physics, although of course its importance to these investigations was never as singularly important in these epistemic senses as it was in the 30's. It continued to play individual experimental and explanatory roles. But in the context of particle physics, conceptually it became part of a particle system, which was itself the subject of investigation. Nonetheless as part of what was a complex, dynamic and evolving research process into the particle and field nature of matter, one could never say the concept was fixed by scientists. In the context of new quantum field theories in fact the research process took the particle concept in general as something to be investigated, while maintaining to some extent the epistemic roles these concepts were required to perform. Of course this is highly complex issue that shouldn't be summarised so lightly. However what is clear is that in the context of this process, the neutron concept again changed its interpretation with the acceptance of the quark model in the 60's as of ontological, not just formal, import. The entity thus lost its status as fundamental and became a structure composed of three quarks. This change while significant I would hesitate was not a conceptual disruption for the researchers involved, but the outcome of a continuing research process in which concepts like the neutron concept were amenable to change as part of their very roles structuring theoretical investigation. The remainder of this thesis concerns precisely what we should make of suppositions like these.

Chapter 5: Conceptual and Referential Continuity in Research Processes

It’s probably quite evident from what I’ve said so far that I’ve already assumed a degree of conceptual continuity in the case of these two examples, which is I think evident from the way scientists approached these concepts and developed them across time and in different contexts such as the experimental and theoretical contexts of nuclear physics. In this chapter I want to draw out what I think is part of the basis for this continuity in practice, which I think guides not just conceptual continuity but also referential continuity in the context of ongoing research. I’ll elaborate as such on what I see is the methodology scientists rely on, or have relied on, themselves. In this chapter I won’t argue that there is necessarily a rational basis to these practices of reference. I’ll save that for the next chapter. Thus I see myself more or less extracting from the historical records elements of practice that fill in what is otherwise a rather larger hole in our accounts, at least those given by general philosophy of science (concerned as it has been with realism and reference), which have generally proceeded first from a philosophical standpoint concerned with the linguistic analysis of concepts, rather than an empirical one, ascribing positions to scientists that simply don’t reflect very well what scientists seem to be doing with theoretical entity concepts and how they have communicated about them. However seeing this requires somewhat of a significant shift in the usual ways we understand conceptual and referential continuity, as it requires that we contextualise theoretical entity concepts at the centre of ongoing research processes and thus shift the philosophical presumption away from looking at such concepts as static or ring-fenced and determinate constructs to seeing them in practice as more open and flexible epistemic resources for furthering this research into complex and uncertain phenomena.

Like then perhaps a growing number of philosophers and historians I believe that the problem of incommensurability has been somewhat misconceived, attributing a notion of ‘concept’ to scientists which in practice they don’t rely on. In fact incommensurability itself can be understood as an artefact of a particular notion of ‘concept’ and a particular method of constructing concepts which philosophers have traditionally employed. I would ascribe to it the following presumptions about concepts.

1. They are elements of claims about the world – representations in other words.
2. They are essentially linguistic structures (analysable within philosophy of language) in this way treated as ring-fenced containers or vessels which carry content.

3. They are formed as the outputs or products of research and thus read off or 'reconstructed' from the beliefs of researchers at a given time.
4. We observe and analyse them historically in terms of these reconstructions by comparing concepts as snapshots at different stages of a research process.

While this doesn't reflect all philosophical practice, it does characterise the underlying basis of the incommensurability dispute most of which has focused on just these kinds of theoretical entity concepts I've been referring to, although the position is often meant more generally to dispute realist assertions about the correspondence of such concepts to real world entities. I want to emphasise that the consideration of the epistemic roles of such concepts in actual practice renders this problem of conceptual discontinuity somewhat artificial reflecting the fact that in the research context the kinds of conceptual disjunctions that are supposed to flow from these presumptions about such concepts, do not reflect the actual treatment of them in practice, and thus form a bad basis for our understanding of them. As such my targets are those instances of conceptual change that occur in science where theoretical entity concepts were nonetheless considered by researchers to persist despite significant changes in their descriptions. In fact even this gives somewhat of a false static impression since conceptual change is continually involved in any research process, both in the actions of individuals with respect to their concepts and at the group level. In the two case studies we've seen this I hope was a rather evident part of the narrative I gave. Conceptual change where we are talking about meaning change or change in representations is not exceptional, but is a routine part of research with these concepts, and incommensurability is the result of our attempt to analyse them in static terms.

In fact the meaning-based definition of concepts is in fact a critical part of theories of incommensurability as a natural expression of these presumptions above (although causal theories of reference too rely on aspects of them). These, which trace back to Frege but are implicit in the work of Kuhn and Feyerabend, assert that a concept is essentially a linguistic structure defined by a set of necessary and sufficient conditions. As such changes in these change the meaning of the concept and thus generate a different concept. But as we've already suggested in our analysis of the epistemic roles of the gene and neutron cases, the continuity of concepts does not seemed to be constrained by their descriptions. We expressed this as an open-endedness in the use of such concepts, which extended from simple vagueness in the description, a lack even of an account of the underlying physical structure or basis of the entity, to a disposition to accept the impermanence of property ascriptions and revise them as required. As such in terms of reference it seems that scientists in practice do not make decisions about the continuity of reference based solely on the meaning of such theoretical entity concepts but seem to have a broader understanding that meaning is a variable in the context of their research and can't be relied upon in any straightforward way in this respect, which is particularly the case for theoretical entity concepts.

I'll use the case then of the gene and neutron concepts to argue that different continuity relations for central theoretical entity concepts are at work other than meaning similarity when these are placed in their research contexts and argue that meaning-based approaches to concepts in the context of scientific practice give an inadequate account of how concepts are employed and retained. Further incommensurability theories give an inaccurate impression of discontinuity which misrepresents the views and methodology of scientists in the research context, and the way they have often employed theoretical entity concepts. My position is that these epistemic roles, through which theoretical entity concepts have participated centrally in research processes, have been important elements for framing the interpretation (or identification) of phenomena in terms of the entity, and the investigation and development of it conceptually. To some extent they may even be said to have been constitutive of that investigation and development. Certainly entity concepts haven't been traditionally developed in a vacuum. They have been historically developed as part of the research processes with which they are actively engaged through their epistemic roles, and as part of this process their roles in this research acted as means by which the entity concept was itself considered. Developments in the context of research typically in their consideration of a concept have worked towards an elaboration of these very roles, since these include presumptions about how the entity interacts with the world experimentally, or conceptually in the context of theory, and thus the very channels for investigating them further. Theoretical entity concepts have thus been to an important extent creatures of these roles. 'Constitutive' then describes the sense that these roles have provided the necessary basis that researchers could rely on to identify the entity and communicate a common referent by virtue of providing means or tools for its investigation as part of this process. They have shared in other words the same set of roles for directing their study of some aspect of the world, a putative causal agent for example, and interpreting the information they've uncovered relative to, or in terms of, an entity concept that was playing these epistemic roles in their research. This kind of activity is not independent of presumptions but in fact depended on having some epistemic frame which was open enough to allow a genuine investigative process, but provided also some gripping points for that investigation and development to function in the first place.

When scientists have agreed then about these roles in the research context then they have agreed to some extent about what they could rely upon to focus on some uncertain and unknown aspect of the world, which a theoretical entity has generally represented, and pursued it without necessarily knowing much about that entity or being confident in what could be asserted of it. This makes sense in an ongoing research context. These roles weren't meant to tell scientists what the entity is in any deep way, or fully describe its behaviour, or provide a coherent description, but were there to provide *means* for getting just these things with the aid of further experimental and theoretical results and discoveries. As such they might not have agreed about what they were looking for but they agreed about how to look for it, in the metaphorical sense that 'looking' for theoretical entities is done through the indirect lens of certain theoretical presumptions, and thus have had a basis for comparing and discussing their results, and giving their different interpretations.

But this doesn't mean they would have agreed about the results of the interpretation, investigation and developments pursued through these roles. These roles were not determinative, but acted as resources and constraints. What matters for continuity then is not the descriptions come up with by virtue of relying on these roles and their constitutive properties, but reliance itself which has given historically to scientists a basis for belief that they were developing the same concept, by having some shared means to find out and develop more about the underlying entities in the course of ongoing research.

My claims regarding conceptual and referential continuity historically regarding central theoretical entity concepts devolve into two main points….,

1. Open-endedness: Theoretical entity concepts have been typically employed through their epistemic roles as central parts of ongoing processes of research, utilised towards achieving their goals of understanding certain phenomena. These concepts have been typically developed not in isolation but as part of these processes with these goals in mind. In the context of complexity and uncertainty of the phenomena under study, the lack of epistemic access to the underlying entities, and the continual development of new experimental and theoretical information pertinent to the research, researchers

have tended towards a methodology of open-endedness in their formulations of the concept, lessening the importance of descriptions to conceptual and referential continuity.

2. Means of investigation and development: Secondly scientists have relied on these epistemic roles to provide a basis for directing their investigation and development of the entity concept. These explanatory, conceptual and experimental roles, and the investigative roles they illuminated, have provided shared means through which information was gained about the entity and interpreted and with respect to which the concept was further developed. To some extent we might talk of these roles as constitutive of the investigation and development of the concept, providing the experimental and theoretical contexts through which the underlying entity could be pursued. A theoretical entity then is identified in the research context partially by the paths and means through which it can be further investigated and understood in the course of research. Historically then when researchers have agreed about these means of investigation they have agreed about what they've been talking and developing conceptually as the same underlying entity.

Again, these are claims about how we account for the practices of researchers regarding theoretical entity concepts. With regard to the two case studies of the gene and neutron I'll try to draw out how in the context of research, these roles formed a basis of continuity across time despite descriptive change in the concept, and across disciplinary divides. But I won't restrict myself to these examples, drawing upon other examples in the literature that have themselves already been well studied for which the similar concerns of conceptual and referential continuity have arisen (namely the electromagnetic field concept for Nancy Nersessian[395] and the electron concept for Theodore Arabatzis).

I want to claim then that part of the problem with more traditional accounts of concepts, especially with respect to such theoretical entity concepts, is precisely that they tends to treat concepts independently of their research environment as simply containers of content which can be reconstructed from available theoretical information as a set of properties, causal powers, behaviours and so on. Not only is the implication there that this is their only scientific role but also that in the context of practice, concepts are treated this way in research as simple

[395] Nancy Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Martinus Nijhoff, Dordrecht, 1984). Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (Chicago and London, University of Chicago Press, 2006).

ring-fenced or ring-bounded containers. We have already seen enough in the last two case studies I believe to doubt it. In fact I think that for the purposes of studying conceptual change, extracting concepts out of research processes in this way gives a misleading impression favouring readings of conceptual discontinuity. Theoretical concepts rather are particularly embedded in the research processes to which they belong and the particular way in which they are treated is driven by their active epistemic roles helping this research towards its goals. Open-endedness and future development of the concept is anticipated as part of that investigation. As such at the hands of researchers they need to be treated as dynamic constructions rather than static ones, for which any historical analysis needs to interpret them with respect to their associated historical research processes. You can't pull theoretical entity concepts out of their research environments and expect to have an understanding of what such concepts are or what they do that is faithful to scientific practice.

But I'll start by distinguishing my approach to concepts and conceptual change and referential continuity from another that is becoming more fruitful today: that of Nancy Nersessian's viewpoint on the construction of meaning in research practices and the cognitive approach. This is important because the cognitive approach to history of science aims precisely at showing how a revised view of concepts can give a more reasonable account of conceptual change in scientific practice than is available to the linguistic or meaning-based approach. My aim is to show how these more informed accounts of conceptual change and the case studies they give can be distinguished from, but also accommodated to, my theory on theoretical entity concepts.

5.1 Nersessian: Constructing Meaning in Scientific Theories

Nersessian's 1984 text on the history of the electromagnetic field concept from Faraday to Einstein, was to my knowledge one of the first texts to directly engage with the history of individual research practices and concept development, and to highlight the problem with a clear case study of the Kuhnian approach to conceptual change and continuity.[396] The ostensible purpose of this text was to explore how historically the electromagnetic field concept acquired and varied its meaning, within the 'network of methodological, theoretical and metaphysical beliefs' each scientist operated with, and relative to the problem situations

[396] Nancy Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Martinus Nijhoff, Dordrecht, 1984).

each scientist faced. As she identifies the field concept is an important case for assessing meaning variance because the field concept crossed several ‘paradigms’ from the 19th to early 20th century, including classical mechanics, electron theory, relativity theory and on into quantum mechanics. On Kuhnian theory this is the sort of concept for which continuity would have been abruptly severed upon such paradigm shifting.

Nersessian focuses on central individuals, rather than research groups or broader scientific opinion, namely Faraday, Maxwell, Lorentz and Einstein. In turn this historical study reveals that the field concept in terms of its representation underwent quite considerable change up until Einstein’s formulation from which modern concepts directly descends. Each individual departed to varying degrees from the conceptual formulations that preceded them, although each was committed to the basic field idea that ‘some physical processes take place in the region surrounding bodies and charges’ that causally influences them.[397] This was the basic field idea that each worked within and informed their development of the concept conceptually. On my account such an idea would be an essential part of the explanatory role and conceptual role of the field concept.

As she puts it the question that arises given this meaning variance is ‘what justifies our calling all of these conceptions ‘electromagnetic field’ and claiming they are related?’[398] Arguing for the more open-ended view of concepts as prototypes Nersessian proposes that a concept like the EM-field was structured according to its explanatory and descriptive roles, in this case the explanation of how continuous progressive action is possible and a description of how this action takes place. Fulfilling these roles evoked a meaning schema for the concept which is divided into four categories, its ‘stuff’ or nature, structure, function and causal power. Each contributed jointly to its explanatory and descriptive role. Meaning change can then be analysed relative to the assertions individual scientists made for each category. Nersessian thus breaks meaning down in this way, giving it category or dimension if you will, rather than treating it as a static collection of necessary and sufficient properties. The field concept may have changed between each episode in one or two of these categories but remained constant in the others. In effect then the question of meaning continuity is something to be treated as more complex than the either/or criterion of descriptive theories of meaning. For what counts as continuity for any category ultimately Nersessian falls back on Shapere’s ‘chain of

[397] *Ibid*, p 34.
[398] *Ibid*, p154.

reasoning connection' which posits continuity to lie in the reasons by which changes are made.[399] That is 'for each 'meaning schema', its earlier and later forms are both determined by and connected through the reasoning for initial introduction and subsequent alterations of the concept.'

Nersessian's reconsideration of meaning continuity doesn't go further than this and remains suggestive. I particularly appreciate the emphasis she gives to the various epistemic roles of the field concept - its explanatory and descriptive functions – as consistent roles relative to which the meaning of the field concept was structured into this schema. Although Nersessian doesn't pick this up there are more important roles for these functions than just setting the structure for assessing meaning continuity. They are elements of the continuity itself. The way I approach this issue of continuity is thus obviously different. Nersessian is still relying solely on 'meaning' to do the work defining and assessing conceptual continuity. A broader conception of what a concept is and what it's for I believe brings other elements into the assessment which stands to mitigate, or at least put in a broader epistemic context, the importance of descriptions.

Nonetheless I think many of the ideas she raises and indeed the observations she pulls out of the case study itself are instructive for my approach. I think she is undoubtedly right when she says the broader context in which concepts are used needs to be considered when examining the continuity of concepts and conceptual change. Conceptual change doesn't happen in a vacuum, but within a network of problems and beliefs, which for me are aspects of the research process for which the concept is central. As she puts it, 'In order to do justice to science the idea that meanings are independent of the processes through which they are constructed must be abandoned.'[400] I would put that the critical process here pertains to the research process in which the concept is employed. Meanings or representations are constructed within this context as part critically of an ongoing investigation. It's in this context that the open-endedness of concepts and conceptual change are to be understood. Importantly as Nersessian shows in each case the field concept was addressed to the same fundamental problem, the transmission of electric and magnetic forces. It is thus with respect to this problem that the concept itself was constructed and thus our construction of it should

[399] For example see D. Shapere, "Reason, reference, and the quest for knowledge", *Philosophy of Science* (1982) 49, pp. 485-525.
[400] Nancy Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Martinus Nijhoff, Dordrecht, 1984), p159.

go beyond descriptions to reflect this fact. Nersessian does this by drawing out a meaning schema that is directed at the elements required to perform these roles.

However I think there's more to it than this when we analyse the practices of the scientists exemplified in this case. The research process sets the purposes and goals (which it is often at the same time defined by) for which the explanatory, conceptual and experimental roles of such central concepts are themselves understood. The neutron concept's experimental, conceptual and explanatory roles were understood by virtue of the goal of analysing and understanding the nucleus and the interactions of subatomic particles. In the case of the electro-magnetic field, the field concept was part of an extended period of research and investigation concerning the operation of electric and magnetic forces and their relation to phenomena like light.

Although she focuses more on the concept's history itself rather than the developing research context around it, from Nersessian's account of the electromagnetic field concept and its use at the hands of certain individuals I think we can reconstruct her reading of the history of this concept in these terms. The field concept was grounded on its explanatory role, as an explanation for electromagnetic processes through the supposition of causal physical processes in the space surrounding the bodies. This acted as the principal reference point for the development of the concept as it was applied and reapplied to deal with a changing landscape of phenomena and theory, which included the Michelson-Morley experiment and the advent of the electron concept. It is clear that from the outset the field concept provided a frame of investigation with respect to which electromagnetic phenomena could be conceptualised and understood. It operated this way for individuals in the context of the development of their own ideas. Faraday, the initiator of the concept, used it as basis point upon which to develop his own ideas relative to its explanatory goals, particularly electromagnetic rotations and induction phenomena. That is, he worked from the principle that causal processes were operating in space to develop the explanatory mechanism of the concept, coming up with a lines of force notion.[401] Faraday's concept, as Nersessian reports, developed out of his feeling that induction could not be attributed to action-at-a-distance and that there should be some force operating locally to describe these actions. This vague idea, Faraday developed and worked with to put forward this lines of force conception which acted

[401] Through such texts as M. Faraday , "Historical sketch of electromagnetism," Annals of Philosophy (new series, 1822) 2, pp. 195-200, 274-290, and *Experimental Researches in Electricity* (1831), 3 vols (New York, Dover, 1965) and *Experimental Researches in Chemistry and Physics* (London, Taylor and Francis, 1859).

as the basis of his future research. With this he claimed a greater explanatory potential for electro-magnetic phenomena compared with action-at-a-distance viewpoints. Faraday was aware however of the open-endedness of the field concept. He acknowledged that the concept as he developed it was vague. He played with various interpretations of these lines of force, as line of stress and strain in space, or as properties of the aether. Nersessian exposes the ambiguity of these, and how Faraday struggled with them. But Faraday treated this interpretative question as an open research issue for which he didn't have a precise answer, but which did provide a path of investigation and a conceptual basis for it. Referring to such speculations on the interpretation of his field concept, he puts it,

"For not only are they useful in rendering the vague idea more clear for the time, giving it something like a definite shape, that it may be submitted to experiment and calculation; but they lead on, by deduction and correction, to the discovery of new phenomena, and so cause an increase and advance of real physical truth…"[402]

I take this to be an expression that should be understood in the way I've suggested: as an acknowledgment by Faraday of the existence of an ongoing research and investigation process, which his field concept should play a role in framing. Our understanding of Faraday's field concept should thus be informed by its relation to this research process: that is, as an epistemic frame through which the concept was further developed as he attempted to refine its causal-explanatory mechanism in the context of giving a theoretical account of electro-magnetic phenomena.

Open-endedness in this sense of 'vague' or speculative or unclear is one aspect of the open-endedness of concepts I have referred to in the previous chapters. Another is the disposition methodologically to treat the concept flexibly and not to pin it down by any particular description. That is, to consider the concept as something apart from such description, and thus open to redescription. In Maxwell's case he took the field concept from Faraday, believing like him in the transmission of actions as involving continuous transmission of force, 'as the starting point for the formulation of his own conception.'[403] In other words he carried over the explanatory roles assigned to physical processes in space surrounding bodies.

[402] Quoted in Nancy Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Martinus Nijhoff, Dordrecht, 1984), p66; from M. Faraday, *Experimental Researches in Electricity* (1831) 3 vols (New York, Dover, 1965).
[403] *Ibid,* p 69.

But his retention of Faraday's development of these roles was only partial, relying mainly on the line-of-force conception and not on Faraday's deeper ontological interpretations of them. It was not a case of linear development of Faraday's ideas. 'He replaced Faraday's relationship between the number of lines cut and the strength of the force with a continuous measure'.[404] He did most significantly however develop Faraday's lines of force into a mathematical representation of the field, expressed through his famous equations. The concept nonetheless became the basis of his own research on the subject of electro-magnetic forces, as he moved from the more instrumental construction of formulae to trying to give an account of the underlying mechanical bases for these formulae in terms of aether processes. Like Faraday he was open about treating the concept as part of a research process, and not as a static construction. His mathematical analysis in the first paper in which he closely examined the subject was rationalised by him as purely part of research development calling it a temporary theory, to guide experiment, 'without impeding the progress of the true theory when it appears.'[405] This was doubtless how he considered the role of his 'physical analogy' employed later to provide an imaginary model for the construction of these mathematical equations, and thus a basis upon which to systematically develop them. Substantially as Nersessian reports Maxwell quite dramatically changed his approach from a 'mechanical one' to a 'dynamical one', which dispensed with any need to model his laws on an underlying basis of a mechanical system, but formulated it instead from generalised presumptions about the forces involved and their relation to field states. Again the research dimension was to the fore with Maxwell as he noted that 'we should first investigate the forces with which bodies act on each other in the first place, before attempting to explain how that force is transmitted.'[406] His aim was to set some basis by virtue of the field concept for its further elaboration in line with its epistemic functions. 'The question is….what is the most general specification of the material system consistent with the motions of those parts of the system which we can observe are what we find them to be?'[407] It's only in later papers according to Nersessian that he begins to use his conceptual development to frame his consideration of the underlying causes, which he does by making use of the luminiferous aether concept, having established a

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[404] *Ibid,* p 93.
[405] J.C. Maxwell, "On Faraday's lines of force," *in The Scientific Papers of James Clerk Maxwells*, vol.1, W.D. Niven ed. (Cambridge, Cambridge University Press); reprinted in 1952 (New York, Dover Publications), pp 155-229.
[406] *Ibid,* p 88.
[407] *Ibid,* p 88.

connection between electric and magnetic fields and light waves.[408] He worked explicitly thus towards a physical explanation of the field properties and relations from states of the aether.[409]

As such for Maxwell the electromagnetic field concept was always a developing one in his own research, and the continuity we can trace is not just the commonality of ideas about the field concept Faraday and Maxwell shared (particular as lines of force) but that they both worked towards developing the concept starting from shared explanatory and conceptual roles of the concept (for dealing for EM-phenomena), while acknowledge the open-endedness of it and its part of an ongoing process of research in their hands. Again Lorentz treated the concept similarly as an open concept tied to the question of the operation of electromagnetic fields. He however according to Nersessian took his use of it in a new direction by trying to develop a framework for the interaction of matter and states of the aether. Hence he added a force law between magnetic fields and charged particles to Maxwell's equations. However Lorentz carried on Maxwell's investigations into the underlying physical basis of the concept to try to account for the Michelson-Morley experiment.[410] The field however was not for him to be treated as an independent reality. The field was merely a state of the aether. The aether itself however remained undefined and open-ended. Lorentz never attempted an investigation of it. Its assumption merely supported conceptually his field-matter interactions, particularly his contraction hypothesis. Einstein however shook the concept free of all these assumptions concerning the aether, implying instead that the field was ontologically fundamental. This effected a profound change in the concept of the field, Nersessian notes, but it wasn't adverse to the treatment of the field concept as an epistemic frame for ongoing research into the nature of EM-forces. Einstein through his own use of the concept as part of his theory of relatively, and through his research came to a different conclusion about the nature of the field. Einstein referred to the field in fact as an 'independently existing thing'.[411] The field was for Einstein a state of space, rather than of an underlying aether.

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[408] From 1864 with his paper J.C. Maxwell, "A dynamical theory of the electromagnetic field," *in The Scientific Papers of James Clerk Maxwells*, vol.1, W.D. Niven ed. (Cambridge, Cambridge University Press); reprinted in 1952 (New York, Dover Publications), pp. 526-597.

[409] Critically as Nersessian reports, Maxwell didn't realise that his aim for a mechanical account of the field was fruitless given his own field equations were non-Newtonian.

[410] See for instance H.A. Lorentz, "Simplified theory of electrical and optical phenomena," *Proceedings of the Royal Society of Amsterdam* 1 (1899), pp. 427 – 432.

[411] Quoted in Nancy Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Martinus Nijhoff, Dordrecht, 1984), p66; from A. Einstein, "Über das Relativitätsprinzip und die aus demselben gezogenen Folgerungen," *Jahrbuch Radioactivität* (1909) 4, pp. 411-462; p. 413.ö

All these events and developments in the field concept are historically connected not simply by threads of meaning or chains of reasoning, but by a common appreciation of the scientists involved that the concept of the field was an open concept that provided certain explanatory and conceptual resources to an ongoing process of research into electromagnetic phenomena and later its relations to matter and space and time. This open-endedness manifested itself I believe in the three ways we have noted: first in the acknowledge vagueness or uncertainty of the description of the concept, which scientists like Faraday and Maxwell acknowledged in their own presentations; second, in the fact that none of these individuals accepted the preceding concept as pinned down by its previous formulation. They took the concept as open to further development in the pursuit of the research process, and as an object of investigation in the furtherance of this, to be redeveloped to solve the outstanding theoretical problems. This redevelopment relied upon and was motivated essentially by the explanatory role of the concept, as the causal agent responsible for the non-instantaneous transmission of electric and magnetic forces, and its conceptual role making the space around charged bodies a theoretical variable for theoretical development in this regard possessed of its own causal properties. Thirdly the concept retained at various points its openness to new properties and behaviours being discovered of it, as itself an object of investigation. It was not fixed to a certain set during this period, as investigations of magnetic and electric effects were still ongoing. As such at times the concept, or constructions of it, was posed as an aid to experiment, and thus directly as a starting point for building up more information about the underlying entity itself.

By focusing on the ‘micro-elements’ of conceptual developments – the changes in description – Nersessian misses I think the broader and encompassing aspects at work that overlie these developments and make this change part of the consideration and use of such theoretical concepts in their complex research environments. These roles as I’ve described them formed the basis of the concept’s use and redevelopment, and ultimately of its continuity, in this dynamic process of investigation. Individuals did not feel bound by older formulations but perceived the necessity of their reformulation in order to use the field concept to give better accounts of the developing theoretical and experimental information they were faced with.

5.2 Conceptual Change and Cognitive History of Science

Nersessian’s work on the electromagnetic field was composed shortly before it seems she became one of the main protagonists of the cognitive history of science movement. There are many names involved in this such as Ron Giere and Alvin Goldman, and Paul Thagard who was mentioned already in earlier chapters.[412] Selling itself as the application of cognitive theory to history of science, the program endeavours principally to apply cognitive analysis to scientific processes of discovery and information communication, thereby challenging the now rather archaic idea that the context of discovery, as opposed to justification, is beyond the possibility of meaningful investigation. There are processes involved in 'discovery' that can be constructed as cognitive elements of scientific practice. In Nesessian's words cognitive history of science, "attempts to reconstruct the cognitive dimensions of the process through which vague speculations get *articulated* into scientific understandings, are *communicated* to other scientists, and come to *replace* existing representations of a domain."[413] As such it investigates historical episodes by identifying the cognitive practices employed, such as the use of heuristic devices, mechanisms, analogies and pictorial representations, thought experiments and so on. "The underlying presupposition is that the problem-solving strategies scientists have invented and the representational practices they have developed over the course of the history of science are very sophisticated refined outgrowths of ordinary reasoning and representational processes."[414] The cognitive approach thus aims to make cognitive processes explicit in historical discussions (where they've usually been implicitly active), elaborating contextually how that such psychological tools and models are expressed in varying social and cultural situations.

However the real interest for us about this approach challenges traditional accounts of meaning continuity and seeks an alternative explication of episodes of change. In criticism of Kuhn and Feyerabend, Nersessian notes how their use of 'gestalt switches' to describe the psychological processes of conceptual change as a result of meaning change merely embeds the context of discovery/justification distinction, by focusing only on the end-points of conceptual change rather than the processes of it. As Nersessian puts it "Historically, however, we did not get to relativity without at least passing through electromagnetism and the theory of electrons, and this developmental process is central to understanding such questions as the nature of the relationship – or reason for the lack of relationship- between,

[412] Paul Thagard, *Conceptual Revolutions* (Princeton, Princeton University Press, 1992); Ronald N. Giere, *Explaining Science: A Cognitive Approach* (Chicago, University of Chicago, 1988); Alvin Goldman, *Epistemology and Cognition* (Cambridge Mass.., Harvard University Press, 1986).
[413] Nancy Nersessian, "Opening the Black Box: Cognitive Science and History of Science", *Osiris,* 2nd Series, Vol. 10, *Constructing Knowledge in the History of Science* (1995), pp. 194-211; p. 194.
[414] Nancy Nersessian, "How do scientists think? Capturing the dynamics of conceptual change in science", in *Cognitive Models of Science*, ed. Ronald Giere, Minnesota Studies in the Philosophy of Science (Minneapolis, University of Minnesote Press, 1992), pp. 3 -44; p. 5.

e.g. different concepts called by the name "mass" in each theory."[415] Again, the traditional focus on concepts as purely linguistic structures merely serves to make conceptual change seem mysterious and disconnected. There is as such very little analysis of the theoretical practices scientists employ in respect of conceptual innovation, but Nersessian is sure with a cognitive approach, such practices exist and can be identified. For instance the observation can be immediately made that generally people, and scientists too, don't represent concepts by necessary and sufficient conditions (or propositionally) but treat them as interpretative devices with which to construct mental models for dealing with particular situations. The kinematics of change should thus be represented through overlaps in successive mental representations and their salient features, as in the Faraday to Einstein case. Dynamically the issue is how conceptual change is tied together by common cognitive strategies and devices scientists employ. A dynamical theory of continuity thus analyses how old conceptual structures are converted to new ones through the uses of these devices and strategies relative to particular problem situations. In this respect Nersessian has focused for instance on the role of 'analogical and imagistic reasoning', 'thought experiments and limiting case analysis', and 'abstraction techniques'.

Thagard also writes in this vein.[416] His focus is generally on conceptual frameworks and on revolutionary change, and also on the manner in which belief shifts occur from one conceptual framework to a competitor, based on a neural network theory of conceptual relations. Revolutionary change is analysed in this way as a kind of schizophrenia in the minds of scientists who know how to employ each competitor until the weight of success by one means abandonment of the other. This dual use is enabled partially by conceptual overlap between the two frameworks. However importantly for us as we noted in chapter 2, Thagard also develops an accumulation or 'accretion' theory for individual frameworks, which is analysed in terms of shifting relations (such as 'kind', part/whole and explanatory) between concepts of these frameworks during a research processes. As such Thagard is very interested in what roles an important concept, like a theoretical entity concept which motivates a new conceptual framework, might play. In the second chapter of this dissertation we talked about his example of the development by Lavoisier of his oxygen theory of combustion.[417] Thagard

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[415] *Ibid*, p 6.
[416] Paul Thagard, *Conceptual Revolutions* (Princeton, Princeton University Press, 1992); Paul Thagard, 'The Conceptual Structure of the Chemical Revolution,' *Philosophy of Science* (1990) 57, pp. 183-209.
[417] Relying on works such as A. Lavoisier, *Oeuvres*. 6 vols. (Paris, Imprimerie Imp6riale, 1862); *Essays Physical and Chemical* (1774), Trans. by Thomas Henry of Opuscules Physiques et Chimiques (1774), Second English edition. (London: Cass, 1970)); *Traite' Elementaire de chimie* (1789)

attempts to formulate the particular heuristic rules relied upon by Lavoisier to build up a conceptual framework around the new concept. He thus provides what I would consider a discussion of various epistemic roles of Lavoisier's new concept, particular its conceptual roles as the element of a conceptual framework, but doesn't himself consider what might ground the continuity of individual concepts themselves. What made Lavoisier's original use of the oxygen concept formed in the 1770's coextensive with his later one of the 1780's? Thagard's unit for analysing such questions is broadly the conceptual framework. He discusses continuity in these terms. Nonetheless as with the 'field' example of Nersessian's, Lavoisier's development of the oxygen concept clearly belies a continuity to the concept dependent on its essential part of a continuing process of research with certain epistemic aims and goals hand. This depended on it having conceptual and explanatory roles reliant on very basic ideas about a new principle of combustion and calcification as the result of the input of a substance (rather than release in the case of phlogiston), by which Lavoisier was able to reformulate the concept as a continuous process of development from 1772 to 1779. It seems to me that Lavoisier interpreted himself to be developing the same concept or the same vague idea throughout despite its dramatic reformulation at his hands from a property of air, to an 'elastic flexible fluid', to a component of atmospheric air. As already point out however Thagard in terms of conceptual change is more interested in tracing how belief shifted from the phlogiston theory to the oxygen theory in Lavoisier's mind as his theory developed.

With respect then to the cognitive approaches to conceptual change then I think it is not inconsistent with my approach and in varying degrees supports it. Nonetheless our approaches can be distinguished. Firstly the cognitive approach is addressed more to the question of explaining the process of conceptual change, rather than explicitly the kinds of issues I have raised regarding the preservation of concepts during research processes. For them this is the principal *explanandum*. As such explicit answers to the issues I've raised about scientific practice regarding theoretical entity concepts aren't directly addressed. However that said, cognitive theorists by attempting to expose the cognitive mechanisms underlying conceptual change, undermine any claim that there is fundamental discontinuity between such conceptual shifts, by showing how such cognitive methods reformulate established concepts. This leads to my second point however that I still think there is more to the continuity of concepts like theoretical entity concepts in the minds of scientists than just historical relations of reformulation or chains of reasoning. This in itself I think is insufficient to explain the attitudes of continuity regarding their central concepts that researchers seem to have possessed. Changing an established concept doesn't itself explain why researchers seem to believe that this action is part of the redevelopment of the concept, not the construction a new different concept from the old. Something else, an overriding methodology, seems to be required to explain this.

Likewise there's only so far this can be made to reflect a practice of relating concepts. Was the very first field concept of Faraday related in the minds of scientists to the later one of Einstein, including by Einstein himself, by tracing the links of conceptual development, noting that on their face the concepts were quite distinct in many respects? It's surely not the case that they trace the individual paths of conceptual development, relating each step through cognitive processes, and believe in the continuity of the concept from being able to do so. In most episodes of scientific practice scientists accept a certain version of the concept and then proceed to develop it or change it without discussing its history. Nonetheless they still connect their use of the concept back to original issues in the context of a research process. Thus it's more important I feel that scientists are aware that these processes are part of an overall process of research governed by certain goals and aims which instantiations of the concept share and which are driven by its open-endedness and epistemic roles towards solving these problems. Of course this is not to deny that scientists do historically find chains of reasoning and understand the steps leading from older concepts to theirs. This is usually very important to how they themselves trace concepts historically when looking back to rationalise their own conceptual development. But this is not always part of how they identify the concept, and as mentioned before, even if was, would not be in itself adequate to explain the attitude of continuity. In any case the situation in practice is much more complex than simply a succession of individual uses of concepts. Concepts like theoretical entity concepts are under active development across many groups and research divisions. They cross disciplinary boundaries and emerge in different fields. What ties these concept instantiations together is not in the minds of their practitioners conceptual reasoning from step to step, since they might not share any. Even within a field there's often too many concept instantiations flying around for scientists in practice to be able to group them in this way and treat them as developments of the same concept through tracing links of conceptual development. Chemists in the early part of the century didn't trace the links of conceptual development of physicists back to a common original concept as the basis upon which to hold that they shared a common electron concept. Rather I would assert they both pursued the concept on the basis of an overlapping project of research, and shared roles of the concept for that research, which included in either case attempting to understand the properties and behaviours of the entity in question. That's why I assert there must be something broader which facilitates and promotes wider development in this respect.

However that said, there are two main points at which I distinguish the cognitive-historical approach to continuity and conceptual change from my own. Firstly I make no general claim about all scientific concepts. My focus is on centrally important, historical, theoretical entity concepts. And secondly in respect of this, I make the assertion that their continuity historically is not to be reduced strictly to cognitive processes, and being able to trace back through cognitive steps manipulating concepts or conceptual frameworks, but rather lies at the level of active methodological decisions by the scientists involved as to how to manage research with a set of theoretical concepts. Methodologically then, historical continuity lies partially with the acceptance of a broader understanding and flexibility with respect to theoretical entity concepts on the part of scientists using them, refraining generally at individual and group levels from fixing concepts by their descriptions in the face of uncertainty and complexity (and lack of access to the underlying entities themselves), thus interpreting these very cognitive processes that develop the concept to take place as part of ongoing investigation of the same entity in the context of the same research processes. The way scientists construct their concepts in this instance is under their control, and is not to be purely derived from cognitive processes.

5.3 Arabatzis: Conceptual Continuity as Biography

Lastly it is important to distinguish my approach from that of Theodore Arabatzis', which has already been mentioned earlier as an important contribution to just this kind of more epistemically inclined historical analysis.[418] On the question of conceptual continuity, Arabatzis is very much concerned firstly with the historiographical implications of being unable to trace concepts through their referential relations historically, which seems to be the inevitable results of applying strong descriptive theories of meaning to such concepts, and the question of realism, more precisely whether or not we can ground an idea of continuity that could be consistent with realism.

[418] Theodore Arabatzis, *Representing Electrons: A Biographical Approach to Theoretical Entities* (Chicago and London, University of Chicago Press, 2006)

I think there are several points at which my approach is distinguishable from what Arabatzis is doing with his biography of the electron. In the first place the focus of Arabatzis' study is essentially the interaction between old representations and new representations of theoretical entity concepts, and how the former both constrain and resource the later. This interaction is one well primed it seems for discussing the electron concept in biographical terms as an active participant in its own development. My interest however is not in these dynamics per se, and I haven't specifically investigated how older representations of a theoretical entity historically influence its future development, not outside at least the properties associated with the epistemic roles of the concept, but rather what more broadly sustains them as part of the developmental dynamics regarding a single theoretical entity. My position is that this interaction happens within and is structured by the epistemic roles of these concepts, and of particular importance is the open-endedness of the concept which facilitates these kinds of representational conflicts and problems to emerge in the context of the discussion about the properties and nature of single theoretical entity. As such in the electron's case it seems that the electron concept itself was framed by its explanatory roles, explaining spectral lines for instance, and cathode rays and atomic structure. It was designated certain experimental roles for studying and producing phenomena like spectral lines. Conceptually it provided a tool for organising and understanding the laws at operation in the microscopic world. Within the context of these roles the concept developed, and transformed at the hands of Bohr and others, with significant representational problems continually emerging. Arabatzis documents these.

As such my analysis works at the outer levels of the processes that govern representational change. This is the essential idea behind referring to a theoretical entity concept acting as an open-epistemic frame. This applies too across disciplinary boundaries and can serve I think to help understand why different groups working with the same entity although on different sets of problems and with different conceptual frameworks might nonetheless believe they are applying the same concept with the same referent. Arabatzis addresses the dispute between chemists like, Lewis and Langmuir, and physicists over the nature of the electron, particularly whether to consider it as static or dynamic in the structure of the atom. This is an interesting case study because I think the points Arabatzis makes about what was driving their common reference and my approach overlap here. Both groups as it turned out took themselves to be referring to the same referent when they diversely developed and debated the electron concept in the early part of the 20th century to the point of incompatibility. Arabatzis identifies that the explanatory roles for both groups were the same (spectroscopy, chemical bonding, periodic table..), and likewise they agreed on experimental manifestations of the electron. It was also serving similar conceptual roles being employed as an object of the same physical language in both cases within which for instance new physical forces could be hypothesised and proposed for the electron in the course of building up models of the atom. This gave them a common frame for the investigation of the electron while they variously worked towards solving their own pressing problems which gave rise to the conflicting dynamic and static accounts. I think in addition the groups shared the concept as open-ended and open to investigation on the basis of the open state of knowledge about it and the continuing state of investigation, and also the common goal of understanding it, which can be pictured in the context of overlapping research processes which centred around the structure of the atom and thus incorporated explanatory goals of the other discipline in this respect. I agree then with Arabatzis that this acceptance of each other's goals and overlapping programs of research which required descriptions and investigation of the electron itself within the frame of certain shared epistemic roles, provided the space for these diverse viewpoints on its structure as part of these ongoing investigations and the sense that a better electron description would need to resolve these inconsistencies in their accounts.

Something which is uncertain to me about Arabatzis' approach is whether or not he thinks we should talk in these instances about conceptual continuity or just referential continuity. I get the feeling Arabatzis would suggest that one can't describe these historical processes as regarding the same 'concept', rather than families of different concepts with the same referents. In this sense he can and does talk of physicists and chemists holding different 'conceptions' of the electron. My approach is of course different in this respect because I take the perspective that such theoretical entity concepts have a history of being treated as open-ended and not affixed by certain descriptions. Thus I discuss these historical episodes involving the concept in terms of conceptual continuity by identifying the concept principally with a set of epistemic roles (its construction as an epistemic frame) and the basic properties and relations that base these with which those descriptions are worked out in the course of research. I'm not sure whether talking of the 'open-endedness' of a concept or describing them as frames would make much sense without being able to refer to some degree of conceptual continuity, which of course is meant to distance the dependence of concepts on descriptive theories of meaning or more strictly on meaning itself. It's clear that Arabatzis doesn't seek to do that. Representations are his chief objects of analysis. And these are of course elaborated in terms of their semantic content. For me though it seems that with reference to practice talking about the continuity of concepts makes more sense, since part of the dynamics of science is having these consistent elements that are shared. Arabatzis talks of 'core meanings' for instance as an aspect of the electrons various representations. I talk of consistent epistemic roles and their constitutive properties. My use of 'concept' is meant to capture these continuous elements which are mentally tied together. It is also there to capture the sense in which new representations rely on and build on old ones, and the flexibility of a concept to incorporate new rules and information. These research practices as we've identified are dynamic and rely not on the continual creation of new concepts for the researchers involved but the developments of the ones they have. This inevitably cuts across what researchers might report is happening in different circumstances. Some might read a situation as involving different concepts of the same thing. But ultimately I take them to mean to be saying that their elaborations of the concept are different, where otherwise it seems from the context a case where scientists consider the same object is being investigated within a shared research process governed by shared epistemic roles.

If we do consider there to be an underlying practice of referential continuity in these situations with theoretical entities there needs to be something continuous by which it is identified, and if picking out actual entities is what theoretical entity concepts are precisely employed to do in practice, then associating 'concepts' with the elements of referential continuity seems fundamental to reflecting practice. Concepts after all, are primarily tools of representation, and it is ultimately their attempts to represent something in the outside world and the belief that they do that pins them down. The idea of concept I impute to scientists is thus bound up with the consistency of beliefs in reference and the elements and roles that facilitate this.

However it might be the case that nothing particularly turns on the question whether or not we call these different representations of the world, different concepts with the same referent or just the same concept with different descriptions and the same referent. What really matters is what those elements of continuity consist in, in practice, and how these are grouped by researchers in the context of research processes. The latter however seems more natural to me for capturing the scientific practice of keeping representations somewhat distinct in practice from the referential and investigative (and other epistemic) aspects that have been associated with theoretical terms like gene and electron. What we attribute to scientists in the context of research is a much more fluid and flexible conception of what a concept is in terms of its descriptive content, at least for theoretical entity concepts, where descriptions of the underlying entities are concerned. In my terms such concepts become a set of means for extracting and interpreting information in terms of the entity, in furtherance of research processes. And part of their role in fulfilling these roles contributing to understanding the phenomena at hand, is achieved invariably through better descriptive accounts of the underlying entity concept. So the function of such concepts is not just to represent, but also to represent ways or means of pursuing representations, and which are bound up with formative and non-determinative epistemic roles as I've described them.

This said, I would still note that there is much in common between my approach and Arabatzis'. As I suggested in the first chapter, I think many of the features he observes about entity concepts that lend them to accounts as biographical agents, specifically the electron concept, are related to the more epistemic account of the roles of these concept I'm giving here. Arabatzis talks of the heuristic roles for instance of the electron concept which I think are to some extent manifestations of its epistemic functions in practice, providing scientists certain resources and constraints to redevelop the concept in the face of new data. And while I don't address the processes directly that led to the transition from one representation to another, and thus the aspect of resistance that an entity has to being described in a certain way, I think what's driving this process can itself be attributed to epistemic roles as I've identified them and the attachment to ongoing research processes that such theoretical entity concepts have. It is partially in the methodological open-endedness with which such concepts are applied that we can recognise such historical resistance as the pursuit of the entity in the context of a common process of research according to certain epistemic roles through which the information acquired may not meet with expectations. But within the frame of these epistemic roles the phenomena can still be attributed nonetheless to the entity and demand descriptive change. Clearly part of their activity is that they play roles as I've suggested in my case studies providing investigative directions for research processes. There is as such in the case of these epistemic roles some space in which we could expand the biographical approach and our understanding of the active historical nature of theoretical entities.

5.4 Conceptual Continuity and Reference in the Research Context

So having surveyed some of the relevant existing perspectives on mainly conceptual, rather than referential, continuity, I want to give at least the beginnings of an insight into how this epistemic characterisation of the involvement of theoretical entity concepts in ongoing research processes has been the basis of conceptual and referential continuity in practice. By doing so I hope to provide an alternative to the tradition of deciding questions of continuity with meaning-based criteria. In the introduction I listed these observations regarding scientific practice as somewhat problematic on meaning-theoretical accounts of conceptual continuity and reference as common aspects of scientific practice.

1. Scientists have in many cases persisted in using a concept, and maintaining a belief that they have been continually redescribing the same thing, even though such redescriptions have involved significant and dramatic shifts in the representations of the entity involved. (e.g. various subatomic particles, the gene concept)
2. Significant aspects of an entity's representation may remain unsettled or unexplained. These may pertain to significant ontological questions about the entity, such as what natural kinds it belongs to, how it is internally structured, what might be said to be its 'nature' or constitution (which might require theories of interpretation), or how the collections of properties or causal powers assigned to an entity might be explained where there is a want of explanation (with models of internal structure or models of behaviour – see continental-drift theory). In which case it might be said that there is no real clear picture of what the entity is, or what provides its causal powers, beyond the bundle of properties associated with it and the implications of those powers. Certainly there is nothing that might be classed as an essential description of the object that might be an answer to the question 'what physical properties of X uniquely determine its extension?'(the initial gene concept for instance, the early virus). It simply might be left open for lack of information. Yet lacking these kinds of descriptions does not seem to stop scientists believing their entity concepts refer to something in the world.
3. In other instances there might be disagreements over such descriptions or interpretations, but still significant agreement that there is a thing being referred to independent of these and despite this debate (atomic structure debates, models of the electron). Thus a kind of principle of charity operates. Scientists implicitly agree that they are talking about the same thing yet disagree over significant aspects of it.
4. In some cases scientists think that, where there has been an acknowledged conceptual shift and an entity replaced by a new one in their explanations of a certain phenomena, they regard the old concept as simply referring to the new, as though they were all along referring to the new even while describing aspects of the old. (e.g. the aether and the EM field). In other cases they don't (e.g. phlogiston and oxygen).

5. Theoretical entity concepts seem to share the same reference ascribed by scientists even across disciplinary boundaries and the different uses and descriptions of the entity given by those various disciplines (e.g. the chemists' and the physicists' electron of the early 20th century, and historically before the 60s, genes across molecular and classical biology).

I treat these as more or less observations of scientific practice, and the way scientists have talked and discussed their research. They of course can be traced to a methodological open-ended treatment of such concepts which we've readily discussed. I analysed this into three elements of the acceptance of vagueness/incompleteness, a disposition to change descriptions, and an unwillingness to close the concept in lieu of ever possible new information about its underlying entity. Of course I've given no strong historical evidence to support all these claims generally, but I have in the context of the two case studies I've already put forward, and I'm confident it extends on the bases of historical studies I've also alluded too, such as Arabatzis' account of the electron or Nersessian's work on the EM-field. They point to a strong historical practice where theoretical entity concepts are involved to interpret the development of those concepts continuously, and to maintain even if mostly implicitly, a continuity of reference throughout their investigations and across disciplinary boundaries.

What I take these practices to indicate however is that there are referential practices of science regarding theoretical entity concepts that simply haven't been compatible with any strong meaning-dependent account of reference (descriptive theories), and that something other than strict meaning criteria were at work in practice. I think we rightly see this when we stop interpreting theoretical entity concepts for these purposes simply as bundles of descriptions or bounded defined structures and realise that their use, and the continuity assigned to them by practicing scientists, has occurred in investigative contexts where research is ongoing. We might of course ascribe to scientists a weaker theory of reference that relies on the transmission of only essential properties or meaning-determining ones, but this is compromised by the fact its often been the case that these too have been they have been redeveloped in the process of research without nonetheless disturbing the generally accepted continuity of reference.[419] The fundamental question is then, what underlies these practices

[419] Nersessian hinges reference to simply the ontological aspect of her meaning schema (the 'stuff' category) which simply raises the same problems in this case since 'stuff' criterion have often proved continually variable despite a practice nonetheless of referential continuity. Nancy Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Martinus Nijhoff, Dordrecht, 1984).

historically, and has sustained the continuity of such concepts while supporting the methodological treatment of them open-endedly.

So here's what I want to propose to explain these practices of conceptual and referential continuity, which I'll support with reference to my two case studies. Firstly, I want to say that conceptual continuity and referential continuity have been linked generally in the case of theoretical entity concepts, such that reasons for conceptual continuity are also those that ground a continuity of reference. This continuity then in practice has been I believe related essentially to these epistemic roles and the properties constitutive of them for the research processes in which these entity concepts have been centrally involved. These epistemic roles not only provided a basis for organising the research itself (choice of investigative path, and conceptual, experimental and explanatory roles), but also provided the basis for interpreting phenomena relative to the entity concept, and for investigating and developing it further. They formed in other words some of the basic means for constructing and reformulating descriptions or representations of the entity in the context of a process of research. Indeed it has been typical practice with regard theoretical entity concepts, for research processes, to pursue paths of investigation that sought to expand upon and also give better descriptions or understandings of the operations of these epistemic roles, because these roles acted as sources of information for researchers involved about how the underlying entity was connected to the world, whether in the context of experiment, or conceptually in the context of theoretical claims and beliefs. It is a good indication of just how embedded these concepts have been in their research processes, that their own investigation was framed by its very roles in this research, and the connections they imputed.

If scientists within a research process then agreed across time or disciplines about these properties and epistemic functions, and shared a usage of the concept in this way, then they could agree that although they might have reached different conclusions about the underlying entity in the course of their research, given the information they had, or may have had to change their minds about it as more arose, they could agree that they were nonetheless developing the same concept and talking about the same underlying entity, since they shared basic modes of examining it, acquiring information about it, and developing descriptions of it despite different and varying informational contexts.

This is only half the story however. The other important aspect is that the use of the theoretical entity concept took place within the context of a process of research for which the concept itself was a central part of reaching the goals of that process. These goals were generally composed of an ambition to understand or comprehend a certain general class of phenomena or objects (itself developing or expanding). The concept played its epistemic roles in this regard. As a more or less continual process of investigation into generally complex and uncertain phenomena scientists didn't ally such theoretical entity concepts closely to particular descriptions, but appreciated that as part of this research the concept would develop generally as part of their attempts to reach their research goals (goals which almost always required an understanding of the underlying entity itself and its behaviours). Of course their theoretical nature made this a more important methodological attitude in the context of complex and uncertain phenomena where degrees of unobservability were involved. Hence the open-endedness of these concepts in practice reflected the association of a theoretical entity concept with ongoing processes of research and the variability of its representational content for practicing researchers.

As such theoretical entity concepts have been continuous and can be traced continuously through processes of research because scientists treated these concepts open-endedly and agreed about the basic elements by which that concept would be developed and investigated, and ultimately revised. Thus the continuity of these concepts and their consistent reference for the scientists involved is very much to be interpreted with the context of continuing and open investigation, and the epistemic participation of these concepts in this regard. Let me then say a bit more about how these epistemic roles I've been talking about could have this kind of significance. Firstly the basic explanatory roles assigned to the concept as part of its function in a research process, have acted as both resources and constraints for descriptions of that entity. Description of the entity and its behaviour would work towards if possible an account of how the entity performs this explanatory role, where as I emphasised, the mechanism at the outset might be unknown or might need to change. In this sense it would have provided an investigative role too, by giving a frame for their further investigation often to lower levels. For instance the attribution of spectral lines to the bound electron acted as an explanatory constant for electron theory, such that Bohr could orientate his account of the electron by this consistent explanatory goal. Later theorists pursued the electron in the same way and more broadly the principals of quantum mechanics by virtue of this relation even as spectral phenomena became more complex. The commitment to this explanatory role in fact governed significant changes in the electron's representation. The essentiality of such explanatory roles to the broader processes of research to which such entities are essential meant that it was treated as more or less basic to, or constitutive, of the way the entity concept could be further developed. It was after all often the reason for the presupposition of the entity in the first place.

Conceptual roles have manifested themselves as roles in the construction of theoretical frameworks in the creating theoretical solutions to research problems. These roles usually themselves depended on certain presumed properties about the entity, which I'm labelling as constitutive because of their historical centrality to these roles. The unit nature of the gene concept has been largely constitutive of the conceptual use of the gene. At their base these concepts provided a resource for constructing theoretical frameworks towards solving specific problems theoretically, or crafting particular theoretical positions, based on these properties, around which kind relations and part-whole relations to other concepts could be generated. What we find is that these conceptual roles tended to grow in complexity over time as better solutions to these problems could be given, but the application of the concept to their constructing through these properties were fairly constant on the basis of basic property assumptions. The conceptual role of Lavoisier's oxygen principle seems fairly constant across its development at his hands as a unit of calx distinct from metal and a property or unit of air. Air would combine with metals to produce calx. On this basis his theoretical structure evolved. As such the actual theory could develop significantly and change substantially but the conceptual role of the theoretical entity concept in that theory remained central. Its in such a context that the concept itself was elaborated in order to adapt it better to the data and developing information regarding the problems it was applied to, and its thus through its participation as a basic element of theoretical structures that the description of the underlying entity could be refined. We'll emphasise some good examples of this in the case of the gene and the neutron concepts below. These conceptual roles were also very important in the identification or interpretation of aspects of the world in terms of the concept. The gene was looked for and identified at a lower biochemical level in terms of its roles as a unit of Mendelian laws and principles. Likewise Lavoisier's agent was sought out in terms of its conceptual role dealing in the theory of combustion and calcification as a part of air and a unit of calx. On this basis it was later identified as oxygen. As such conceptual roles played an important part in the interpretation, investigation and development of the concept, and although these conceptual roles, led to different developments of the concept, they nonetheless acted as a basis for them and thus as a shared means for interpreting the aspects of the phenomena in terms of the entity and investigating it further.

Experimental roles, the roles a theoretical entity concept might have in the experimental investigation of phenomena, were also constitutive in this sense, since often they were the basis by which experimental information was obtained relevant to the entity itself. In research processes, theoretical entity concepts have often been relied upon to produce new phenomena in the context of investigation, which then itself required the redescription of the entity. There roles were often not neutral in this regard. This is particularly the case where the experimental roles relied on the proposed causal powers of the object in question, and thus the investigation of the object itself depends upon what could be produced with those powers, or how conditions could be varied etc to test those powers in different conditions and so on.

So by agreeing on what framed their investigation of the theoretical entity, and how information relevant to it could be obtained, scientists could allow in the context of research for development of contrasting ideas about the entity and disagreement about them.
Researchers disagreed for instance over how the entity performed its explanatory roles, and developed different accounts of aspects of the entity concept in the process, but all recognised the centrality of this explanatory role to the use of the concept in the context of their research, without which the justification for the theoretical entity might well have disappeared. This is an indication of how important explanatory roles have been for theoretical entity concepts, as essential to the hypothetical existence of the entity and thus the presumed functionality of the concept for researchers' ongoing investigation and for the identification of aspects of the world with that concept. They might in addition have developed the conceptual role of the concept towards solving a specific problem in different ways giving in the process different accounts of aspects of the entity. Experimental results might have been interpreted differently or an experimental role developed through different uses of the entity, even though these experiments were nonetheless attributed to the action of the same entity. How these roles have been used and interpreted has varied from context to context, and historically, with the amount of relevant information scientists have: a continually changing situation. There is nothing necessarily conclusive or determinate about them, in fact there has usually been a significant underdetermination regarding their use and application, which marries with the open-endedness of their own description, but they have nonetheless acted I believe as resources and constraint which helped direct investigation and descriptive development. They

provided a common reference point, or better ‘reference frame’, which provided elements necessary themselves for the further investigation and development of the underlying entity. That is my assertion at least. As a result scientists understood developments in a theoretical entity concept as part of a shared investigative process with the same referent. I think this is a critical element in conceptual and referential continuity in the context of ongoing processes of research, where the concept then was conceived much more open-endedly and reference is based on the idea that these epistemic roles provided a degree of connection with the underlying entity necessary for obtaining further information relevant to it, to interpreting that information in terms of the entity concept and developing the understanding of the entity further. In other words, when basic elements of the means or frame of investigations were considered stable, developments as a result of using those could be understood as an investigation into the same thing for which different varying descriptions may well be reached as a result of applying them in the context of ongoing research.

What is nice about piecing together practice in this way historically is that it works as well for individual research as for developments at the group level. When an individual developed a new theory involving a theoretical entity concept, then often that theory went through many changes, before something satisfactory was reached. But for them the concept was playing these epistemic roles in their own theoretical development. One can see that with studies which elaborate the steps of an individual’s theory construction, as new phenomena arose and his or her own thought developed. Conceptual continuity is identified with this continuity of investigation in this respect, and reliance on certain conceptual and explanatory roles that helped structure and orientate that research. At the group level it explains the way referential views have tolerated shifts in the descriptions of concepts even to what might be considered the very most basic understanding of what the underlying entities are without conceptual or referential rupture. It is always the same concept and the same object under investigation to the scientists involved. It also explains how scientists using the same entity concept in different disciplines might have incompatible accounts of the entity but still believe in the same underlying referent and a conceptual continuity between their ideas. This occurs when the researchers of the different groups share common research goals which encompass understanding the nature and behaviour of the entity, and the entity shares epistemic roles for each discipline (i.e. shares explanatory, conceptual or experimental roles). This grounds the idea that they are both investigating the same object where the concept remains open-ended in the context of their overlapping but ongoing research.

This is all quite theoretical of course and rather schematic, but I'll try to give justification to these ideas in the context of our case studies. Of course scientists themselves don't generally outlay precisely what their criteria of reference or continuity are or were, so our method here will be to work through the case studies again to tease it out, as really the best understanding we can have as to what was being relied upon in these ongoing research contexts.

5.4.1 Conceptual Continuity, Reference and the Gene Concept (1900-1953)

Perhaps more than any other concept of science the gene has invoked these issues of referential and conceptual continuity amongst philosophers of science. There is now already a significant literature discussing the continuity of the gene and its consistency of reference. The reasons are not difficult to understand. On the one hand the gene seems to be one of the most central concepts of modern biology. Throughout many fields the term 'gene' is constantly used, but on the other it doesn't seem amenable to any one stable definition. I would say the majority of philosophers that look at the gene concept today argue that there are both conceptual ruptures and referential shifts throughout its history, particularly with the development of the gene at the hands of molecular biologists, claiming thus that any modern molecular gene concept is not comparable with, or reducible to, the classical ones of the early gene period, or of its descendant, modern population genetics. What interests philosophers most in this respect are the frequent shifts of definition applied to the gene concept from its inception, and the oscillation between its treatment as a unit of structure, a unit of function, recombination or mutation, to potentially no natural kind at all. Histories often treat these definitional shifts as quite revolutionary changes in the gene concept. As such these histories have fixated on the discontinuity of definitions as though scientist's own viewpoints about the gene were fixed to them, making these changes reflect significant referential shifts for the scientists involved. What I want to do here then is argue that at least until the *cistron* test of Benzer in the 50s that this picture is misleading, and does not take into account the ongoing processes of research for which the gene concept was the central concept throughout this period, and within which the gene was continuously developed. Indeed the mark of my claim here is that for the most part the biologists and biochemists themselves involved did not sense any radical discontinuity, but perceived the gene as undergoing a process of investigation, within the frame of certain epistemic roles which were constitutive of this very research, and to their development and investigation of the gene itself. In other words, the gene concept in this period is a good illustration of the conceptual continuity and referential practices I've been describing above.

Indeed if you read the research literature itself, it's hard to pick up signs of conceptual or referential rupture. Some papers worry about how to define the gene concept or whether the concept is being used ambiguously[420]. They attempt to come up with clearer definitions, but for the most part most users of the term up until the 50s and Benzer's work presumed a continuity of the concept with older previous uses right back to Bateson presence-absence hypothesis, such that in their own histories of the development of the concept or past debates they presumed that the same concept was being developed, different formulations of the gene concept were being decided for or against, and thus that the same putative entity was being investigated and talked about. The language of researchers reflected this. Sturtevant in his historical account of genetics, as an actual participant and witness (he was a student of Morgan's), characterised the 'biochemical approaches to the study of genetics' as a study of the biochemical effects of genes, which implicitly presumed the separation of the distinction between the pre-established gene concept and this investigation.[421] Bateson is thus said for instance to have noticed that 'gene action' was very similar to the action of enzymes, and therefore might operate through the production of enzymes.[422] Sturtevant doesn't pretend to give an account of what genes are in his history, which at that time in 1965 seemed to him still an open question. His conceptualisation of the gene was thus independent of this information, presenting it almost always as an object of investigation not definition. Along with the tenets of this 'open-concept' realism I promoted in chapter 3 it was centrally assumed that there was something meant by the gene concept to be investigated, so one could hypothesise about its composition or behaviour, as issues of research, but about which potentially nothing was known or certain. This might seem quite trivial but it's actually of central importance to seeing that pure descriptive theories of meaning do not give good accounts of practice with respect to theoretical entity concepts.

That said then it's worth considering what is often claimed about the early gene concept. There is a lot I agree with in Raphael Falk's 'What is a Gene?' which attempts to 'examine

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[420] See for instance H.B. Frost, 'Different meanings of the term 'factor' as affecting clearness in genetic discussion', *The American Naturalist* (1917) 51, pp. 244-250. T.H. Morgan, 'The Theory of the Gene', *The American Naturalist* (1917) 51, pp. 513-544.

[421] A.H. Sturtevant, *A History of Genetics*, first publ. 1965 (republished Cold Spring Harbor Laboratory Press, 2001).

[422] *Ibid*, p. 101.

the causes and the consequences of the evolution of the concept of the gene.'[423] I suggested as such in the case study. He emphasises that the ill-defined nature of the gene concept and its vagueness across these historical episodes was 'a blessing' unappreciated by geneticists and historians of genetics. This arguably enabled its survival for so long, given the complexity and uncertainty of genetic phenomena, as a more pragmatic notion, now that we know of course just how complicated things really are. But he does argue that there was a significant shift of meaning from the early basic classical gene concept, which he argues was operationally defined. That means that Morgan manipulated the genes 'as if' they were material units, without concern for their nature. As he puts it, to the population geneticist it was irrelevant what genes were, or today are, as long as they could be treated as independent units that obeyed the basic Mendelian laws. This changed with Muller who according to Falk fixed the gene as discrete molecular units that could be measured by chemical-physical methods. This meant the acceptance of genes as material entities 'in their own right' and reached it's zenith with Beadle and Tatum's postulation that each gene produces an enzyme, thus refining its function by which a more refined structural account could be found. This shift produced a gene concept for Muller which was 'conceptually the inverse' of Morgan's 'while for the one the gene was *determined* through its phenotype, for the other it *determined* the phenotype'[424]. The Watson and Crick discovery which while seemingly fixing the gene concept structurally in accordance with its function of producing enzymes and simultaneously paring off the idea of the gene as a unit of mutation or recombination, opened the door to the real problem of how to chemically define the gene concept in circumstances of increasing complexity as biochemistry began to understand that the way in which segments of DNA operated and could not be pinned down by this functional-structural relationship, especially when the role of factors in the expression of DNA were taken into account. Thus the concept exploded into a myriad of structural and functional notions. Falk in effect seems to think that significant conceptual shifts happened quite frequently in the gene concept's history.

Kitcher however takes a more subtle approach in this regard, arguing that the history of the gene is characterised by a set of shifting reference potentials: the compendium of different ways in which the referents of a tokens of a term are fixed for members of the community [425]. Many of these shifts involved the refinement of modes of reference in the hope of finding

[423] Raphael Falk, 'What is a Gene?', *Studies in the History and Philosophy of Science*, Vol. 17, No. 2 (1986), pp 133-173
[424] *Ibid*, 151.
[425] Philip Kitcher, 'Genes', *British Journal for the Philosophy of Science* (1982) 33, pp. 337-359; p. 340.

better more accurate ways of referring to genes while eliminating faulty ones. At the same time after 1953 the reference potentials expanded to increasingly heterogeneous sets of modes of reference some which overlapped in extension with the classical concept (through gene maps for instance) and others which didn't. Which of these was used in a given problem situation depended on the aims of the scientists involved, and this ambiguity was 'sanctioned and acknowledged' by geneticists themselves because of its usefulness and flexibility in adapting one's concepts relative to the phenomena and problem at hand. Weber, picking up on Kitcher's theory of referential practice, maps the history of the gene concept as a history of shifting modes of reference from the Mendelian factor identified by the laws it obeyed, to the unit of a chromosome undergoing recombination and mutation, to Benzer's complementation test of definitions and finally the DNA definitions following Watson and Crick (the class of DNA sequences that determine the linear sequence of amino acids in proteins)[426]. He notes that trying to make extensional links between these different modes is very difficult on the basis of different these descriptions of the gene and it remains unclear what level of continuity there is, but certainly according to Weber, there has been considerable shifts over its history.

Weber, like Falk and even Kitcher, stick it seems to a more or less descriptive theory of meaning and reference, and this makes it of course rather difficult to say anything about how extensions are related, when the modes of reference are vague in the first place, or simply can't be resolved in modern scientific language or according to modern theory, as referring to something definite. I think of course defining modes of reference in this way leaves out critical aspects of scientific practice that hold together these different definitional attempts as part of the study of the same theoretical entity. I think in general those that fall on the side of discontinuity across the transition from classical genetics to molecular genetics tend to locate this almost exclusively in reconstructions of what biologists were thinking about the gene concept at particular times in order to fit them to their ideas of concepts as ring-fenced or bounded structures: that is, in terms of certain descriptions whether functional or structural. This gives the impression that biologists always operated with deliberately fixed criterion for the gene and this amplifies the possibility of reading historical events in terms of discontinuity and referential shifts.

[426] Marcel Weber, *Philosophy of Experimental Biology* (Cambridge, Cambridge University Press, 2004). See chapter 7.

What I'm going to argue then is that for the geneticists involved the changes in the gene concept from 1900 to 1953 were part of a continuous process of research into the nature of genetic material under the heading of the science of 'genetics' that fits with the idea that the issue of the description of the gene itself was embedded in this ongoing research and thus not fixed to any particular description, but that geneticists relied upon and agreed upon shared epistemic roles of the concept which were central to the research process itself and to the investigation and further development of the gene concept. In this regard by having consistent accepted means and constraints for investigating the gene in an environment of ongoing research and ever changing information and research problems, we have a basis for understanding why geneticists in this research context held to the continuity of the gene concept, and considered the same entity the subject of their descriptions and redescriptions.

To flesh these ideas out a bit more, the continuity of the concept and its reference relied upon a belief by geneticists of the existence of a stable particulate causal agent of heredity with certain stable epistemic roles critical to its participation in the study of heredity and developmental phenomena and for which the investigation of that unit was an essential ongoing part of that research. It was part of the pattern of problem solving and thinking that constituted genetics. These roles broadly included,

1. Explanatory role: ascribing as the cause of traits (or of their differences at least) a stable causal unit of gametic material.
2. Conceptual role: as a stable unit for developing an understanding or conceptualising the causes of various and diverse hereditary phenomena.
3. Experimental role: as a unit for producing hereditary phenomena and exploring them.
4. Conceptual role: as a basis for conceptualising the composition and properties of gametic material and its operation, through particularly its self-replication (stability).
5. Experimental role: as a unit of manipulation for testing and probing the properties and structure of underlying gene entities themselves and their causal action.

These, as I expressed in chapter 3, set up the gene concept as an epistemic frame. They acted as resources for understanding the gene and were constitutive of the manner in which the concept was identified or used to interpret phenomena, and with which information was extracted relevant to it in the process of this research. At the same time, these roles provided a framework of basic information and presumptions that were relied on to pursue the concept descriptively. And indeed in the gene's case, researchers did work towards developing and investigating the gene concept through clarifying and elaborating how the gene performed these epistemic roles. This gives us the underlying basis I believe for the evident disposition of the genetics community to treat the concept open-endedly as part of a continuous investigative process into complex phenomena for which ideas about the entity were continually being developed and redeveloped as part of that process. Geneticists centred the concept by the very epistemic roles and properties that were relied upon as part of their use of the concept and which were at the same time essential to the development of the entity concept itself.

The genetics research literature in form of research papers and commentaries I think makes a number of things fairly apparent which are the starting points for giving this type of explanation of the presumed referential and conceptual continuity in such instances of practice. Firstly, the participants took the gene concept to be absolutelz central to performing their research (see East for instance)[427], but at the same time considered the behaviour and structure of the gene to be a central question if not the central problem for their research. In this regard they didn't recognise a firm distinction between classical genetics and molecular genetics as in effect different sciences with different agendas. That is I think a more modern development that is projected historically back. In this context the concept was in fact an essential part of achieving the general goal of understanding the causes of heredity and development, and in turn the function and structure of germinal material. These goals characterised the particular research process relevant to the developments of the gene concept. Driven by this goal researchers elaborated the gene to more precise cytological, chemical and then molecular levels in order to resolve the problems that arose and in order to explain what they were observing as they investigated genetic phenomena more closely and precisely through crossing-over and mutations experiments, and then later biochemical techniques. The switch from the classical gene to the molecular gene occurred in the course of an ongoing process of investigation and the hunt for a better understanding of how genes and genetic phenomena were linked. This is how the biologists involved such as Muller who took up the deeper question of the biochemistry of the gene understood what they were doing. Secondly what centralised the concept for these scientists was being able to rely on the explanatory,

[427] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), pp. 633-655.

conceptual, experimental and investigative roles the gene concept provided with which this research could be carried out.

Of course many different streams of research emerged with the advent of the gene concept that used the concept for other investigations, such as investigation of the boundaries between species, or of natural selection and evolution. This stream however usually came under the heading of the science of 'genetics', and was conceived with general goals in mind, namely understanding heredity and development. Scientists reflected often on the centrality of the gene concept epistemically to this investigation from the beginning, as we saw in the third chapter. Bateson for instance referred to the 'factor' concept as a 'novel and hitherto untried instrument by which the nature of the living organism may be explored.'[428] He pronounces that "…we have now the means of beginning an analysis of living organisms and distinguishing many of the units or factors which essentially determine and cause the development of their several attributes."[429] Researchers understood that these goals led them to the problem of understanding the chemical basis of hereditary processes, as understanding the interactions of genes at the chemical level leading to the expression of traits became increasingly necessary to understanding and explaining more complex hereditary phenomena. The advent of the gene itself created issues of research that extended into the nature of germinal material. Morgan, usually labelled as the central figure of classical genetics, called the constitution of the germ plasma the 'fundamental problem in the study of heredity,' which set no limits on the explanatory depths that geneticists would need to go to resolve it. Indeed Morgan was perfectly aware in 1917 that biochemical methods and explanations would be needed to push these investigations further. What is interesting moreover is how constitutive the gene was for these investigations. Bateson for instance expresses that, 'It is becoming probable that if more knowledge of the chemical and physical structure of organisms is to be attained the clue will be found through genetics….The recognition of the unit factor may lead- indeed must lead- to great advances in chemical physiology which without that clue would have been impossible.'[430] Later not much had changed in this respect. In Muller's terms in 1945, it was to be "the future task of biochemistry, combined with genetics, to unravel the whole complicated web of protoplasmic and bodily interactions, from the primary gene

[428] William Bateson, *Problems of Genetics* (London, Cambridge University Press, 1914), p. 2.
[429] *Ibid*, p.2.
[430] *Ibid,* p. 31 (Also a quote from Bateson's 1907 Silliman Lecture at Yale).

products to the last phenotypic effects."[431] Wright put it that, "The difficultly in the study of heredity is that characters of the germ cell must be deduced from a study of variation at the other end of developmental history,' yet, 'it remains for genetics to assist embryology and biochemistry in filling the links in the chain between germ cell and adult in specific cases."[432] Wright of course pursued just this course by trying to tie eye-colour enzymes and supposed eye-colour factors and investigating the different causal effects these factors might have on production or operation of these enzymes. As such central geneticists like Bateson, Morgan, Muller and Wright, imagined their research program and its goals broadly to discover the mechanism and operation of heredity and development, making no distinction in this way between biochemical or physiological genetics and classical genetics, but considering them part of the same ongoing investigation albeit with somewhat different methods and tools of explanation.

Importantly at the same that this research into the causes heredity and development was relying on the gene concept, the concept itself was being continually investigated, redeveloped and redefined with respect to these causal relations and functions in terms of the gene's behaviour and structure. The nature of the gene was itself considered a central problem for research into heredity and development. In Goldschmidt's own historical account of the history of genetics he expresses that, 'As long as genetics has existed, the ultimate problem has been the nature of the gene, its reduplication and mutation.'[433] Goldschmidt wasn't just being critical. He gave this comment in the context of a fairly neutral discussion on the history of genetics. While Morgan acknowledged that he had nothing to say of substance about the composition of the gene he took it to be an essential question that would work itself out in the course of investigation when biologists could begin to say something significant about developmental processes. 'I hope to be among the first to welcome any real contribution concerning the nature of genes based on chemical changes that take place in the embryo where the products of the genes show their effects. In fact I do not know of any more direct way in which we can ultimately hope to find out the nature of the materials that we think of as genes in the germ cells.'[434] Again for these researchers like Morgan in questions of development lay the progression of their research, and characterising and investigating

[431] H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), pp. 1-36; p. 26.
[432] Sewall Wright, 'Color Inheritance in Mammals', *Journal of Heredity*, Vol. 8, No. 5 (1917) pp 224-235; p. 224.
[433] Richard B. Goldschmidt, 'Fifty Years of Genetics', *The American Naturalist* (1950) 818, pp.313-339; p. 336.
[434] T.H. Morgan, 'The Theory of the Gene', *The American Naturalist* (1917) 609, pp.513-544; p.535.

heredity through cytology was part of a process of obtaining information relevant to that research. The gene concept itself raised issues that required explanations at deeper biochemical levels, such as the nature of its operation and chemical composition that was always recognised as part of whatever a fuller understanding would consist in. Muller of course explicitly addressed the gene question with his mutation analyses, stating that chief interest of his experiments 'lies in their bearing on the problems of the composition and behaviour of chromosomes and genes'[435] which he imagined as a natural progression of the work of Morgan to a deeper level.

In this context as we saw the gene was extremely open-ended in its formulations, and with which ideas about it were reformed. As Goldschmidt put it in 1950 about the gene, 'At each step in the development of genetics more or less specific ideas were formed.'[436] Scientists at various times worried about the consistent lack of a definition, or conflicting definitions of the gene, and tried to do something about it. But this kind of demand doesn't ever seem to have been particularly pressing, although it was certainly important that it was made clear how the concept could be used conceptually and experimentally, which was the tenor of East's paper attacking the opponents of the gene.[437] What I want to suggest then is that what mattered was this existence of a continuous process of research in which the gene concept, through its epistemic roles, played a central part, and for which the investigation of the gene itself was necessary to this research. These roles constituted a significant basis for this investigation. As such through the agency of these roles, scientists understood their engagement to be a continual investigation of the same object and same underlying referent.

A critical part of how this was to be managed was through the explanatory role linking genes and traits, which was the source of causal relations for experimental study and manipulation, and the investigation of the mechanisms underlying these relations (including the structure of germinal material and genes themselves). This investigative role was expressed by Beadle and Tatum such that in 'investigating the roles of gene the physiological geneticist usually attempts to determine the physiological and biochemical basis of already known hereditary traits.'[438] The usual methodology was to rely on this causal relation to further investigation of

[435] H.J. Muller, 'Artificial Transmutation of the Gene', *Science* 66, pp. 84-87; p. 86.
[436] Richard B. Goldschmidt, 'Fifty Years of Genetics', *The American Naturalist* (1950) 818, pp.313-339; p. 336
[437] E. M. East, 'The Mendelian Notation as a Description of Physiological Facts', *The American Naturalist*, Vol. 46, No. 551 (1912), pp. 633-655.
[438] G. W. Beadle and E. L. Tatum, 'Genetic Control of Biochemical Reactions in Neurospora', *Proceedings of the National Academy of Sciences of the United States of America*, Vol. 27, No. 11 (1941), pp. 499-506; p. 499.

genes themselves. Morgan expressed the need for making these kinds of relational connections such that "the more characters that are obtained that show the association in inheritance the further we may hope to go in our analysis of the constitution of the germ-plasma…."[439] Of course for Morgan's group this presumed link between trait and gene was vital for cytological investigation of germinal material. Through it the idea genetic composition of chromosomes and the 'beads on string' explanation of linkage was experimented on and developed. As the biochemical and molecular analysis of germinal material became ever part of investigation, Muller took the gene to be a stable controlling structure around which processes of development evolved and thus epistemically a conceptual basis point to which chemical theories could refer.[440] Beadle and Tatum relied heavily on the explanatory role of the gene concept to construct their investigative program of attempting to fathom how genes chemically controlled the biochemical reactions with which they were associated. I said a lot about this in chapter 3 of course. But geneticists also acknowledged the significant experimental roles emerging from this presumed causal relation as not only providing a basis for producing new phenomena to help them in their research, but also for more closely examining the phenomena they had. Muller for instance, noting the importance of being able to physically alter a gene through mutation, referred to the process with individual genes as 'in effect a scalpel' for experiments 'in which the finest, most fundamental elements of the body fabric are separately attacked'.[441]

It's here that talk of a concept as an epistemic frame really has value, because it gives us an explanation of how such central theoretical entity concepts were managed in the context of ongoing research by virtue of such epistemic roles. Their epistemic roles, which become essential for this research and the progression of it towards its goals, provided also the contact with the entity through which information relevant to the entity concept could be accumulated through experiment and interpreted as the research process carried on. The gene-trait relation was an inherent part of the fabric of genetic research, relative to which both the gene itself was consistently developed, by providing a basis through which the gene could be

[439] T.H. Morgan, 'Factors and Unit Characters in Mendelian Heredity', *The American Naturalist*, Vol. 47, No. 553 (1913), pp. 5-16; p. 16.
[440] H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), pp. 1-36. Or in an earlier paper, "these genes exist as ultramicroscopic particles; their influences nevertheless permeate the entire cell, and they play a fundamental role in determining the nature of all cell substances, cell structure and cell activities. Through these cell effects, in turn, the genes affect the entire organism." H.J.Muller, ' Variation due to change in the individual gene', *The American Naturalist* (1922) 642, pp. 32 -50, p. 32.
[441] H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), p.26.

investigated and interpreted. The research context and its higher goals which relied on this particular aspect of the gene concept sustained the dispositions of the research community to adapt the concept (treating it open-endedly) and reformulate it in light of new information relative to these roles, which were often part of the tools and techniques by which such information was produced. This explains then why geneticists like Muller and Wright considered themselves following a continuous path of research when they began examining the chemical structure of germinal material. Wright explicitly points out that understanding better the link between genes and traits will tell us of 'the ultimate germinal factors involved', proposing the role of geneticists to 'trace back through the contributing causes at each stage of development…and, on the other hand, the ramifying influences of unit variations in the germ cell must be traced forward through development.'[442] For Wright this causal link was the epistemic basis of the investigation of the gene and structured his approach through trying to fathom the link between genes and enzymes, and enzymes and characters, and sorting out the multiple causal relationships involved.

On this basis too we can say something about Falk's claim that Muller himself elicited a conceptual shift in the gene concept when he explicitly began to treat it as a material entity with a material structure. In first place it misses the fact which I emphasised in chapter 3 that Morgan and many of his contemporaries had a realist belief in the gene concept and explicitly took it to be ultimately necessary to the understanding of heredity and development to have a biochemical account of the gene. For them it was a material entity even if there was little they could say about it. Muller held himself to be investigating something already established and with particular properties and relations which he could rely on to frame his investigation as we saw in chapter 3. He talks of gene mutation as making a 'fundamental contribution to cell physiology,' and that this contribution "has so far scarcely been assimilated by the general physiologists themselves, consists in the demonstration that….there are present within the cell *thousands* of distinct substances – the 'genes'; these genes exist as ultramicroscopic particles; their influences nevertheless play a fundamental role in determining the nature of all cell substances, cell structures and cell activities."[443]

Although he develops the concept as an 'ultramicroscopic particle', nowhere does Muller contend that his investigations require a new gene concept or represented a significant

[442] Sewall Wright, 'Color Inheritance in Mammals', *Journal of Heredity*, Vol. 8, No. 5 (1917), p. 224.
[443] H.J.Muller, ' Variation due to change in the individual gene', *The American Naturalist* (1922) 642, pp. 32 - 50, p. 32.

departure from its other uses. He held what he was doing to rely on those. It would have in fact been unfathomable to him since the concept as it was through its various conceptual, explanatory and experimental roles, was constitutive of his very investigation. Thus while Muller does seemingly shift the gene concept from definition in terms of phenotypic differences, to the structural consideration of self-reproductive units that produced chemical reactions producing ultimately phenotypic differences, he was relying nonetheless on a further deeper investigation of this causal relation between genes and traits, and the particulate unit nature, through his use of mutations. Mutations, or the productions of change in individual genes was essential to his individual methodology through examining the causal outcomes. "Such studies as those on pigment production in flowers and in insect eyes, on the synthesis of amino-acids and vitamins in *Neurospora*, and on the trains of effects following given mutations in mice and in poultry illustrate the progress which this general method is capable of making."[444] With this methodology he worked on measurements of gene structure and size. Even the belief in the self-reproductive property of the gene was derived from this epistemic role, and could be analysed only with respect to it, based on exploring patterns of causal effects of genetic material 'as judged by the result of their passage through generations'[445] under mutation, and inferring from this the independence and stability of the genes in their self-reproduction. Muller thought that with the unit of self-reproduction he had a better basis for the chemical analysis of the gene, but only because self-reproduction was an obvious essential property of the gene's causal action and ultimately its explanatory power. Thus he was relying on these relations and the unit assumption to investigate germinal material for a chemical account of the nature of the gene.

In a similar manner Beadle and Tatum can be understood to have worked within the frame of the relation between gene and trait, but investigating that relation at a finer level, in order to build a theory about the primary products of genes. This is often pointed to similarly as effecting a methodological change from the traditional method of examining the chemical bases of genes starting from 'already known hereditary traits' and working back to the underlying chemical process, to instead setting out 'to determine if and how genes control known biochemical reactions'.[446] They claim this methodological shift themselves. But this method still worked on the assumption that genes had causal effects and these could be

[444] H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), p.26.
[445] *Ibid*, p. 5.
[446] G. W. Beadle and E. L. Tatum, 'Genetic Control of Biochemical Reactions in Neurospora', *Proceedings of the National Academy of Sciences of the United States of America*, Vol. 27, No. 11 (1941), p. 500.

associated with particular biochemical processes that themselves were part of the chain leading to traits. It was a different path of investigation, but one still informed by the same epistemic roles. With the aid of mutations one could test the relation between these effects and genes, and thus determine more precisely the causal role. Of course, importantly it took the emphasis off traits as the causal output by looking at more direct products, but this didn't sever this basic link for the scientists involved, it in fact gave them a frame for the closer investigation of it. If enzymes were more immediately connected to characters as Sewall Wright hypothesised, then genes could be studied more immediately through their connection to enzymes.[447] Rather they understood themselves simply to be more finely investigating an aspect of that link, which was part of the general framework they were committed to, that 'the development and functioning of an organism consist essentially of an integrated system of chemical reactions controlled in some manner by genes.'[448] Physiological or molecular genetics in this way was not some kind of disjunction from classical genetics but an elaboration of it, in the context of an ongoing program of research into the causes of heredity and development.

As I expressed in chapter three throughout the 30's and 40's the significant events of the genetic theory such as Avery, Macleod and McCarty's determination that DNA not protein was the chemical basis of heredity relied on the supposed causal relations of the gene concept, and its ultimate connection to traits or more precisely trait differences.[449] When Benzer began to dissect the fine structure of the gene and to challenge the preconception from Morgan that genes were at the same time units of recombination, mutation and function, thus challenging descriptions of the gene, he relied on the gene-trait relation.[450] It gave him the standpoint to make these determinations in a meaningful way as part of a finer investigation of genetic material.

Philosophers have themselves, when investigating the continuity of the gene concept, emphasised the importance to the history of genetics of this relation, and sometimes even its modern persistence. Waters argues that the molecular gene concept is not divorced from the

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[447] Sewall Wright, 'Color Inheritance in Mammals', *Journal of Heredity*, Vol. 8, No. 5 (1917), p. 224
[448] G. W. Beadle and E. L. Tatum, 'Genetic Control of Biochemical Reactions in Neurospora', *Proceedings of the National Academy of Sciences of the United States of America*, Vol. 27, No. 11 (1941), p. 499.
[449] O. T. Avery, C. M. MacLeod, and M. McCarty, 'Studies on the chemical nature of the substance inducing transformation of pneumococcal types,' *The Journal of Experimental Medicine*, Vol 83 (1944), pp. 89-96.
[450] Seymour Benzer, "On the Topography of the Genetic Fine Structure" *Proceedings of the National Academy of Sciences* (1961), 47 (3): 403–415.

classical one, but systematically related by a program of clarification and articulation of this very causal-explanatory role. He thinks that the attribution of a rupture between classical and molecular genetics stems from a misconception of classical genetics, ascribing it a direct causal relation to traits, which might have described Bateson's beliefs, but not Morgan's or those of the majority of geneticists from Johannsen on. The causal connection was conceived of as complex by classical geneticists, and the relation was not described in any determinate way without the possibility of redevelopment. When we look at the history of developments we see the continued attempt to articulate what that relation might be and how this relation itself could be used to identify what a gene is. Waters identifies a modern molecular conception in contemporary practice that reflects classical ideas about the gene as causing differences in phenotypes where these have through articulation been translated to differences in nucleotide sequences affecting the transcription of molecular genes.[451] As such many of the genotypic differences discovered by Morgan can according to Waters be identified at the molecular level. For me the implication is that geneticists whether classical or molecular have continued to be directed in their thinking about the gene concept and how to define it by this particular causal relation. As Waters puts it '..at earlier stages of investigation, when they have not gotten close to specifying nucleotide sequences, they tend to think of genes in terms of the rougher grained classical concept.'[452] I would hesitate then that even from the 60s on this relation has remained somewhat essential to the conception of the gene. Hence the recent attempt in 2008 to define the gene relies essentially on the idea that the gene is a functional concept, such that genotype determines phenotype.[453] Modern scientists work to give a modern sense and interpretation to this, but they still seem to accept it as basic or constitutive. For my analysis, up till the mid-50's, I would strongly emphasise the important of this relation. This relation was not just an 'essential relation' it was constitutive of the use and development of the gene concept, and played a fundamental epistemic role in how geneticists examined, categorised and interpreted genetic material.

But this was not the only significant epistemic role of the gene in this respect. The particulate nature of the gene concept was also somewhat constitutive of investigation into germinal material, certainly its conceptual use for its analysis and investigation, and thus of investigation of the gene itself. Scientists looked for a unit object, and designed their theories

[451] Kenneth Waters, 'Genes made Molecular', *Philosophy of Science* (1994) 61, pp.163-185.
[452] *Ibid*, p. 183.
[453] Gerstein MB, Bruce C, Rozowsky JS, Zheng D, Du J, Korbel JO, Emanuelsson O, Zhang ZD, Weissman S, Snyder M (2007). "What is a gene, post-ENCODE? History and updated definition". *Genome Research* **17** (6): 669-681.

on the basis that it could be identified as a unit. Mendelianism of course relies on this. This unitary nature was the presumption of those that continued the investigation to the biochemical level. Muller's research into the nature of the gene through radiation experiments aimed 'to obtain evidence concerning the degree of inner particulateness of the gene'[454]. This idea manifested itself in the conceptual use of the gene concept in the Mendelian system of heredity, but also in the subsequent investigations of the chemical basis of that system, with geneticists and biochemists both relying on and seeking out, the unit nature of the gene. The whole project of mapping genes to chromosomes 'as beads on a string' and explaining linkage in terms of units depended upon it. The question of what defined it as a unit remained open and subject to change. The presumptions that the gene could be defined as a unit of recombination and of mutation were realised to be problematic, on the basis that the unit of function was the prerequisite understanding, which of course we have already emphasised in understanding modern definitions of the gene, related as it was to the most important epistemic roles of the gene concept. Conceptually Goldschmidt, as we saw, characterised the genetics of his contemporaries and historically as dominated by the unit idea, to the point of being constitutive of the very way of thinking geneticists had of genetic material. I think Goldschmidt was not merely being provocative when he saw it this way.

In addition it was also epistemically important to researchers that the gene concept represented an invariant or stable unit across generations. Griesemer thinks this was particularly important.[455] It was the reason for instance that East so heavily attacked Castle for his assertion that natural selection might alter Mendelian factors, prompting the response that this would render the concept epistemically useless. Muller stresses that,

"the finding of the starting-point in a complex course, were it observed in any other field, would be taken to imply the existence of some guide or guides, some elements that are themselves relatively invariable and that serve as a frame of reference in relation to which the passing phases of other features are adjusted…..So, too, in the organism it would be inferred that there exists a relatively stable controlling structure, to which the rest is attached, and about which it in a sense revolves."[456]

[454] H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), p.9.
[455] J.R. Griesemer, 'Reproduction and Reduction of Genetics', in P. Beutron, R. Falk, and H.J. Rheinberger, *The Concept of the Gene in Development and Evolution* (Cambridge, Cambridge University Press, 2000), p240-285.
[456] H.J. Muller, 'The Gene', *Notes and Records of the Royal Society of London*, Pilgrim Trust Lecture, Vol. 137 (1946), p.1.

Muller himself took this property to be most importantly expressed through the self-reproduction of the gene, but of course it was also vital to the way in which the explanations with the gene concept were grounded, the way its causality was conceived and understood, and particularly, tested. To quote Griesemer, 'The experimental geneticist relies on the developmental invariance of the gene to discover its presence through hybridisation, so invariance is embedded in the instrumental notion of Mendelian factors as well as in its conceptual analysis."[457] As such, "A process historiography of the gene concept would show that challenges to the stability or invariance of the gene cut deeply because they challenge not only concepts, but the epistemological basis and experimental utility of the gene concept."[458] Griesemer thus thinks that the development invariance was to some extent constitutive of the investigation into the developmental operation of genetic material and the gene itself, and new definitions of the gene concept, even if it restricted its function, did so in a way that would preserve this property.

To sum up then in the words of Griffiths and Neumann-Held, " The history of the molecular gene concept reflects a continuing conviction on the part of geneticists that a theory of inheritance should be a theory of inheritance of units of development, be these phenotypic traits, enzymes, or polypeptide."[459] My argument is the conceptual, explanatory, experimental and investigative roles of these properties and roles of the gene concept at least up until the 60s were to some extent constitutive of their very investigation into the nature of heredity and development. They were not simply essential properties and relations, in fact they were open themselves in many cases to interpretation, but the fact of their existence played an essential guiding role in the direction their research and theory took.

So when we consider these epistemic roles associated with certain properties and relations of gene concept, and the continuing research process, for which the gene itself was an object of investigation, we begin to be able understand I think why geneticists understood themselves to be invariably talking about the same concept and the same underlying entity, from Bateson through to Benzer. This process of research made the structure and operation of the gene an open investigative issue, which in this situation of complexity and uncertainty manifested itself as a strong disposition of the community to accept changing viewpoints about what the

[457] J.R. Griesemer, 'Reproduction and Reduction of Genetics', p. 269.
[458] *Ibid*, p. 269.
[459] Paul E. Griffiths and Eva M. Neumann-Held, 'The Many Faces of the Gene,' *BioScience* (1999) 49(8), pp. 656 – 662; p. 657.

gene was and accept redefinitions as more information arose in the course of the research. On the other side the concept and its referent were stabilised by the consistency of its epistemic roles, through which the concept was itself investigated – the explanatory and experimental roles of it causal relations, and the conceptual roles stemming from its unitary nature, and its stability. These were part of epistemic basis of the entire genetics research program into heredity and development. Thus these historical actors across time while within the same research process shared principles for the investigation and development of the gene concept, and thus shared the idea that they were investigating the same object despite changes in their understand of its nature and operation.

That then is my essential claim about the development of the gene concept as a theoretical entity concept from 1900 until the end of the 1950s. It places the gene concept as an essential part of an ongoing process of investigation, and makes the scientific practices of reference and conceptual continuity dependent on the consistent means provided by the gene concept for that investigation.

Within this environment, the description and re-descriptions associated with the gene concept could be interpreted as the results of the use of those roles in the course deeper investigation of the underlying entity. This explains why geneticists spent much of their time looking not for a random entity, but for something that fit with their explanatory and conceptual expectations of it, as for instance a stable unit structure that possessed certain causal roles in generation of phenotypes across generations in accordance partially at least with Mendelian laws. This applied as well for Bateson with his presence-absence hypothesis, as it did for Morgan with respect to his chromosome theory, and to the later biochemists who sought for the chemical structure and operation of the gene. They were all engaged in a continual process of using the gene concept conceptually, experimentally and in explanation through these roles in the course of their research and through which they could modify their understanding of the gene concept as new phenomena arose and new theories were composed. It seems to me than the conclusion we should reach about referential practices in this historical case is that researchers linked their concepts as such through the ongoing nature of this research and the consistent epistemic roles and properties the gene concept provided it, as a shared frame for their research and investigation including into the underlying entity itself.

5.4.2 Conceptual Continuity, Reference and the Neutron Concept (1900-1939)

The neutron concept comes to us without the kind of philosophical and historical disputes over its meaning that beset the gene concept or even the electron concept. Although of course in the context of contemporary physics the content of the 'particle concept' is hugely debated, the controversy is general and stems mostly from the application of quantum field theories. There have of course been disputes naturally about what 'particle' could mean in the context of quantum mechanical theories, where the concept of 'trajectory' is problematic on the standard interpretation. These philosophical concerns, did not however afflict day to day research involving the neutron concept, which emerged already with the assumption that it was a 'quantum particle' and for which the application of quantum field theories to it in resolving beta decay did not at least in the research texts provoke ontological concerns. In fact, in the context of the concerns of modern philosophers of science and their presumptions about concepts it is surprising perhaps, that developments in the neutron and protons concepts like these were so smoothly accepted. Nonetheless the neutron concept's development throughout this period should raise traditional historical questions over meaning and conceptual continuity, and it is probably only a happenstance that it as yet hasn't to my knowledge. The reasons for saying this can be traced to a few pertinent observations. Firstly there could well be a question over whether or not Rutherford's neutron concept is linked conceptually and referentially, with that developed after Chadwick's discovery. In fact Feather writing much later asserts that 'Rutherford's neutron is not ours' by virtue of its reliance on its asserted proton-electron structure.[460] This observation itself raises the second issue, whether or not the uncertainty and shifts over the structure of the neutron does in fact represent a lack of reference and shifts of reference as scientists changed their opinions on what they were dealing with. Is the 'neutron' composed of a proton and electron referentially or conceptually continuous with the 'neutron' that is fundamental, or that which is merely a particle state? Thirdly the general uncertainty of the formulation of the neutron and the willingness of scientists to change their minds or pose only hypothetical ideas about it, might prompt the statement that the concept simply had no precise referent but was a hypothetical construct during this period. The mass of the neutron became an open question for instance in 1933 and 1934, such that mass became unavailable as a ground to define the 'neutron' and distinguish it from other particles.

[460] N. Feather, 'A History of Neutrons and Nuclei', *Contemporary Physics* (1960), pp.428-453.

Of course none of these kinds of claims really fit well with the behaviours or beliefs of practicing nuclear physicists at the time, where concerns of conceptual discontinuity or referential discontinuity were infrequent if non-existent in this period. When mass became uncertain there was no doubt in the minds of experimenters that they were all engaged in the project of trying to measure a property of the same thing even if they disagreed about how to do it. They didn't splinter into groups claiming they had discovered different particles with different masses. Certain conditions were holding together their claims as referring to the same object. As such, even if there still isn't the kind of debate that there is with respect the gene concept, there still seems to be good grounds given its variability for investigating what was underlying the beliefs in continuity in the case of the neutron concept.

In my account then of the use of the neutron concept in the development of nuclear physics before the Second World War I drew out more or less the following observations. Firstly with the discovery of the nucleus an ongoing research process involving at first only experimenters emerged directed at the understanding nuclear phenomena, nuclear process and forces, and the structure of the nucleus itself. It's in this context that we talk of the epistemic roles of the neutron concept. Importantly with the neutron concept, the theoretical development of nuclear physics gained impetus which was previously lacking. Secondly this context of nuclear physics was one of acknowledged complexity, where what was known about nuclear structure and nuclear forces was deeply uncertain and recognised as such. As such most theorising about nuclear structure and nuclear forces was piecemeal, not general. It aimed at giving specific accounts of relatively circumscribed aspects of the nucleus or particle interactions. This informs the way we should understand this research process at the research frontier as fluid and quickly developing, which is itself part of understanding the way the neutron concept was conceived and employed. At the same time new nuclear phenomena were being produced consistently which served to exemplify that complexity and set new considerations and constraints for theory or model construction. Thirdly I emphasised that the neutron concept was employed flexibly in the pursuit of theories of nuclear structure and processes in terms of its own properties and structure. Many aspects of the neutron concept were developed relative to the goals and aims of the research process, to explain the operation for instance of nuclear forces or nuclear structure. Physicists adapted the concept to fulfil these roles. I claimed as such that like the gene concept, the neutron concept was playing generally a vital role for researchers as an epistemic frame. Researchers relied on the epistemic roles of the concept to frame their understanding of it, and their investigation and further development of it. That it was relied on this way was in fact vital to its discovery by Chadwick, which was possible because he was working with a concept of it from the outset, with which he could interpret the experimental results in terms of the neutron.

The significant epistemic roles I identified with the neutron concept included then;

1. The explanatory and conceptual role explaining and describing the properties and composition of the nucleus as a unit of nuclear structure of neutral charge and subject to quantum mechanical principles.
2. Conceptual and experimental role constructing theories and producing information respectively about nuclear forces, as a unit of nuclear force subject to quantum mechanical principles.
3. Explanatory role explaining experimental nuclear reaction phenomena as a neutral unit of the nucleus.
4. Experimental role producing nuclear phenomena and probing nuclear reactions: relying on the neutrality of the neutron and its role as a unit of nuclear force.

As in the gene case these roles were non-determinative, open to re-interpretation and principally for guiding research. How the neutron played its physical roles in the nucleus and as an element of nuclear force was something at the outset uncertain and open to development but it was nonetheless critical to the very approach researchers had chosen to take to understanding and explaining the nucleus. At the same time property assumptions and behaviours regarding the neutron were important to their operation, such as the neutrality of the neutron and part/whole relation to the nucleus, its part in nuclear forces, and its explanation of certain experimental reactions which gave experimenters a means to produce and use the neutron. These roles combined then to give the neutron concept the important function of an investigative tool into the nuclear structure and processes. The neutron concept through these roles became the central concept for nuclear investigation which physicists, both experimenters and theorists, acknowledged.

My point then regarding the conceptual and referential continuity of the neutron concept is similar to that made in respect of the gene concept. The development of the neutron concept didn’t occur in a vacuum and when it comes to assessing its continuity in practice can’t be simply extracted out of its research context and compared in terms of elements of its meaning.

Rather the neutron concept was interpreted (or identified), developed and investigated within the context of an ongoing process of research and within the frame of its epistemic roles. There are again two factors I believe involved in its continuity. Firstly the neutron concept was an embedded part through its epistemic roles of the project of understanding the complex nucleus and the complex principles and laws at work at the nuclear level. In this context the neutron concept was treated open-endedly in the process of grappling with complexity and achieving these general goals. Secondly these roles provided means for the investigation of the neutron, such that agreement on these roles could amount to agreement on how to get information about the neutron and how to develop the concept of it, even if this led to different outcomes on behalf of different scientists, or to only transitory descriptions. Being shared I think these roles were part of what prompted the continuity of the concept and reference between scientists working theoretically and experimentally.

Let's see how this worked. Firstly I think it is historically clear that the neutron concept became a central part of the ongoing investigation of the nucleus, through its epistemic roles, to the extent that its own description and investigation was an integrated matter for this research. From Rutherford on, the neutron concept was considered important not just because it represented a successful discovery about the world, but because it promised a concept that could provide itself a starting point to understanding the nucleus, as an element of its structure and as a unit of nuclear forces, but also because it revived the possibility of a quantum theoretical account. The neutron concept did not create nuclear physics. That existed before at least in the experimental context. But it did bring with it these explanatory, conceptual and experimental roles that opened up a whole new realm of investigative paths towards its understanding. On the other hand the complexity of nuclear phenomena and nuclear forces were reiterated frequently, and became more apparent as new phenomena were produced. These roles were essential to beginning even to grapple theoretically with this complexity, and acted as starting points for doing.

In this context it was recognised that understanding the properties and behaviours of the neutron itself was central to the understanding of the nucleus. In Chadwick's words, "the nature and properties of the neutron are of interest not only because of their novelty but because the neutron is probably a very important unit in the structure of matter."[461] Ideas

[461] J. Chadwick, 'The Bakerian Lecture: The Neutron', *Proceedings of the Royal Society* (1933) A142, pp.1-25; p. 2.

about the neutron concept were in fact often largely developed and redeveloped within the context of broader attempts to conceptualise the nucleus and nuclear processes. As I detailed in the case study Heisenberg, Majorana, and Cassen and Condon tried a succession of different theories about the structure of the neutron in order to come up with a theory of nuclear force, or a formalism for it, which was their main goal.[462] Fermi and Yukawa postulated the novel ability of the neutron (and proton) to decay or transform into a proton in order to explain beta decay and the nuclear force in quantum field theoretic terms respectively.[463] As part of this process of investigation the *pe*-model of the neutron came to be rejected, as not only incompatible with mass measurements, but also with the different and partially successful theories about nuclear force. As we'll point out below, in itself the *pe*-model offered to scientists who pursued it a way of elaborating the epistemic roles of the neutron concept, and thus a basis for hypothesising how the neutron might be involved in nuclear forces and coulomb interactions with other particles, and also how it might be tested for by virtue of the field arising from this close combination. In any case it was in the context of investigating nuclear structure experimentally through the aid of the neutron concept that new properties and behaviours came to be attributed to the neutron. The discovery of the slow neutron required a revision in the laws governing the neutron which engaged itself a reconceptualisation of nuclear interactions. Testing the resonance properties of slow neutrons was interrelated with the question of nuclear structure. As such the description of neutron concept was very much embedded through its epistemic roles in the general frame of nuclear research.

The open-endedness with which the neutron concept was applied in this regard was an important part of its use in this context and characterised the willingness to reformulate ideas about it, and not to affix the neutron to any particular description. We've already noted the way in which the structure of the neutron itself was handled open-endedly, as something incorporated into the investigations of nuclear theorists and open to speculation. Neither Chadwick nor Heisenberg fully committed themselves to one account of neutron structure

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[462] W. Heisenberg , 'Über den Bau der Atomkerne. I', *Zeitschrift für Physik* (1932) 77(1-2), pp.1-11; Über den Bau der Atomkerne. II', *Zeitschrift für Physik* (1932) 78(3-4) ; Über den Bau der Atomkerne. III', *Zeitschrift für Physik* (1932) 80(9-10); E. Majorana, 'Über die Kerntheorie', *Zeitschrift für Physik* (1933) 82, p.137. Translated in D.M. Brink, *Nuclear Forces* (Oxford, Pergamon Press, 1965, pp.161-169; B. Cassen and E. U. Condon, 'On Nuclear Forces', *Physical Review* (1936) 50, pp. 846 – 849.

[463] E. Fermi, ‚Versuch einer Theorie der β-Strahlen. I' , *Zeitschrift für Physik* (1934) 88, pp. 161-177. Translated in F.L. Wilson, 'Fermi's Theory of Beta Decay', *American Journal of Physics* (1968) 36(12), pp. 1150-1160; H. Yukawa, 'On the Interaction of Elementary Particles', *Proceedings of the Mathematical Society of Japan* (1935), 17, p.48.

both remaining cautious in the face of what they considered to be inconclusive evidence. Heisenberg weighed up of different models of neutron structure at the Solvay conference in 1933.[464] When the *pe*-model was finally ruled out with the mass measurements of the neutron, it didn't lead to any conceptual rupture. Scientists agreed simply that this account, though it had been influential in the conception of the neutron and thinking about it, was not critical to it, and the concept was independent of it. In this environment of complexity and uncertainty researchers could accept that their ideas about the concept, no matter what their beliefs and the evidence at hand at any one time, might be wrong and need to change. Likewise experimenters were open to new discoveries regarding the neutron in process of their research, such as the discovery of slow neutrons with radically different properties of interaction.

Secondly, it was through these epistemic roles that researchers could be said to have shared basis for the investigation and research into the neutron concept itself. It is important to see how these epistemic roles structured the development of the neutron concept. Like the explanatory role of the gene-trait relationship, the role of the neutron concept in composing a theory of nuclear structure, as a unit of the conceptualisation of nuclear structure, was singly the most important. This was fundamental to its investigation throughout the thirties and unites the concept of this period to that of the twenties. Before Chadwick's discovery for instance the concept was identifiable precisely through its potential role as a structural unit of the nucleus. I say potential, because no one really had any theory on how it could be used to build up atoms. Rutherford thought this structural component could be used to explain the cosmological construction of atoms in stars, by overcoming the repulsive forces. He referred to it as 'a probable unit of nuclear structure', and began a research project looking for it that presumed its presence in the nucleus.[465] Pauli thought rather it would explain the spin statistics of the nucleus.[466] But both their conceptions pursued fundamentally this conceptual role for the nucleus, and it was an essential part of their explanations using the concept, even though they differed on other descriptive aspects they attached to the concept. Chadwick and Rutherford relied on this conceptual role composing nuclei, but also their belief that the neutron was composed of a proton and electron, to begin an experimental program looking for

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[464] Paper and discussions published, W. Heisenberg, *Structure et Proprietes de Noyaux Atomiques: Rapports et Discussions du Septième Conseil de Physique* (Gauthier-Villars, Paris, 1934). Also summarised in English J. Mehra, *The Solvay Conferences on Physics* (Reidel, Dordrecht, 1975), pp.207-226.
[465] E. Rutherford, 'Bakerian Lecture: Nuclear Constitution of Atoms', *Proceedings of the Royal Society* (1920) 97(686), pp. 374-400
[466] Quoted in Abraham Pais, *Inward Bound* (Oxford, Oxford University Press, 1986), p. 316

the neutron. Pauli took the neutron to be fundamental and to have the mass of an electron. Likewise he proposed a research program looking for the neutron as a hypothesis that required checking. As such both relied on this role for providing paths of investigation, and agreed that the concept was vague and ill-refined, despite otherwise disagreeing about aspects of its description. In fact it's probable that Pauli got the neutron concept from Rutherford, for which he took, like Rutherford, the structural role to be paramount. As I stated in chapter 4 when Pauli later dropped its role composing nuclei he made the decision that his concept was no longer identifiable with Rutherford's and changed its name to a neutrino.

Other theorists who picked up the concept before Chadwick's discovery themselves centralised this particular conceptual role in nuclear construction, even though it was principally only for giving them guidance as to how to develop the neutron concept and apply it, since they had no real details on how it performed that role. Carlson and Oppenheimer took its principal role to be the 'building up of nuclei'.[467] Langer and Rosen gave some theoretical thoughts directly on Rutherford's idea of the neutron building up the nuclei cosmologically.[468]

These ideas, theoretical or experimental, all proceeded from the starting point that the neutron was a structural unit of the nucleus. The neutron concept was otherwise open-ended and disagreement could be had over the properties and structure of the neutron in terms of its contribution otherwise to this role. Thus I think a way of characterising what was going on in this period before the discovery must pay particular attention to this epistemic role. Physicists agreed as such on this role as a means to developing nuclear physics and also to developing the neutron concept itself.

At the same time, they all agreed that the neutrality of the neutron, would give it important properties that were of immense importance for its use studying nuclei and in developing ideas about the neutron. It played an important part in the explanatory, conceptual, experimental and investigative roles of the concept, but particularly with regard the experimental attack on nuclei structure it was considered absolutely essential. I've documented how numerous experimental physicists, even before they could identify the entity like Rutherford, reflected on neutron's neutrality to be its principal useful property for their research into the nucleus by virtue of its penetrative power. But this experimental role and this

[467] J. F. Carlson and J. R. Oppenheimer, 'On the Range of Fast Electrons and Neutrons', *Physical Review Letters* (1931) 41, pp. 1787-1788.
[468] R.M. Langer and N. Rosen, 'The Neutron', *Physical Review* (1931) 37, pp.1579-1582.

neutrality, were resources and constraints for the development of the neutron concept itself, not only through the information emerging about the properties and behaviour of neutrons, but because this 'neutrality' had to inform ideas about the neutron. The proton-electron model of the neutron was of course constructed to explain this neutrality out of familiar established concepts. But it was also relevant to the theories of interaction between neutrons and other particles. However most importantly this neutrality informed the investigative strategies for using the neutron experimentally, and in the process informed elaborations of it.

So while the experimental role of the neutron was principally to produce nuclear phenomena and probe the nucleus, relying on its role as a structural unit of the nucleus and its neutrality to provide unique penetrative power, it was also part of the practice of obtaining information about the neutron at the same time. As expressed in chapter 4, Chadwick relied on both the neutrality of the hypothesised neutron and its structural role in the nucleus, to identify it physically, on the assumption that the Beryllium nucleus contained neutral particles that were being released on collision with Helium.[469] Measurements of the neutron's mass both relied on its structural role as an element of the nucleus and its neutrality, which provided the agreed basis for calculating that mass and thus formed the basis for disputes over it. Fermi was working experimentally with the neutron concept on just this basis, attributing new behaviours to the neutron as they arose. His discovery of the properties of slow neutrons came in the context of the experimental use of the neutron producing new physical phenomena for use in the interpretation and understanding of the nucleus, which was precisely the task this discovery was put to. Experimenters were building up repertoires of information about what could be done with the neutron and its various powers and behaviours in the process.[470] This extended to properties like their cross-section, scattering lengths, diffusions properties, absorption properties and so on. This information emerged in the context of employing the neutron experimentally and was interrelated with question of nuclear structure and nuclear forces.

In the theoretical sphere epistemic roles were central to the shared investigation and development of the neutron concept. Nuclear systematics worked mainly by using the neutron concept conceptually to put together nuclei schema. In the process its was hoped to be able to

[469] J. Chadwick, 'Possible Existence of a Neutron', *Nature* (1932) 129, p.312.
[470] See for instance J. R. Dunning, 'The Emission and Scattering of Neutrons,' *Physical Review* (1934) 45, pp.586- 600, or W.D. Harkins, D.M. Gans, and H.W: Newson, 'The Disintegration of the Nuclei of Nitrogen and other light Atoms by Neutrons. I', *Physical Review* (1933) 44, pp.529-537 and J. Chadwick, 'The Bakerian Lecture: The Neutron', *Proceedings of the Royal Society* (1933) A142, pp.1-25.

express in terms of proton and neutron combinations the rules for stability of nuclei and thus begin to fathom the relevant forces and processes at work, such as the Pauli exclusion principle, in holding atoms together. The neutron concept itself was not simply a fixed constant in the process of developing this understanding. It was important to understand what the neutron could or could not do in regard to nuclear stability, and of course precisely its behaviour and properties in the nucleus. Much of this kind of thinking within the context of these particular problems involved the proton concept as well. Models of the nucleus such as the shell model and the liquid drop model made assumptions in this regard about the interaction between neutrons and protons and could be seen as attempts to develop these concepts. But it was certainly in the context of trying to account for nuclear structure and nuclear stability that Landé proposed for instance that neutrons may transform to protons as a process of beta decay in order to shift unstable nuclear configurations to stable ones.[471]

However the more adventurous developments of the neutron occurred in the context of the role of the neutron as a unit of nuclear force, and conceptually for developing theories of that force (or forces as it turned out). For example Heisenberg's, Majorana's and later Cassen and Condon, and then later Yukawa, applied different hypotheses about neutron nature within the frame of the neutron's conceptual role as a quantum theoretical unit of nuclear force, for the purpose immediately of giving an account of that force. Heisenberg postulated the *pe*-model for instance in order to give himself some conceptual basis with which to build up a mathematical formalism for his exchange force. Majorana however questioned the physical sense of Heisenberg's molecular exchange analogy for the proton-neutron system and proposed a model that treated the neutron as fundamental. Cassen and Condon re-raised Heisenberg's isospin idea to suggest a formalism for nuclear interactions that treated the neutron and proton as states of the same particle in order to highlight the symmetry of these interactions between proton-proton, neutron-neutron and neutron-proton. Of course they didn't necessarily mean to be read as making ontological assertions about the nature of these particles, but Yukawa certainly did. Yukawa developed the neutron as a particle state rather than fundamental, in the vein of Fermi, in order to ground the nuclear force between protons and neutrons on quantum field theoretic principles.

So my claim is then that the epistemic roles, which informed the general explanatory, conceptual and experimental uses to which the neutron concept was put in the course of this

[471] A. Landé, 'Neutrons in the Nucleus. I and II.,' *Physical Review* (1933) 43, pp. 620-626.

research into the nucleus, provided also an agreed framework and means for the investigation and development of the neutron concept itself. The continuity of the concept and its consistent reference thus derived from the reliance on these roles as to an extent constitutive of their insight into the nucleus and the neutron, and bases for their research and investigation in this regard. The concept was a central epistemic part of a shared process of research into the structure of the nucleus and the nature of nuclear processes relying on shared non-determinative epistemic roles of the neutron concept. In this way Rutherford's neutron concept could be considered coextensive with that from Chadwick on, since although it was speculative and wrong about the structure of it, producing many failed experiments to detect it, it still shared the epistemic purpose of explaining and conceptualising the structure of the nucleus with a hypothesised neutral particle and the basic properties constitutive of these roles like neutrality.

These roles then, as elements of continuity themselves, formative of this ongoing research into the nucleus, can be located as the basis of referential practices and conceptual continuity with respect to the neutron concept, when we trace how the scientists themselves relied on these to develop different viewpoints about the neutron in this context of ongoing research while nonetheless holding to a belief in the constancy of their reference.

5.5 Conclusion

So my purpose in this chapter has been to provide what I think is the methodological basis of conceptual and referential continuity of theoretical entity concepts in scientific practice, which accords much better to practice than the meaning-based descriptive approaches. Underlying it is the observation more or less that in the context of research what concepts are and how they are used is different from how they might come across in text books or in the reports from scientists of the state of knowledge, and certainly different from philosophical reconstructions, which almost all present scientific concepts as well-defined or bounded structures that act merely as the containers of information. But in the research context, at least for theoretical entity concepts, I've tried to give examples to show that they are actually doing much more than this, and our account of these concepts must reflect their continuity for the researchers involved. On this view above theoretical entity concepts emerge as much more open-ended structures that are embedded in ongoing processes of research into complex and uncertain phenomena through their various epistemic roles for this research. These roles not only provide the basis for the use of these concepts, but also a shared basis for their interpretation or recognition in the phenomena under study, and their investigation and development, development which itself is usually an embedded aspect of this ongoing research. In this environment it seems to me these epistemic roles play for scientists something like an agreed set of means (a frame) for investigation and development of the entity concept as it is carried along by the research, and as such provide the ground for different descriptions through time and across disciplines to emerge in the course of research, that nonetheless are held to have a common referent. Scientists develop the entity concept in frequently different ways, but are nonetheless consistent in their reliance on these roles, and their developments typically serve to elaborate the content of them or expand them. Pursuing these concepts in this way through these roles is seen then by the scientists involved as part of a shared investigation into the same underlying entity whether this investigation involves groups or a single individual working through different theories. In this regard these roles are to an extent constitutive or formative of this investigation and development, being part of the means by which the theoretical entity is identified, and means by which it can be further elaborated.

In seems then that the practices of reference of theoretical entity concepts might boil down to something like this: "We might not have a good understanding or description of the entity, and we'll change our view in the course of ongoing research into complex and uncertain phenomena, but we do share a viewpoint on what the concept is *for* in the context of ongoing research, conceptually, experimentally and in terms of investigation and explanation, and this is information necessary to our interpretation and elaboration of whatever it is underlying these roles and the properties that constitute them. It gives us a common 'epistemic' grip (if it in fact exists) on some otherwise unknown aspect of the world for the purposes of pursuing it further and this helps us pinpoint that we have loosely the same concept and underlying aspect of the world in mind, framed as it is by these various roles and properties. Thus we're jointly relying on these roles <u>to interpret, investigate and develop some aspect of the world further</u> in the course of achieving our research goals."

In the next chapter I'll consider more closely what the philosophical dimension of this purported practice of reference, and what it might mean for scientific realism.

Chapter 6 – Research Processes, Reference and Realism: A theory of reference for referential practices, and a new take on realism

A common criticism of the debate over reference, and theories of reference, is that they have generally failed to pay much attention to what are in fact the actual referential practices of scientists. Criticism targets the way in which such theories have proceeded on the basis of thoughts about language and linguistic relations, relying on abstract notions of meaning and definition, rather than on a strict observation of when scientists thought they were referring consistently and when not. My view is that this is a correct observation, although with the added observation that the bias towards philosophy of language starts with the imputation of a more or less linguistically framed understanding of what concepts are (or what the meaning of a term is) that is itself a philosophical construction and divorced from practical contexts. If we want a theory of reference that follows the methodological practice of scientists themselves then it has to involve a shift from concerns with logico-semantic aspects of reference towards a realisation that reference occurs for practicing scientists in contexts of ongoing research where there is always a degree of uncertainty, and thus epistemic factors should be recognised to have played a crucial role in reference-fixing. The accounts of reference that were for a long while dominant (such as descriptive theories) might be called 'textbook' accounts since they take reference as a product of static instances of concepts reported as definitions or descriptions. But reference treated this way will tell us nothing about the methodology or rationale of these practices in the research context where such restrictions on reference are simply incompatible with the nature of scientific investigation and the complicated and indirect interaction between researchers and the particularly unobservable or hidden aspects of the world. Nor will it I believe give us any sort credible discussion of realism, since the whole debate rests on this account of concepts and representations which is an abstraction and as such ill-adapted in first place to telling us what scientific practice says we actually can be realist about.

This brings us then to the last part of this account of the epistemic roles of theoretical entity concepts in scientific practice, and it draws more on a philosophical discussion regarding them. This chapter is principally devoted to the elaboration and rationalisation of the referential practices that I identified in the last, and the ramifications if any for scientific realism. The first question in this respect is, to what extent current theories of reference give, or rely on, accounts of reference that fit well with the practices I've identified? I'll give a survey of the literature which heads I think in the direction of trying to give a more faithful account in this regard, from causal theories, to causal-descriptive theories. The second question is whether we can otherwise find for these referential practices the rational bases that these philosophical accounts aspire for, and otherwise understand how these practices are conceived to generate language-world relations. After all, one of the key questions for a theory of reference, or more specifically reference-fixing, is how entities are picked out of the world or identified. In this respect what I have to say might be construed as a more generalised account of a causal-descriptive theory of reference, although it differs to some extent, placing less emphasis on the importance of that causal aspect. Lastly, I'll say something about what the ramifications might be for the realism debate if one could 'semantically' justify these practices, which leans more heavily on their open-endedness and epistemic value in the context of research. Giving a more continuous picture of reference of course it provides a stronger basis for realist claims but at the same time, given the open-endedness of these concepts in practice, leave us with the question, 'well what precisely is it that we can then be realist about?', when it comes to theoretical entity concepts.

No one of course should tackle this question of reference lightly, given its old and rather intransigent nature, and while I don't promise to give a well-worked out solution, I do promise to provide a new direction towards finding one, which doesn't approach reference from the starting point of an analysis of language, but rather from the perspective of the researcher located at the frontier of research, engaged in a dynamic process of theoretical and experimental development. For him or her theoretical entity concepts are resources and constraints for moving that research forward, and not just simple ontological posits for which the task is seeking out an appropriate definition. Reference as such needs to be understood and rationalised with respect to the context of the research environment, and not apart from it, which has been unfortunately the classical approach both for descriptive and causal theories.

Having said that let me specify a few things. First I'm going to concern myself mainly with the method of reference-fixing, rather than reference-transmission. This tends to be the area in the literature that is most vexed. For my part reference-transmission follows from shared aspects of the use of the theoretical entity concept in the context of research processes with shared goals. Reference-transmission does not, unlike in the case of causal theories, rely on a different mechanism from that used in reference-fixing. Reference-fixing conditions are passed along in this context. Secondly in the context of theories of reference it is customary to make the distinction between theoretical concepts and theoretical terms. Reference relates generally to terms in their particular environment of propositions or statements. Terms are linguistic elements. A concept is a mental structure that elaborates on the term, or its meaning, and how it can be properly used and so on. I'll try to stick to this nomenclature, and not fall back into talking about the reference of concepts, although it seems to me it's not so ambiguous to talk of the reference of a concept, whereby we mean just the reference that comes with using that concept (or its property ascriptions) as a term in a linguistic context.

6.1 On Causal Theories of Reference

I won't here elaborate deeply on causal theories of reference since it's clear they are of limited usefulness in any account of referential practices with respect theoretical entity concepts. They are interesting because in general causal-descriptive theories take their point of departure to be causal-theories. Indeed the motivation for causal-descriptive theories is best understood within the context of causal theories and the failure of those theories, and partially at least the intuition of causal theories is often preserved in the causal-descriptive version.

The causal theory of Kripke and Putnam is generally acknowledged to give a reasonable account of reference with respect to proper names and common nouns.[472] It can be summed up most precisely perhaps with Devitt's contention that a theoretical term refers to "those aspects of reality that prompted its use."[473] For observable terms however reference is provided by the non-conceptual, but in these cases perceptual, relations in which individuals stand with respect to objects. Reference can be fixed for instance by a practice of ostension, or physically pointing to and signalling the object about which one is speaking. One of course might give a wrong description of what it is that he or she sees, but if through ostension this referential relation is formed, then this description is simply superfluous and of no referential effect. I can point to a horse I see in the distance and mistake it as a cow. But by talking about the horse, for instance as a 'strange looking cow', listeners will take it that I mean the object in the distance, however it was described.

[472] S. Kripke, Naming and Necessity, (Oxford: Blackwell, 1980). H. Putnam, 'Reference and Truth', in *Philosophical Papers*, Vol. 3, *Realism and Reason*, (Cambridge: Cambridge University Press, 1983); H. Putnam, 'Explanation and Reference', in *Philosophical Papers*, Vol. 3, *Mind, Language and Reality*, (Cambridge: Cambridge University Press, 1975).
[473] Quoted in D. Cummiskey, 'Reference Failure and Scientific Realism: a Response to the Meta-Induction', *Brit. J. Phil. Sci.* 43 (1992) pp 21-40, p26.

There are problem with even this simple mechanism, as appealing as it sounds, but with respect to kind and theoretical terms however the situation is much more problematic. Firstly ostension alone is clearly not enough in the case of observable kind terms because any sample pointed to instantiates many kinds, not just one. A *tiger* instantiates a mammal, a predator, a feline etc….Which kind is pointed to when I call something 'tiger'. This is the so called *qua* problem. Further it seems hardly conceivable that ostension of one sample could tie down its entire extension as a whole. Devitt and Sterelny reply to such criticisms by asserting a structural component.[474] The internal structure of a kind responsible for its manifest properties used to identify the kind bears the burden of reference. This of course seems reasonable and deals with the *qua* problem mentioned above, by fixing down kinds, but as Psillos points out, forces an inconsistency on the causal theorist.[475] A causal theorist valuing the causal relation of the 'dubber' to the natural kind over any theoretical description would be want to say in the case where a description of a kind's internal structure was discovered to be incorrect, that referential continuity was maintained regardless. Yet the introduction of 'internal structure' was precisely because ostension itself, or a kind's manifest properties, were not sufficient to pin it down and eliminate referential ambiguity.

Of course theoretical terms cannot be dubbed in any sense by direct ostension. This is probably not an intractable problem. It wasn't at least to Kripke who thought that ostension could be extended to the causal circumstances for which the underlying entities (or properties etc) were said to be responsible in some way. As such dubbing could be abstracted in some sense, either through arguing for an extension of our perception along causal pathways (per Devitt's theory of quasi-perception)[476] or marking an entity as the causal agent of a set of phenomena (as per Putnam) by some causal-description. In the latter case the description plays itself nothing other than the function of an elaborate sign, the important link is the causal chain that leads to our dubbing. The intuition is naturally enough that for any phenomena a causal agent lies behind, and we dub whatever that is with our description. Because the description itself is irrelevant any shift in that description of the causal-agent will not change the referent.

In either case, kinds or theoretical terms, the essential problem is very apparent. Causal theories of reference do facilitate reference continuity, prevent the proliferation of empty

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[474] M. Devitt and K. Sterelny, *Language and Reality* (Oxford: Blackwell, 1987).
[475] Stathis Psillos, *Scientific Realism; How science tracks truth*, (Routledge, London, 1999), pp 285-286.
[476] M. Devitt, *Designation* (New York, Columbia University Press, 1981).

terms and provide for the inductive open-endedness of scientific discovery, but ultimately make reference too easy. If a term refers to whatever prompted its use then it can't in fact fail to refer, even when there is drastic theory change. Hence causal theories fail to give any account of reference failure, which is a deeply unintuitive situation, trivialising the referential claims of scientific realism and conflicting with scientific practice. The almost standard test case in this respect is phlogiston theory. The phenomena for which oxygen is said to play a causal role and for which phlogiston was said to play a causal role, overlap to the extent that by the logic of causal theories we should accept that when scientists were referring to phlogiston they were in fact referring always referring to oxygen, the actual cause. While it is possible perhaps to have a consistent theory of reference that gives such conclusions, this is semantics after all, not epistemology - if we unwittingly refer then fine - it should be rejected not least on the basis that it cuts across actual practice which is what we wish ultimately to have an account of. After all a description of phlogiston or how phlogiston brings about the phenomena for which it is a putative cause, is nothing like that of oxygen. Surely it makes much more sense to conclude that the term 'phlogiston' actually referred to nothing, as scientists in fact themselves did. Scientists have not simply relied on their causal relations to particular causal agents to establish reference, and it is a philosophical abstraction to think so. Science has reached decisions that certain entities once posed do not in fact exist, and has comprehended routinely the existence of different entities as the explanations of certain phenomena. How this behaviour would be fit within a causal theory is hard to comprehend.

Other deeper criticisms target the whole approach of the causal theory, which is seen to try to reduce to the practical use of language to rather *a priori* principles. Although Hacking for instance provides a part for the causal theory in his experimental philosophy, he rejects the idea that philosophy of language can tell us anything about science.[477] Shapere is especially critical of the practice of deriving scientific methodology from thoughts about the structure and possibilities of language. "I protest against...the view, namely, that what science must aim at can be established by an examination of the nature of language." [478] He particularly opposes the assertion by both Kripke and Putnam that science aims at discovering the essences of things, first fixing reference by causal relations and second by using this to discover the essential properties of the kind or entity under examination, properties which cause the observable ones associated with the object or kind. These then become the

[477] Ian Hacking, *Representing and Intervening,* (Cambridge, Cambridge University Press, 1983).
[478] D. Shapere, 'Reason, Reference and the Quest for Knowledge,' *Philosophy of Science* (1982) 49, pp. 1-23; p19.

permanent reference-identifying properties for it, such that scientists classify things for all future time by these essential properties. As Shapere points out more or less it seems that Putnam and Kripke are following some idealised understanding of how language can be efficiently and precisely organised and projecting that onto practice: “the idea that, by an analysis of features of language, we should be able to lay down inviolable conditions on the knowledge-seeking enterprise.”[479] Such criticisms apply as well to descriptive theories of meaning and reference too as Shapere notes, but these were the very theories that Kripke and Putnam had set out to criticise. And while Shapere focuses on the essentialism of Kripke and Putnam, pointing out that in practice scientists don’t always search for ‘essential properties’ for the concepts and don’t fix their use of concepts or terms by essential properties, its clear that initial reference-fixing through simple causal relations likewise has a sense of ‘imposition’ about it. Should we really bind scientists to a practice of reference that is more or less metaphysically contrived, on first principles, from a position on the causal structure of the world, rather than is constructed methodologically by virtue of the discoveries and continuing vicissitudes of research, and which itself might be subject to refinement and development? Surely reference like anything else is an epistemically informed practice. Of course Kripke and Putnam are committed to scientific belief being epistemically determined, just not the aim of science or practices of reference and meaning, which follow from metaphysical considerations and certain theories about the structure of language.

6.2 Causal-Descriptive Theories of Reference

The philosophers who have developed causal-descriptive theories have done so with a basic acceptance and appreciation of the motivations and intuitions of causal theories. The problem as such was not the idea that non-conceptual causal relations had something to do with reference in the case of theoretical terms and reference-fixing, but that they could be assigned completely the role of fixing reference irrespective of any attempt to conceptualise those relations, leading to an intolerable inability to account for reference failure. But the conceptualisation of these causal relations is part of the detail required by scientists in order to be able to pick out objects in the world in a way that allows for reference failure. In case they argue ‘non-ostensibility’ of theoretical entities makes it essential that some manner of description is involved to provide some equivalent for ostension in a theoretical context.

[479] *Ibid*, p. 20.

As such quite a variety of causal-descriptive theories have emerged over the last 20 years or so. What they generally all try to do is find relations between the causal situations in which theoretical entities are supposedly identified and their descriptive elaboration, that serve to provide a basis for uniquely identifying the entity (or kind) involved on the basis that the description of the causal processes or mechanisms by which the entity plays its causal roles (given the assumed causal structure of the world) generally serves to pick out the entity. These theories tend settle as such on identifying a set of core causal properties which play an essential (nomological) part in identifying the entity and fixing its reference, and which are presumed to be stable across time, or on causal-explanatory 'mechanisms' themselves as the central aspect.

Such observations I think do play a significant role in the actual referential practices of science. However my view in general is that while they make a better attempt to capture referential practices, and particularly provide for reference failure, they move too far in the direction of reference failure to the extent that cases of referential continuity in science are left without explanation, and on these accounts would be considered as failures. To put it simply they invoke too much description. More importantly I think they only partially capture what's at play here in the case of reference-fixing for theoretical entity terms and focus too much on descriptive elements at the expense of means for producing those descriptions. Causal roles are important as just such means, but are themselves only part of the collection of elements that go into reference-fixing for theoretical terms typically, for which the fact that the term or concept is part of an ongoing process of research, means that its reference is fixed by the part or roles it plays in that respect, which serve as epistemic conditions for its own investigation. As such Frederick Kroon's 'epistemic' causal-descriptivism is the most interesting as we'll see, although it itself fails to give any semantic motivation for why we should think the epistemic reasons for fixing-reference by causal-explanatory roles serve to pick out objects in the world.[480] In general then I will propose, albeit somewhat speculatively, a way of generalising on the intuitions of causal-descriptivists that captures the practices we've observed and provides some kind of rationale for them. So it is worth then beginning with an account of some of the causal-descriptive theories out there and their particular accounts of referential practices.

[480] Frederick W. Kroon, 'Theoretical Terms and the Causal View of Reference', *Australasian Journal of Philosophy* (1985) 63(2), pp. 143 – 166.

6.2.1 Enç: Causal Roles and Kind-Constituting Properties

The first real attempt that I can trace at providing such a theory was given by Berent Enç in a paper entitled 'The Reference of Theoretical Terms.'[481] Enç well acknowledges I think that theoretical entities (and kind terms in general) are introduced in explanatory situations and are shaped and distinguished by the explanatory function they are meant to provide. It is not just that they are the cause, but they are the cause in some manner or other and require the properties enough to fulfil their causal function.

Enç's particular theory applies to non-ostensible terms, particularly theoretical entity terms. He finds little problem with saying that in ostensible cases causal-theories provide a plausible account. A theoretical term however is introduced principally to explain a set of phenomena. To do so it needs be associated with a causal-explanatory mechanism and a set of properties; and simply put, the explanatory mechanism employs these properties to provide explanations of the phenomena, sufficient enough for the term to supply a complete explanatory package. The properties themselves "bring along with them the suggestion" that the object in question is of a specific kind, and we can refer to them as 'kind-constituting' properties. The understanding is that the object must itself be clarified sufficiently to play its causal role and for Enç this means that these properties that mark it out as a new kind; that is, identify it as a new distinct 'kind of thing'.

On this important point Enç gives no strong argument. Why should the properties imputed to an entity in this way be sufficient to constitute a kind or reveal descriptively a kind? This is an important point and remains in general one of the chief points of clarification for this type of causal-descriptive theory which I will discuss later. Enç does however give the intuition underlying it. One can't explain by virtue of a new object unless that object can be distinguished from others. Where the object remains uncertain or underdetermined, then the explanation will be caught out begging the question as to why this object is explanatory rather than say several, or any object at all. It would be unreasonable for a scientist to introduce a 'new' object unless its status as new object is identified. So for Enç one can't explain with the aid of a new entity without an understanding of what 'specific kind of thing' the entity is, which includes necessarily some account of the object's essential properties. To fulfil these

[481] Berent Enç, 'The Reference of Theoretical Terms', *Noûs* (1976) 10, pp.261-282.

explanatory tasks and institute this entity as a kind we need what seems to be a description sufficient to tell us that is new and the 'kind of thing' we have.

We can see how this challenges purely causal theories. To insist that a non ostensible term refers to just what it is that is responsible for the phenomena is in Enç's terms to be unreasonable because it denies the very belief which forms the grounds for introducing the non-ostensible term in the first place (that it is a certain new kind of thing). Our account of reference-failure with this model is linked with the replacement of those descriptions of the 'kind of thing' we have by others that give a contradictory account. Of the collection of properties that describe the kind or the associated causal mechanism that must be abandoned sufficient for reference change is not something Enç strongly develops, but leaves somewhat indeterminate. Nonetheless it is clear if these properties and the causal mechanism are preserved despite theory change then we can talk of referential continuity.

Enç does I think outline well the kind of intuition that drives the causal-descriptive model with its emphasis on the causal explanatory context in which entities are conceived. But his intuitions on what is, or has been, required for explanation in general is undoubtedly wrong, running up against the open-endedness historically of such terms. The main objection as such to Enç comes by pointing to cases where the terms were introduced without kind-constituting properties to any substantial degree, or which changed their kind classification. Kroon's major objection to Enç follows these lines and motivates his own theory placing weight on causal-explanatory mechanisms we'll examine later. As he puts it kind-constituting properties 'specify too much structure'[482] and are clearly too much to always demand of scientists when they introduce new theoretical terms. He uses phlogiston as an historical example, claiming that there was a 'vigorous debate' about what kind of stuff phlogiston was, with different theories giving different answers, but scientists nonetheless agreed on that they were referring to a particular substance involved causally in calcination and combustion. Similar things could be said about the original gene and the electromagnetic field concepts, where in the later case especially, ideas of what it was changed significantly without to the scientists involved shifting the reference from the same underlying entity. Kind-constituting properties don't seem as such to play generally a necessary part in reference-fixing for theoretical entity terms. We can reason I believe from this that even when such terms have been introduced as

[482] Frederick W. Kroon, 'Theoretical Terms and the Causal View of Reference', *Australasian Journal of Philosophy* (1985) 63(2), pp. 143 – 166; p. 153.

being of a particular specific kind, like a 'physical particle' for instance, there has been a disposition on the part of researchers not to attach reference to the kind-constituting properties that identify the kind.

6.2.2 Psillos and Core-Causal Properties

Psillos' position follows more directly that of Enç's, pursuing a more directing descriptive basis to reference-fixing and reference continuity with some important twists.[483] Like Enç he assumes that to give an entity-supposing causal-explanation must at the same time, uniquely identify an underlying kind. In his language, to identify the causal origin of the information attributed to an entity requires pinning down that origin to an identifiable kind via some description. The properties so described outline the manner in which an entity plays its causal role vis-à-vis a set of phenomena. He gives the criteria that to be a referring account the kind-constitutive properties postulated must be the *causal origin* (in fact) of the information associated with the term (manifest properties and the like). Unlike Enç however Psillos tries to provide some more structure to the theory so we can at least give more detailed conditions for reference continuity. Psillos labels the kind constitutive properties that are posited when an entity is introduced the *core-causal description* of an object. A core-causal description, to fulfil its function as kind-constituting, will exclude any other entity from playing the same causal role other than one of that kind. Of course what types of properties we take to be kind-constitutive are theory-dependent but it's clear they should identify the extension of a kind and fix it down uniquely. According to Psillos then a term can continue to refer despite theory change if there is a substantial preservation of its core-causal description.

This raises its own problem which I won't go into of how we might go about identifying what counts as part of the core-causal description, without hindsight. Clearly not all properties attributed at the introduction of a term are supposed to count as core-causal. The more significant and probably difficult claim, distinct from Enç, is that "the core-causal description need not necessarily, and does not generally, include detailed accounts of the specific constitution of the posited entity."[484] I don't believe Psillos has any good argument in this respect, except to say that such information is not necessary "for a general explanation of how a posited entity plays its causal role". True, but of course it begs the question again, why

[483] Stathis Psillos, *Scientific Realism; How science tracks truth*, (Routledge, London, 1999), pp. 280-293.
[484] *Ibid*, p. 295.

should we expect these to be kind-constitutive? There remains the problem as there was with Enç of getting from the explanatory function to a kind-constituting set of properties, only now with the added complication from Psillos that such properties where they are posited will not generally include constitutional or inner-structural properties. Unlike Kroon who doesn't seem to think kind-constituting is necessary, or is at least not pursuing this kind of semantic justification, Psillos does and so needs an argument or evidence that properties posed in these explanatory contexts for explanatory purposes will be non-constitutional but kind-constituting.

The suggestion seems to be though that there is a one-to-one link between causal powers and kind constituting structures of nomological necessity. As Nola puts it, "For, when two things differ in their causal powers, but lead to the same kinds of effects in the same conditions then they must distinct kinds of things."[485] Scientists refer to their kinds and entities on the basis of their causal powers independent of whether they succeed or not in giving a correct description of the actual kind. Causal properties are themselves adequate. This in fact seems like a potentially fruitful account of how world-language connections are forged by scientists, on the basis of a rather simple metaphysical belief. The problem comes when we try to elaborate what a 'causal power' is. If it's simply the ability to cause X, then of course there will be a problem of over-reference. So the issue is finding a way of spelling out what is meant by 'causal power' without trespassing these two bounds. As we'll see in Kroon's case, ascribing causal powers to causal mechanisms (X causes Y in manner M) leads to potential under reference in the cases where descriptions of causal mechanisms have changed. So the issue is finding a way of spelling out what is meant by 'causal power' without trespassing these two bounds.

Nonetheless I don't want to discount this approach as a plausible rationale for reference-fixing, despite its metaphysical presumptions about the links between kinds and causation. I just don't in fact think that such causal-descriptive theories as these that rely on core causal properties or powers capture actual referential practices in their entirety, nor the actual rationale that researchers rely on for this practice. This kind of theory of reference seems to place much emphasis on the supposed causal roles of entities, which are only part of the

[485] *Ibid*, 525.

epistemic roles that have been generally used I think in the fixing of reference for theoretical terms.[486]

6.2.3 Cummiskey and Theory-Constitutive Metaphors

Let me then mention an alternative causal descriptive theory which associates a different kind of information which comes with entity postulation as important for reference-fixing practices. This comes from D. Cummiskey who we talked about earlier, although with a different purpose in mind.[487] Cummiskey follows Boyd in attributing to theory constitutive metaphors a fundamental role in science but seeking to develop Boyd's views with respect to the reference of theoretical terms.[488] Again like other causal-descriptive theorists Cummiskey makes his point of departure the 'over-reference' of terms under the causal theory. Metaphors however stand to provide both the descriptive constraints necessary to judge reference failure, the flexibility for open-ended investigation and the programmatic nature of scientific research.

In Boyd's theory constitutive metaphor theory, these explanatory metaphors consist in a primary subject with which we are well versed, and a secondary subject for which we think the primary subject could act as an analogical guide; for instance, Rutherford's solar system model of the atom, or machine computation and the human brain. These metaphors are introduced (or quickly developed) with the introduction of new terms, on the assumption that the referent will prove similar to the primary subject. In the process some tentative or preliminary indication of the properties of the presumed kind is brought along with the metaphor, but mostly the description of the entity is left open. Why is this causal-descriptive? Well, the metaphors are explanatory devices, and meant to accompany the entity in providing a loose causal explanation of the phenomena that can then be further refined. A change in metaphor signals a change in reference, and this will occur if a metaphor no longer provides a fruitful explanatory framework in which scientists can articulate further the nature of the entity they are dealing with. Failure in this way of course naturally signals referential failure,

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[486] Again I hesitate that these properties are really sufficient to identify descriptively something uniquely. Yet if it did descriptively identify the kind then it seems a rather underdetermined way to talk about kinds, because as we know there were types of things that might have been described consistent with these causal powers, such as a liquid-like substance or an abstract field entity which are quite different, and I don't see how it can be ruled out *a priori* that different kinds might be described to fit a set of causal powers, whatever the particular causal structure of the world.

[487] See chapter 1 of this dissertation. D. Cummiskey, 'Reference Failure and Scientific Realism: a Response to the Meta-Induction', *Brit. J. Phil. Sci.* 43 (1992) pp 21-40.

[488] R. Boyd, 'Metaphor and Theory-Change', in Andrew Ortony (ed.), *Metaphor and Thought* (Cambridge, Cambridge University Press, 1979).

because whatever entity is behind the phenomena, it doesn't conform to the properties predicted by the metaphor. So the metaphor provides a similar role to the properties and causal-explanatory mechanism of Enç, but Cummiskey would argue, better captures the ongoing and open-ended nature of scientific research into entities. Setting up a loose flexible idea of what the entity is engages us in a kind of directional or focused research. Crucial to this, a metaphor does not assert that the entity in question need be exactly as the primary comparison, only similar to it. The aether wasn't presumed to be a liquid, but only similar to it, sharing many of its properties but not all of them.

I take it that there is something to what Cummiskey says here and in certain cases analogies do drive the investigation of entities. It is the most sophisticated account yet perhaps of the relation between the open-endedness of scientific investigation and reference. It tries to give sense to the idea which as been an essential theme of this dissertation that scientific research is guided epistemically by loose postulations about the world, thus attempting to find a starting point that however schematically provides an accommodation of language to the causal structure of the world that can be further relied on. Metaphors provide the loose causal-explanatory framework in which we further refine our descriptions. And it is that aspect of reality responsible for our original use of the term – which prompts our metaphorical description - and guides its further development as causal theories have always maintained. It is of course an empirical question whether such theory-constitutive metaphors are widespread or not or in fact can give a good account of what scientists rely upon to judge referential continuity and referential success. I for one suspect that it cannot be as universal an account as Cummiskey thinks – there are no doubt occasions in science where entities are posited with no real idea what in terms of its nature it is analogical to.[489] I think it is asking too much of scientists to have these metaphorical concepts of an entity always at the outset. In any case I would argue, as I would in the case of Enç, that the theory does not provide enough reference continuity for a good account of scientific practice, and is thus not a good explanation of scientific referential practices. These metaphors provide too much structure and ask for too much information from scientists over and above what scientists ordinarily need it seems to refer. They are after all meant to provide a guide to the essential nature of an entity, even if it is only loose. In any case theory-constitutive metaphors are adjusted too often in science. The

[489] The gene concept is mostly a good example of this. Of course Johannsen had the idea that the gene could be considered like an organic molecule with side-chains the functional parts. This played a heuristic value for the sake of understanding but didn't provide as far as I know an enduring idea for geneticists. Most would not be drawn on what they were dealing with even analogically. William Johannsen, 'The Genotype Conception of Heredity', *The American Naturalist*, Vol. 45, No. 531 (1911), pp. 129-159.

history of the model of the atom from plum-pudding to planetary to quantum for instance is doubtless a history of shifting theory-constitutive metaphors, yet we would consider this a classic case of reference-continuity despite theory change. Under Cummiskey's model we can't talk about the continuity of reference of the EM-field concept, because the metaphor associated with the aether, that of a mechanically acting liquid, proved fruitless and was supplanted by something much different. Analogies in fact come and go, as scientists have searched for ways of dealing with new phenomena and new information, as cognitive historians of science could tell us. But they don't seem to be involved generally in reference fixing for theoretical entity terms.

6.2.4 Kroon and Epistemic Warrant

However the most interesting and relevant causal-descriptivist theory comes from Frederick Kroon, which I think makes the most serious effort and sophisticated attempt to try to fathom how researchers structure and motivate their referential practice, especially with regard theoretical entity terms.[490] It comes in effect close to what I'm claiming about these practices, although with a few important differences.

Kroon starts with common criticisms of causal theories of reference, such that standing in causal relations is not in itself enough to pick-out a kind or entity in the world on the basis of the logico-semantic failures of ostension. "We (sometimes) cannot, that is, for logico-semantic reasons, do without a moderate degree of conceptualisation of the objects we are in causal contact with if we are to introduce a term on the basis of that causal relationship."[491] But curiously he sets up the problem of accounting for our reference-fixing behaviour as a problem of the 'overdetermination of reference by theory' (ORT), as though nonetheless these causal relations would be generally sufficient for reference-fixing, but through various reasons unrelated to logico-semantic ones, we choose to fix reference by conditions that are typically overdeterminate. It is as such not clear whether Kroon is merely posing this principle as a null hypothesis, for the purposes of teasing out relative to the causalist-perspective our actual practice, or whether he actually thinks there is in fact some sufficiency about the causal theory, even if there are often certain problems with it, making a degree of conceptualisation 'necessary' to any reference-fixing at all. He seems in the case particularly of theoretical

[490] Frederick W. Kroon, 'Theoretical Terms and the Causal View of Reference', *Australasian Journal of Philosophy* (1985) 63(2), pp. 143 – 166
[491] *Ibid*, p.145.

terms to mean the later, rather than the former, introducing his ORT principle as a genuine conditional;

"condition C that *actually* fixes the reference of t….is (typically) overdeterminate; there is usually a condition C^1 – the cautious causal substitute for C – which does not entail C and which is such that while C^1 *could* have been used to fix the reference of t,….C^1 was not so used."[492] (his emphasis)

This principle captures he claims 'the referential apparatus of all so-called 'theoretical' terms.' As such, 'Phlogiston could have been introduced 'cautiously' as the name for the substance in fact responsible for calcination and combustion,...' However, 'the fact that we do not believe in the theoretical identification of phlogiston and oxygen – the substance in fact causally responsible for calcination and combustion- shows that we do not take the more cautious causal description seriously.'[493] The question for Kroon then is 'why?' His aim is hence to give an explanation of these seemingly superfluous referential practices. This is where the causal-descriptivist element is raised; 'the burden of reference for the term introduced rests broadly on the theory within which the term is embedded, and not on some cautious causal description of the form: whatever is responsible for such and such phenomena'.[494] In this vein he rejects Berent Enç's account based on the kind structure of theoretical entity suppositions for the reasons we have talked about. Kind-constituting properties don't seem strictly necessary in practice (or have not seemed) to reference-fixing behaviour. As he notes, 'the 'history of phlogiston' shows there was a vigorous debate about what *kind* of stuff phlogiston was: whether a gas or a dry earth, a compound or an element. Different phlogiston theories gave radically different answers to this question of the intrinsic nature of phlogiston…'[495] But what they did agree in phlogiston's case was that this 'was the substantial kind residing in combustible bodies whose leaving these bodies caused (or even constituted) combustion.'[496] As he puts a scientists may have 'excellent reasons' for supposing a new substance as a causal explanation for certain phenomena without being confident of any kind-based explanation of the phenomena. In this regard Kroon sees the important element of description not to reside in the kind-constituting or structural properties that figure in the causal explanations that are used to fix the reference, but in the 'causal-

[492] *Ibid,* p148.
[493] *Ibid,* p148.
[494] *Ibid,* p150.
[495] *Ibid,* p153.
[496] *Ibid,* p153.

explanatory mechanism' the theory associates with a term, more precisely, 'how a postulated object's (or kind's) causal behaviour explains certain phenomena.' This may entail of necessity given our background knowledge certain assumptions about the structural features of the kind or object doing the explaining, such as for instance that phlogiston needs to be diffuse enough to exist within combustible bodies, 'but often little more than this is claimed'. By this he seems to mean that an explanation might set conditions on the elaboration of the substance or structure but not require its explicit articulation. This emphasis then on causal-explanatory mechanisms is at the heart of referential practice for theoretical terms, and raises itself what he calls a *selective stress criterion* on these mechanisms for carrying the burden of reference. As such his project duly expands, such that the explanation we are looking for should explain not just ORT but also this selective stress.

In this respect then it's his answer that is most interesting. Kroon senses that there is an epistemic dimension to referential practices, and this extends to the entirety of these practices and not just with respect to theoretical terms. With any referential relation what matters for a referential practice is not the simple causal relation itself but that the relation has ultimately 'cognitive' import; that is, a relation by which 'a person is able to acquire information about the object/substance to which he thus stands related.'[497] In the case of observable objects and kinds, perception provides just this kind of causal link, because perception acts as a device through which information can be further obtained. As such 'it is the cognitive, epistemic character of the relation of perception that is of overriding importance for the theory of reference, not its extensional causal character.'[498] In other words the causal relation is only important to referential practice with respect to perceptible kinds and objects because it puts the user in a position to make verifiable statements and claims about them.

Kroon thereby proposes a theory of 'epistemic warrant' as a condition for reference fixing, such that the introduction of a term is epistemically warranted if and only if, it satisfies an existence condition (E) such the introducer or user believes that the entity or kind exists; secondly that the user has an interest in the object or kind that warrants a referential practice with respect to it and that he or she knows how to set about determining true propositions about this object or kind, which are themselves the kinds of propositions that fit this interest in the object or kind (the fact-finding condition(F)). Kroon's justification of this seems to be,

[497] *Ibid*, p157.
[498] *Ibid*, p157.

that without both an existence and a fact-finding condition of these kinds then there is simply no motivation for a referential practice with respect to an object or kind, and there simply would be no referential practice. Enforcing one through a philosophical exercise would be thus I assume, simply be artificial for Kroon. He gives the example for instance of *minsects* referred to as the name for the species of insects with the fewest number of members. Since nothing as all could be said about this species (in particular since we have no way of identifying them) nothing would be achieved by securing reference to them.

While this is meant to be a theory that applies to all practices of reference, everyday or scientific, scientists themselves have precise specialised interests lying behind their postulations of substances and kinds. Generally they name things because they want to investigate their beliefs in the substance or kind further, and learn such things as how to identify samples, what can be explained with it, what are its internal properties and so on. Epistemic warrant is thus Kroon's general putative explanation for the ORT principle. Of course this contradicts the possibility of the causal theory of reference providing an account of referential practices, since simply standing in a causal contact with a substance or kind doesn't provide any kind of a frame for the acquisition of more information about the substance or kind. Now in the case of theoretical terms then in fact what provides F, the fact-finding condition, is typically the *causal-explanatory* mechanism associated with the term. These according to Kroon play the role that perceptual access plays in the case of perceptible objects and kinds. Thus he asserts that in the case of phlogiston, 'by outlining the causal *modus operandi* of the substance postulated as causing combustion and calcination – namely, its escape from bodies undergoing these chemical changes-notably in the form of fire – the theory suggests ways of enlarging our knowledge of this substance.'[499] Kroon attributes a number of uses of phlogiston historically to these although without providing much detail as to how they would have been derived from such a simple description of the mechanism. But they included the techniques for measuring, locating and isolating samples of a substance, strategies for determining the part played by the substance in the formation of others, and a theoretical matrix to help in the construction of phlogiston based explanations and other phenomena. These were all part of the theoretical content associated with the causal-mechanism that phlogiston scientists relied on to extend their knowledge of phlogiston.

[499] *Ibid*, p161.

His conditions he claims thus provide the justification for disputes over the nature of the entities involved, such as in the case of phlogiston, since such structural properties and description cannot of themselves play a reference fixing role. Of themselves the simple fact that these might serve to distinguish the substance from all others doesn't provide a basis of belief in it, which is required by the existence condition E. The causal-mechanism provides this and is thus the essential element. Yet these of course double at the same time as the basis by which the fact-finding is managed with respect to the theoretical entity, ruling out the austere descriptions that are put forward by the causal theory and its non-conceptual relations for reference. As such Kroon's 'epistemic warrant' condition tries to marry belief in the entity with the tools for its investigation, and in the process find a way between the under-reference of Berent Enç's causal descriptive theory and the over-reference of causal theories. The causal-explanatory story that motivates the introduction and further use of a term often provides the very belief in the existence of the underlying entity and the very fact-finding techniques that sustain epistemically its investigation. In some cases of course there might be some logical (or also I assume nomological) necessity to a term being associated with a particular description of its nature or structure for the purpose of the explanatory mechanism, but Kroon thinks these are usually too detailed for reference-fixing, precisely because of the evident transtheoretical character of theoretical terms.

Kroon doesn't give enough empirical examples for him to really make out his claims generally, and this contributes to the weaknesses that his presumptions about the referential practices with respect to theoretical terms, particularly those representing entities, don't fit strictly the historical record. He relies almost completely on the example of phlogiston which is not necessarily a canonical example (although it is usually treated that way) of practice in general with respect to theory change and theoretical entities. He doesn't apply his theory to cases where the name of a concept has remained constant but there has been descriptive change, which of course most common in daily practice, as opposed to complete entity revisions. I think this leads to a problem in his theory we'll detail below. But Kroon is right that the substantial *explanandum* regarding referential practices is the fact that the reference fixing of theoretical terms seems independent of theoretical positions regarding their structure or nature, which are usually points of debate by scientists who nonetheless agree on the reference of their terms, and he is right to think our understanding of this should not be attendant on a study of linguistic or semantic necessities, but depends rather on the epistemic aspects of science. I've tried to argue that this is evident from the practices of scientists themselves. As such I think there's a lot of compatibility between Kroon's causal-descriptivist approach and the way I would frame the philosophy underlying practices. Reference is based on 'epistemic warrant' to use Kroon's term certainly in the context of the theoretical entity terms, if not generally, and this requires both a belief in the existence of the underlying entity (itself of epistemic import) an interest in making propositions about the entity, and to some extent presuppositions about the entity, that serve to provide conditions or resources for obtaining more information about that entity. However in respect of what these conditions or resources are, I think Kroon's insight is too narrow.

As a causal descriptivist Kroon naturally sees much value in the causal-explanatory roles of theoretical terms, and particularly the causal mechanism by which entities are said to provide those causal explanations. He presents a strong belief that these causal mechanisms are generally continuous factors in the history of science for theoretical entity terms. Although what is meant by a 'causal mechanism' is somewhat vague, I think it's fairly clear such mechanisms, if we mean by this the physical elements and processes relied upon to formulate the explanations, are not always continuous. Arabatzis points to the electron as the case in point.[500] Its explanation of many phenomena shifted drastically with the shift from classical to quantum mechanical descriptions of its behaviour. The explanation of spectral lines is a case in point, explained in quite distinct ways by distinct processes. It's hard to see how one could maintain the continuity of the causal mechanism in these cases. In other cases the causal mechanism might really have been unknown with the postulation of the term, and itself be an item of investigation. Of course my main example in this respect is the gene concept, for which significant aspects of its causal-explanatory mechanism were open, and nothing really could be said about how traits arose from their operation. But the patterns in the data were compelling enough to believe that such entities were nonetheless causally involved. Further the processes by which genes operated in this respect were open to continual change and variation. As genetics developed even the original Mendelian explanatory arguments connecting genes and traits altered in substantial ways (from for instance the direct causal role to the geno-differences conception).[501]

[500] T. Arabatzis, *Representing Electrons: A biographical approach to theoretical entities* (Chicago, University of Chicago Press, 2006), p.260 (fn. 59)

[501] Of course if Kroon means nothing more than the explanatory links and the elements historically identifiable as formative to possible formulations and reformulations of the actual explanations, providing vital explanatory power necessary to any iteration, then our perspectives here would simply overlap, which they seem to in Lavoisier's case. There the use of oxygen as a something or a property of air added to materials in the case of calcification was constitutive of his use of the concept and central to its explanatory power. Without it there simply was no reason for the oxygen concept. Actual explanations were developed out of this. But generally by

Indeed its not just elements of causal-explanatory mechanisms that are relied on for making further statements and postulations about the entity and testing them. In fact causal roles in general are not essential. As we've seen some theoretical entities were introduced through means of explanation other than causal-explanation, such as various subatomic particles for instance, which relied on the conceptual role of the entity concept unifying various phenomena within the same set of principles. The point however is that there are varieties of epistemic roles that theoretical entity concepts play and these aren't necessarily attached to the descriptions of how the entities play these roles, and these don't need to arise from a *causal*-explanatory story, although as I've emphasised the explanations for which entities are applied are often paired with certain properties and relations that are relied upon in the further elaboration of the entity concept (like the unit structure of the gene or the neutrality of the neutron) and are constitutive for the scientists involved of these epistemic roles it plays. But epistemic roles like causal-explanatory relations can subsist above particular elaborations of them and act to direct research with respect the entity. Further other roles, which don't necessarily flow directly from these explanatory roles which serve to introduce an entity concept but are more directed at using the entity for other goals. Experimental roles, using the entity to probe and produce phenomena are just such roles, phenomena which are then used in the elaboration of the entity. But also conceptual roles developing theoretical positions and conceptual frameworks with the entity concept have naturally involved development of ideas about the concept and are relied on to do so. These roles don't necessarily depend on the *modus operandi* by which the entity plays its causal-explanatory roles, but in fact are part of the tools of developing it and reformulating it.

This observation leads I think to essential difference between Kroon's and my accounts of referential practice. Kroon doesn't in fact give a good account of how we get from causal-explanatory mechanisms to resources for making further claims about the entity concept. He is mainly suggestive and relies on the phlogiston example. Even there he doesn't really trace the links. I've tried to be more specific in this regard with my elaboration of different epistemic roles that act to provide epistemic warrant in Kroon's terms. But what limits Kroon's account I think, is that it fails to make a connection between reference-fixing practices and the general research processes in which such theoretical entity terms are

'causal-explanatory mechanism' Kroon seems to mean something more full-blooded, consisting of statements about the physical process involved connecting cause and effect, thereby giving an description the physical mechanism involved necessary to giving the explanation. These of course have had a tendency to change.

conceived. They're not just conceived for the purpose of finding out more about them. These concepts come into being to serve roles for overriding research processes, and it's these roles that serve to provide the framework for the concept's descriptive development and investigation as I asserted in the last chapter. As such the term's reference is also fixed by its roles relative to this research and this sustains its flexible treatment and open-endedness. There is a broader context to referential practices. So while Kroon limits the epistemic warrant for the reference fixing of such entity terms to the ability to develop theories of the entity or make further testable postulations about it, we need also to take into account that the entity is created to serve the purposes of a research process, relative to which its epistemic uses are defined, and relative to which they are used to generate accounts of the entity. This is what ultimately allows the continuity of reference despite changes in even the causal-mechanisms of the concept, because there is a persistence of these roles as part of a research framework.

Kroon of course has a notion of 'interest' in his definition of epistemic warrant which is relevant here. Scientists (and more generally others) need to have an interest in the object or kind in order to have epistemic warrant for a referential practice with respect to it. Kroon restricts this 'interest' to an interest in the object or kind, and its further elaboration. But there seems no *a priori* reason why a scientist's interest in fixing the reference of a term might not be related more broadly to his or her use of it in the context of research, towards resolving its problems. We could generalise Kroon's theory by expanding this notion 'interest' in these broader ways.

Finally Kroon is rather vague about the semantic justification for his criterion of epistemic warrant. It's in fact ambiguous throughout. What I mean by this, is that Kroon doesn't really proffer a basis for taking the conditions necessary for a term to have epistemic warrant as philosophical reasons for thinking that this is sufficient to identify or pick out an object or kind in the world. This is the kind of underlying rationale we're seeking in trying to clarify how these practices operate. Kroon's objectives are arguably limited to providing an epistemic explanation of what scientists rely on in practice but not what justifies this reliance. There's a difference between the epistemic motivation for a particular theory of reference, and the semantic motivation for it. After all we might believe that causal-explanatory mechanisms should guide our referential practices, because our belief in the existence in objects should be tied to our ability to further elaborate those objects, but is this itself enough to think that these mechanisms actually do pick out a unique kind or entity, remembering that kind-descriptions do not carry any burden of reference for Kroon, as they do for Enç for instance.

So how could we make the semantic link between things and causal-explanatory mechanisms. Does Kroon in fact do this? I can offer some speculations here. One tack might to be to read the situation as more or less a true extension of the causal-theory. The whole point of presenting the situation as one of reference over-determination seems to be that the causal-theory is considered largely sufficient for reference but it is epistemically unwarranted to rely upon it in the case of non-ostensible entities. So we add an extra layer of detail in the form of causal-explanatory descriptions to satisfy our epistemic demands but which also serve to exclude reference where it is unwarranted. Thus for the sake of theoretical practice, we attempt to limit the referential relations by imposing on them some level of description to more or less weed out certain disadvantageous or unintuitive situations, such as referring by phlogiston to oxygen, and give us a basis upon which we can fail to refer. Scientists and indeed people in general take more control over what they are referring to by employing causal-explanatory mechanisms as a qualification. We still refer to whatever is the causal-agent for a set of phenomena, however now in the case of theoretical terms, we refer to whatever it is on the basis that it possess a certain causal-explanatory mechanism. We don't thus need any particular kind-based idea of what we are referring to, although the causal-explanatory mechanism itself might necessitate a certain picture. But that's really not important– it does no reference-fixing work and I presume could have historically changed. Where science is concerned it accords well with the idea that scientists are interested in entities which have epistemic value to their research and put them forward on the basis of these. They're not interested in simply inflating language. This would be an interesting approach but it does rely essentially then on a causal theory, which is itself problematic. My concern in any case remains that it leaves out the important part of 'ongoing research processes' which I think must have something to do with these semantic relations.

Another argument I would suggest, although similar in vein, would relate this position back to that of Psillos by arguing for the role the causal-mechanism itself has in providing access to kind-constituting properties. This is in fact Roberta Nola's argument more or less.[502] If it is in fact believed that causal powers do allow a mechanism to the further discovery of kind's internal or kind demarcating properties via the theory in which it is embedded, then if this

[502] R. Nola, 'Fixing the Reference of Theoretical Term,' *Philosophy of Science* (1980) 47, pp.501-531.

mechanism is correct (does actually correspond to something that exists) then presumably we should believe they are sufficient in themselves to identify a unique kind. In other words the causal-explanatory mechanism plus the theoretical context (various laws of nature and the like) provide all that is necessary to discover the kind-properties attached to that mechanism. This one-to-one link suggests that the causal-mechanism itself is enough to map to a kind, if they do themselves correspond to the powers of a kind, and can be relied upon as such. But it's hard to see why causal-mechanisms rather than simply causal powers are any more sufficient. If causal mechanisms are nomologically related one-to-one with kinds, then so should causal powers be. As Kroon points with respect to Nola, there's no real logico-semantic justification for choosing one over the other. Since it is primarily Nola's desire to give a logico-semantic justification for causal-mechanisms, it simply fails, hence Kroon's desire to pursue an epistemic basis for it.[503] But it could be again that Kroon subscribes to this kind of position and is simply adding a layer of conditions on top to better capture greater selectivity of actual practices.

In any case I think Kroon's reference-fixing by epistemic warrant is certainly something of a starting point to seeing a way forward towards a theory of reference appropriate to scientific practice with respect to theoretical entity terms. And in so far as Kroon sets out to give a survey of scientific practice, he successfully makes the very important link between those practices and the epistemic value of the concepts involved, which frames in general the approach I'm taking here.

6.3 Arabatzis - An experimental theory

Before we begin the construction of an alternative theoretical position to these then, it is worth noting Arabatzis' thoughts on reference-fixing practices.[504] He starts from a more or less causal-descriptivist sentiment, suggesting that we can identify a relatively theory independent 'core of meaning' associated with theoretical entity concepts to which reference can be attributed. The problem remains of course how we identify these, except by noting their relative stability, and how we attribute reference to them independently of this fact. Arabatzis suggests that the core aspects of description associated with an entity, are those that

[503] *Ibid*, p156.
[504] T. Arabatzis, *Representing Electrons: A biographical approach to theoretical entities* (Chicago, University of Chicago Press, 2006), chapter 9.

'enable one to pick out the experimental situations attributed to it'.[505] That is, entities like electrons have been traditionally identified with a stable or accumulative collection of experimental set-ups, through which stable central properties, like mass-to-charge ratios, have been measured. The stability of the later is of course partially the result of the stability of the former. Of course explanatory mechanisms are required for a particular experimental context to yield such properties, but these needn't themselves be stable. The important things are the associations between the experimental set-up, the entity concept, and these associated properties which can be relied upon to identify the entity in new experimental situations.

In later papers Arabatzis has emphasised the fundamental importance of the experimental manifestations of theoretical entities, whereby in the electron case both diachronically and synchronically, experimenters agreed that the experimentally observed phenomena could be attributed to the electron concept. "The existence of incompatible representations of electrons did not raise doubts about their identity in experimental contexts."[506] This is really I believe an important observation, since certainly in physics there is a stability, or stable accumulation, of experiments that get associated with theoretical entity concepts, like particles, suggesting that experimental set-ups play fundamental identifying roles for researchers. The neutron concept supports Arabatzis' contention in this respect about the electron concept. Certain experimental contexts became fixed with respect to the neutron and were used to develop and sometimes redevelop descriptive elements of the concept. Indeed in the neutron's case, the experimental contexts persisted over and above even the measurements of the mass-to-charge ratio derived from those contexts, since this itself became an area of dispute, although the experimental situations that Chadwick at least had used to identify the neutron were not questioned, only his interpretation of them. This raises the point as such that I would in my terms put weight on the obvious epistemic importance of these experimental contexts, and the explanatory roles which theoretical entity concepts play with respect to them. They are contexts relied upon for the development of already assumed unobservable causal agents. That they are stable shows their centrality to the identification of the underlying entity, and the priority of these presumed basic explanatory roles. Clearly Arabatzis' association of theoretical entity concepts with experimental set-ups should be part of any broader account of reference-fixing in practice, and elements of it should overlap with what I have to say below

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[505] *Ibid*, p259.
[506] Arabatzis cites Gallison, *Image and Logic: A Material Culture of Microphysics* (Chicago, University of Chicago Press,1997) here. Theodore Arabatzis, 'The historicity of scientific objects' (unpublished), p10.

insofar as these experimental contexts are relied on (as phenomena fixing explanatory roles) for the description and re-description of their underlying entities by composing an agreed epistemic access to them. They are also the basis of the experimental roles they might have, providing contexts which can be manipulated so as to manipulate the entity for research purposes. However I think experimental contexts are only one aspect of how this idea of 'epistemic access' is generated for researchers, and constitutes for them a basis of reference.

6.4 A Theory of Reference for Referential Practice

My promise at the start of this chapter was to present some ideas on how to put together a theory of reference that tries to reconstruct what these referential practices are and at the same provides some kind of rationale for them, in terms of picking out entities in the world. These examples above were themselves attempts to give a theory of referential practices. And while they don't I believe completely capture these practices, they do provide a relatively rich context of work on reference which can be generalised towards providing a more informed account of what's going on and the principles underlying it. And I think this is largely because many of them have been moving towards a recognition of the importance of the epistemic dimension of these practices, and the open-endedness with which theoretical entity concepts have been employed in the context of research. For Arabatzis, Kroon and Cummiskey I think this is particularly clear. To get from these causal-descriptive accounts to a more complete account requires something of a shift of perspective nonetheless, which stops trying to identify and consider entity concepts independently of the research processes in which they play central roles, and realises that this context actually matters for referential practice. This means ultimately being more circumspect about the role of descriptive elements of such concepts that can be associated with them at any one point in their history in picking out the underlying entities, and shifting to a 'research' perspective which places this practice of reference at the frontier of research, a frontier characterised generally by uncertain and complex phenomena, and by the need for conceptual structures that can facilitate the investigation of it. These practices of reference relate to the dynamics of research as such, and certainly not to the perceived mechanics of language and linguistic relations. But there should still be, if these practices are to achieve their aim (and we expect science to be rational regarding such a methodology), some underlying rationale for why scientists should think that the use of theoretical entity concepts in these ways is sufficient to pick out stably things in the world, if they exist, and thus share common referents diachronically and synchronically.

Again I stress that what I'll say here is far from a complete theory, but really just a few tentative pokes in the direction I think theories of reference which aim to account for actual practices with respect theoretical entity concepts should go. I'm not going to give much historical evidence directly on this, noting that scientists rarely do comment directly on their referential practices or their rationale, but rather a plausibility argument that ties together what I've noted about these practices and the importance of the research context to them. Let me start then by drawing together my observations on referential practices, and the insights of these causal descriptive theories above, from which I think we can move towards a more complete picture of what's going on historically.

Scientists have interacted with the world through the agency of their theoretical entity assumptions. The epistemic roles attributed to them for the purposes of research have provided a frame of conceptual, explanatory and experimental tools through which that interaction was managed and developed. Researchers have historically based their referential practices around these roles as continuous elements constitutive or formative of their research for given research processes. This was the claim of the last chapter. We want to explain then what has motivated this reliance on these epistemic roles to fix the reference of terms. Here's a suggestion. Epistemic roles as we've noted are fundamentally productive in nature. Their value lies in being able to rely on them to produce successful outcomes. A theory of reference built on them should reflect this. Ultimately then I think researchers have believed that their terms have referred through these epistemic roles, because of the potential of such roles to provide them with tools and resources for pursuing their research successfully towards its goals. That is, there is an underlying belief that if a concept refers to something actually existing in a way consistent with these roles and their constituent properties, then reliance on these roles should be correlated with the advancement of their research through the successful accumulation of knowledge, explanations, predictions, theory-production and the like. In this way it is sufficient to rely on them as basic reference-fixing elements. This is then a suggestion as to what has rationally informed this methodology in a way appropriate to the dynamic context of ongoing research, the complexity and uncertainty associated with it, and the roles theoretical entity presuppositions as epistemic frames have furthering it.

So this relies more or less on abductive type inference that the degree of success or chance of success of relying on these roles in their research activities would be increased where the term actually refers, as opposed to when it doesn't, and thus can provide a reference fixing basis.

This needn't have been a strong belief that it made a significant difference but only that it should have generally made some. Of course it might be the case that there was no underlying referent that played these roles, but then of course in the course of research that should have become clear not just through contradictory experimental results, but the irresolvable difficulties and '*ad-hocness'* that emerged trying to use the concept with various background beliefs through these roles to account for emerging phenomena and greater levels of complexity. The concept simply would have become less productive or much more difficult to rely on for addressing and solving problems and providing fruitful paths of investigation and research through the development of its roles.

As such when researchers have used a concept to develop conflicting theoretical positions for instance, guided jointly by certain conceptual or explanatory roles, both could understand their theoretical entity term to be referring to the same entity and making different statements about it, by virtue of their reliance on these roles, because both shared the belief that their reliance on these roles reflected a shared starting point or means for pursuing their research into more speculative territory, even if doing so led them in different directions giving different background contexts and theoretical presuppositions. In either case their basis for relying on these roles to fix their reference and ground their common communication, was a belief that the reference of the concept through these roles was necessary, or at least highly relevant, to their success in either case, since in either case these roles were formative of their shared research approach.

To put it another way then, use of non-determinative epistemic roles to fix reference relies on a belief that if there is reference through these roles, then they can be relied upon as basic to the production and obtainment of true or approximately true or improved statements and theories relevant to the questions of research and the underlying entity itself. The fact that epistemic roles might make a difference to the outcome of research if the concept refers by virtue of them is sufficient for using these roles referentially, if only in a very loose and approximate way, since what is identified by these roles themselves is generally only vaguely conceivable. But this method of fixing-reference unlike others doesn't do so by giving a precise or essential picture of the referent that picks them out, but rather by providing elements and conditions that can be used to help find those out in the course of research, which of course follows if they do provide researchers some conditions and constraints through explanatory, conceptual and experimental roles which reflects a reality in the first by providing some conceptual structure and experimental tools that helps them over time sort through the complexity and uncertainty of the phenomena for just such a clearer picture.

Causal-explanatory roles are important in this regard, as they were in the gene case and the neutron case. But since the emphasis is on being able to rely on these roles productively what matters ultimately is the role itself and whatever properties are constitutive for the entity concept playing that role. Even descriptions of the causal-explanatory mechanism might not be necessary to the use of the role productively, as we saw in the ability of geneticists to fall back on the simple causal relation in their management of genetic research. This allows them as such to share extremely open, but nonetheless referring, beliefs about the underlying entity. But causal roles themselves aren't the only roles that have been relied on this respect. So were conceptual and experimental roles. Conceptually, the ability to develop theoretical frameworks with the aid of the concept and its basic properties for resolving research problems, has translated to a belief that in order to be able to do so, it should have been anticipated that the conceptual framework reflects some correspondence with reality through its conceptual roles. For example the gene's conceptual role in the construction of the Mendelian theory of heredity transmission as a stable unit of gametic material played a referential role for the concept through the reliance on that role to develop accounts of heredity further, and deal with more complex heredity phenomena. The conceptual role of the neutron with respect to the introduction of quantum mechanics to the nucleus, translated referentially as well for researchers, through the hypothesis of a neutral particle that served a particular theoretical role as an object obeying quantum laws and principles in the context of the nucleus. This in addition to its explanatory and experimental roles, was certainly a basis for its own further elaboration and investigation. These roles all in some way were meant to reflect vaguely and loosely of course, some aspect of reality.

Experimental roles, producing and probing certain phenomena, have similarly I believe been used in a referential way by researchers. This relates of course to Hacking's theory. In these instances the concept referred to whatever was being manipulated experimentally to produce these phenomena; whatever was being used so to speak in these experimental set-ups as part of achieving the goals of research. The neutron concept thus referred to the causal agent that was being deployed productively by virtue of the concept in nuclear radiation experiments to break apart nuclei or test nuclear forces. Experimenters fixed reference through their ability use the entity concept as part of their research agenda. Hence these experimental uses were relied on to identify the object, as the object utilisable for testing and producing relevant and important phenomena for the purposes of research.

There’s another supporting dimension however, because as we’ve seen these roles also played a part in the development and investigation of the underlying entity itself. If these epistemic roles are reliable, and thus in the minds of scientists referring, then they should lead to more information and ultimately a better understanding of the entity. The investigation of a theoretical entity obviously relies on having some presumptions available first, that themselves fix the reference of the entity by providing some kind of grip on the entity, whether it be knowledge of its explanatory relations, its experimental roles or conceptual uses. These could be said to provide a degree of ‘epistemic access’ to the underlying entity in the sense of not just providing experimental access, but also providing necessary conceptual structure and explanatory relations, out of which further ideas about the entity could be built. In this sense we add a ‘semantic’ dimension to Kroon’s argumentation but with the difference that on this account there needn’t be a distinction in kind between relying on these roles to further investigate and describe the underlying entity or using these roles as part of achieving the goals of its associated process of research. In either case reference-fixing is based on the fact that these roles can be relied upon for further investigation in the course of research, and successful reference itself is correlated in practice with the success in doing so.

The role of descriptive elements for fixing reference in practice then can be reduced to those which were central or even constitutive of the epistemic roles which the entities played. Mostly the way these roles were to be worked out, and their interpretations were themselves a subject of research. Some descriptive elements however as I suggested were taken by scientists as constitutive of the explanatory, experimental or conceptual roles the entity concept was required for, such as neutrality in the case of the neutron, or the unit-nature of the gene. As such those properties I mentioned in the introduction as broadly common to theoretical entity concepts identified the object as an ‘entity’ were relied on themselves as basic aspects of these roles. These descriptive elements didn’t usually extend so far as to outlining causal-explanatory mechanisms, and certainly the use of causal-explanatory roles hasn’t seemed to rest upon these. In fact it works the other way. The causal-explanatory roles were the basis in practice for thinking about the mechanism, along with shifting background elements, as we saw in the gene case and would hesitate in many other cases too such as the continental explanations of geological phenomena.

Of course in practice what researchers have believed about their entities at any one time or other is various. They've made all kinds of assertions. What we've been tracking though are the reference-fixing elements, those elements of the entity concepts which have ultimately been important for reference-fixing, and thus for the communication of reference, and agreement on it over a period of time, which captures the open-ended disposition scientists have historically held in general towards such concepts. When this is factored in it seems to me that what scientists fell back on were these epistemic roles for orienting their own developments of the concept in the research context, and for agreeing about how the concept framed their research as ongoing investigation. What mattered for reference-fixing was having a shared basis for the use of the concept in furthering their research that acted at the same time as some kind of epistemic access to the underlying entity, as a basis for its own further investigation. Different scientists picked up the concept and used it differently and in the process developed it differently, but agreed nonetheless about the reference, because they were guided by the same underlying elements which governed its use and development, even if the interpretations of these roles they had were different. But because these roles did act as resources and constraints for that use and development, they stood to provide some grip on the world for the purposes of proceeding further relative to the concept's correspondence in reality, and thus could be used to fix a referent as vague and uncertain as they might be when expressed in terms of these roles. In a sense then the methodology scientists used generally with respect to theoretical entity concepts, assessed through their general behaviour and dispositions towards them over time, fits with this kind of rationality, whereby they fixed the referent in this loose way in order to give themselves a common platform with which to address their research problems and investigate further. Of course these roles might often have been sufficient for realist belief in the underlying concept, in which case the content of those beliefs would be at base spelled out in terms of those roles, and their relations and presuppositions about the entity, leading to a more or less open-concept realism.

So this is somewhat of a speculative attempt to find a rationale for what I've observed about the reference-fixing practices historically with regard theoretical entity concepts, and the general open-endedness which characterises their lifetimes. It's speculative simply because scientists generally haven't and don't talk in terms of reference. Digging out these practices, requires looking at the historical record and instances of continuity and discontinuity, and how scientists have persisted with these concepts at individual and group levels. This is what I tried to do of course in the neutron and gene cases. When you take into account the fact that the significant lives of these entity concepts and the reasons for their postulations are internal to ongoing processes of research, then I think it's this kind of theory on how scientists fixed the reference of these terms that stands to make some sense of their dynamic histories and fluid and flexible descriptions. Theoretical entity concepts, more than I assume any other types of ontological suppositions scientists make, have been deployed in order to resolve research problems and issues, or at least provide a basis that could help in this regard. As such what has mattered for researchers, fully aware of the uncertainty and complexity of the phenomena they have been generally dealing with was having resources and constraints for conducting that research. Using these roles to fix reference arose from an implicit judgement that if there is some level of correspondence (some 'grip') this should correlate with the productivity of these roles in directing and resourcing the ongoing process of research for which the concept was developed.

One can't discount then the essential role that ongoing processes of research has played then in reference-fixing for these concepts. It was relative to the epistemic goals of these processes that entities have been postulated and the epistemic roles of their concepts defined. It was of course in the context of research that scientists have chosen this methodological practice for fixing reference, because it suited the circumstances and suited the theoretical nature of these kinds of concepts, allowing the necessary space in which research could happen and alternative ideas could develop and be debated, with the acknowledgement generally that research proceeds normally by piecemeal development and successions of partially or approximately true ideas that are generally in need of improvement, especially as new phenomena continually arise making the picture inevitably more complex. Alternatively we might say the reference has worked in practice to fix theoretical entity concepts at outermost levels, as the basis for using them to move uncertainly and hesitantly inwards. This weak specification imagines of course that the entity might have turned out to be nothing like what was expected of it. It may have turned out not to be an entity at all, but something less conceivable in these terms. But the methodological point is that such information would have emerged in the course of research by structuring research with these epistemic roles, such that if there was something really underlying the concept, it would have been eventually resolved more clearly and precisely by aid of them in the context of theoretical and experimental development.

As such there remains a certain artificiality about assessing them independently of their ongoing research contexts. At least philosophically we have to be wary of doing so, since ongoing processes have apprehended the change in their concepts as an ordinary and frequent part of investigation. Outside of these contexts, teaching or communicating to the public for instance, other standards of reference may indeed apply. But inside the emphasis is on having shared elements (concepts) or indeed ‘reference points’ for the pursuing this research, which can be relied upon because they fix loosely and vaguely at least some aspect of the world through these roles and their constitutive properties. Of course whatever develops by the use of these roles in the course of the research will conform to them, and they remain thus consistent identifying elements.

6.5 What is there to be realist about?

What I’ve done here then is to try to motivate these referential practices of researchers with respect to theoretical entity concepts (or precisely terms in the case of reference). What I haven’t done is try to justify this reasoning on reference, which seems to me to rely on a more or less traditional abductive pattern of inference positing a relation between success and reference, as difficult as ‘success’ can be to define. I won’t attempt to get myself involved in these kinds of extensive arguments over the efficacy or otherwise of abductive inferences for this kind of purpose, which have already been well explored.[507] If it were justifiable though even to a degree, it stands to have considerable importance for how we understand realism about theoretical entities and the realist debate, because while it implies a greater level of referential continuity, which of course might bolster a realist position, on the other what can be described by means of those elements is much weaker and much less clear.

In the realism debate, one of the classical arguments against realism about theoretical entity concepts, is the pessimistic meta-induction argument which is attributable perhaps most precisely to Laudan and Devitt.[508] It argues that the sequence of shifts with respect to the reference of concepts, or the regular abandonment of those concepts, provides us with an

[507] See for instance, among many many others, Larry Laudan, “A Confutation of Convergent Realism” (1984), reprinted in eds. Richard Boyd, Philip Gasper & J.D. Trout, *The Philosophy of Science,* (MIT Press: Massachusetts, 1991), Stathis Psillos, *Scientific Realism; How science tracks truth*, (Routledge, London, 1999); Peter Lipton, *Inference to the Best Explanation* (New York, Routledge, 1991).
[508] Larry Laudan, “A Confutation of Convergent Realism” (1984), reprinted in eds. Richard Boyd, Philip Gasper & J.D. Trout, *The Philosophy of Science*, (MIT Press: Massachusetts, 1991), M. Devitt, *Realism and Truth* (Oxford, Blackwell, 1984).

inductive argument to the effect that we should believe that all our current concepts and terms will shift their reference or be abandoned in the future. At the same time, anti-realist observations of meaning shifts serve to undermine the relation between success and reference that realists so often rely on. Successful terms have been frequently abandoned, according to Laudan for instance, which implies that the assertion of such a link is flawed, as intuitive as it might seem. Of course one way of dealing with these arguments from the historical record, is to argue that one must be more precise about what counts as important success (novel success for instance)[509] for rational realist beliefs, and what elements of theories and concepts have been historically responsible for these. Hence Psillos argues for instance that elements of the interpretation of the aether and caloric models, did not contribute to the novel successes of the theories in which they were embedded, and thus do not stand as contradictions to realist argumentation. Others have countered this historical interpretation![510]

I've got nothing to say here about the link or otherwise between reference and novel success, and the positive arguments for being realist. But by looking at the elements of continuity scientists have themselves relied on in research practice, and by making the assertion that this practice of reference itself is based on the potential efficacy of these elements in influencing research in successful directions of investigation and development, we have a suggestive basis for a realism that sidesteps the problems of pessimistic meta-induction and incommensurability, by bringing in a more acute understanding of scientific practice.

Although I can't give a thorough account here, the ramifications of future investigation in this area are quite interesting, because they suggest a rather different content to potentially justifiable realist beliefs than is put either by traditional scientific realists, or even structural and experimental realists. It envisages that what we can be realist about given the shifts in descriptions that theoretical entity concepts continually undergo, is at minimum something only vaguely refined by these epistemic roles and constitutive properties. This would reflect the vagueness and open-ended of the use of the concepts that I've argued is characteristic of their history. So we reduce our realist claims to one that meets with the typical scientific practice with respect to these concepts. That is, we commit our realism to those aspects of these concepts that constitute the use and further investigation of them, and which, when they

[509] See Martin Carrier, "What is wrong with the Miracle Argument", *Studies in History and Philosophy of Science*, 22(1) (1990), pp. 23-36, for a good discussion of this.
[510] Hasok Chang, 'Preservative Realism and Its Discontents: Revisiting Caloric', *Philosophy of Science* 70 (Dec.) (2003) pp 902-912.

are relied upon to develop a process of research, are interwoven with the success of the concept. Of course the content of our beliefs are then rather limited. We believe in an entity that plays these epistemic roles for researchers and has certain properties necessary or constitutive for those roles. This would mean in the gene case we believe in an entity with identifiable unit structure in the gametic material that is stable and replicates, and explains ultimately trait phenomena and is conceptually, through these properties, a central element of thinking about hereditary phenomena and the framing its biochemical basis. This seems to be at base the content of scientific beliefs even today. Different descriptions and interpretations have naturally developed of what the gene is, but underlying it is something like this framework which has had a central part in its development. Likewise the particle concept is today itself a subject of continuing research. The neutron is bound up with that, and while the 'particle' is undoubtedly changing, and our ideas of the neutron are reforming, it is doing so within the context of its epistemic roles and its imputed neutrality and structural part of the nucleus. The neutron is a unit of nuclear structure and a unit of nuclear forces, and an experimental tool for the examination and production of nuclear phenomena. The quark model itself, although a fundamental change in the understanding of the neutron and other particles, was a development that attempted in itself to account for its role in nuclear forces.

Note this is more or less the open-concept realism which we postulated was the type of realism appropriate for describing early scientific attitudes towards the gene. Our beliefs are restricted to a very open understanding of what the entity might be, and allows that the expectations and presumptions we might have could be significantly wrong. As such what we can assert about our entities where research is ongoing is somewhat restricted to the roles the entity concept plays in furthering and structuring that research. This gives us naturally only a very vague idea about the entity's description, limited to its constitutive properties in this respect. The neutron concept interpreted in this way is nothing more than a mass-bearing neutrally charged particle, where the notion of particle itself is not definite. We might not even know what *kind* the entity instantiates. A thing like a gene is thus a very vague thing corresponding to a self-replicating stable unit of genetic material whatever that may be. But nonetheless our beliefs will be more or in line with the contingency and variability of research processes. These beliefs would take the form in line with researchers that we have through the use of these roles and properties in the successful development of theories and explanation and the creation of fruitful paths of investigation and experimentation, even if only piecemeal and imperfect, picked out some unobservable or only indirectly observable aspect of the world that conforms to these roles and properties.

This a minimal realism of course, but it is more in conformity as I have said with research practices and the disposition of scientists regarding the continuity of their concepts and their reference, and even at times their own realist attitudes assessed in the context of processes of research, and thus I suggest has a better chance of beating pessimistic meta-induction claims, if not finding a more stable relationship between reference and success. It is also a position much more wary of what science actually is, acknowledging the centrality of ongoing processes of research to any assessments of realism, and thus suggesting that the current realism debate, with its debate over incommensurability and the like, is somewhat artificially contrived. Where research is ongoing with respect to certain concepts for which the concepts remain in practice open-ended, which they almost generally are, then mounting a realism debate on concepts as they are extracted from those processes at any one time or another, with the help of particular descriptions, is at least presumptuous of what will happen in the future and at worst relies on artificial reconstructions to assess and argue for and against a philosophical position, that simply misconstrue what these concepts in the context of research are and how they are connected across time and research fields. This in effect only serves to misconstrue what the content of realist beliefs should actually be. Realism should be assessed only when a proper account of the connection between theoretical entity concepts and ongoing processes of research has been fathomed, and the content of what could be realist beliefs, or what there actually is to be realist about, can be understood.

Chapter 7: Conclusion

What I've tried to achieve in this dissertation is a new historical perspective on the importance of theoretical entity concepts in scientific practice that stands I hope to give us a better account and understanding of the referential practices of scientists with respect to such concepts and in the end a more appropriate understanding perhaps of what the claims of a 'scientific realism' more in tune with practice might look like. I've proposed the importance of exploring the epistemic roles of these entity concepts, because it's only through these that we get a more historical picture of what such concepts have contributed to historical research processes. This effects I believe a rather significant shift in the way philosophers, and only recently historians, have traditionally pursued such concepts, from rather static products of research which could be read off in its different variants from the constructions of beliefs of scientists at any point of time, to more dynamic conceptions which capture their role for processes of research and the disposition of researchers to treat them open-endedly, as 'epistemic frames', rather than as simply ring-fenced bounded structures of information or representational content. In this vein it tries to provide a framework for understanding the basis of their development, and the methodological position of researchers that allowed them to develop, in the context of complex research and uncertain phenomena, as embedded parts of this research. It posits that theoretical entity concepts have been central but dynamic parts of constantly adapting and evolving research processes, defined by certain epistemic goals. They have both helped develop and been developed by these processes.

Herein then lies what I would consider the novelty of this dissertation. Most of the concern of philosophers has been with the meaning of these concepts, how they could be defined at particular instances and compared, and the philosophical conclusions that could be reached from doing so. This has left the epistemic dimension of their participation largely unexplored with a few historical exceptions. Yet such epistemic accounts I believe show us that many of our philosophical positions on reference and realism are really not much better than abstractions, that don't reflect the reality of the way science in fact works, as it has tried to work towards better more productive accounts of the world. My tentative account of referential practices builds in the necessity and inevitability of descriptive change, and the ongoing ever-developing nature of research, and the importance of having concepts to structure this pursuit into the unknown and complex. In this vein I think I share the viewpoint of Richard Boyd, that research is and has been a dynamic struggle of researchers to find not just the best descriptions but the best methodology to deal with complexity and uncertainty before them. The use of theoretical entity concepts is itself a microcosm of how methodology has taken shape in this respect, and how scientists themselves have had to adapt their referential practices for instance to permit their own progress, even if only small step-by-step.

But of course what I can claim to really have done here is to have setup and motivated the importance of giving such epistemic accounts historically for history and philosophy of science. I've provided a schematic framework of epistemic roles for giving such analyses, and applied it to two case studies. But of course a motivation is one thing; a complete developed account another! There are still many avenues unexplored, many loose ends untied. I've given no account for instance of when reference shifts have occurred or what has motivated them. I've presented little on the methodology of reference failure. Nor have I tried to compare the reference of entity concepts used in non-overlapping research. And what happens to an entity concept when a research process ends, through a shift in the goals of the practitioners working in the field? When and in what contexts have entity concepts become 'defined' and fixed, if ever? How in general do entity concepts live beyond those processes they were designed for? All these are complicated empirical questions to which I haven't had the space or time to pursue. However if this position is acknowledged as fruitful then of course these are the directions such research could take. It seems though to me there is still an enormous amount to be said about what theoretical entity concepts and scientific concepts in general do and have done for science that we philosophers and historians had no idea about.

On a final note I think that the way I've pursued the topic of theoretical entity concepts is essentially interdisciplinary in nature. I've tried to develop a historiography of such concepts that is relevant too, and sheds light on, philosophical problems and issues. In this sense is draws philosophy into closer connection with having historical accounts, and a proper basis for giving those accounts where understanding scientific practice is a key aspect of what it is we wish to assert philosophical, about methodology, epistemology or otherwise. Interdisciplinarity is no doubt a relatively novel thing, with still much work to go into finding the fruitful bases with which to interact. What I've implicitly suggested here is that a proper historiography for a certain aspect of science, and the historical accounts that emerge from applying it, such as for accounts of theoretical entity concepts in practice, is not divorced from philosophical issues in general, and can itself in turn provide a method for addressing and understanding these issues. I trust that here too, there is still much more to be said.

Abstract - Deutsch

Diese Dissertation beschäftigt sich im Allgemeinen mit den epistemischen Rollen von Konzepten theoretischer Entitäten und zielt auf einen neuen Begriff der historischen Rollen solcher Konzepte in der wissenschaftlichen Praxis und die philosophischen Folgerungen, die sich aus einer solchen neuen Begriffsfassung für die traditionellen philosophischen Probleme Referenz und Realismus: bezüglich ihrer Rolle als begriffliche, erklärende und experimentelle Ressourcen und Hemmnisse für die Theoriebildung und das Experimentieren.

Die These der Arbeit ist, dass dieser neue Begriff theoretischer Entitäten ein besseres historisches Verständnis davon vermittelt, was solche Konzepte sind, und wie sie im spezifischen Kontext eines laufenden Forschungsprozesses verstanden werden sollten. Mein Argument bewegt sich weg davon, Konzepte und begriffliche Bezugssysteme in historischer Perspektive einfach als Output aus dem Forschungsprozess zu sehen und unterstreicht ihre Rolle als Mittel für genau diese Prozesse, indem gezeigt wird, wie Wissenschaftler sich zentral auf unbeobachtbare oder versteckte Entitätskonzepte beim entwerfen und ausstatten von Forschungsprozessen verlassen haben. Solche Konzepte können folglich nicht als einfache Repräsentationen oder zweckgebundene Inhalte (*ring-fenced containers*) verstanden werden und werden oft in dieser Hinsicht als *open-ended* in einem gegebenen Forschungsprozess benutzt, um mit den Kontingenzen und Komplexitäten ihrer Phänomene und ihrer eigenen unzugänglichen und zugrunde liegenden Entitäten umzugehen. Ihre Variabilität ist tatsächlich ein essentieller Aspekt ihrer historischen Rolle in der Methodologie fortschreitender Forschungsprozesse. Insofern entwickelt diese Dissertation ein analytisches Bezugssystem, das auf um historische Begriffe angewendet werden kann, das durch die Fälle des klassischen Gens und frühen Konzepten von Neutronen unterstützt wird.

Die enge Verbindung mit laufenden Forschungsprozessen wiederum hat philosophische Konsequenzen, sie schlägt eine alternative Basis für den Fortbestand solcher Konzepte und Praktiken der Referenzfixierung im Forschungsprozess vor. Diese Praktiken beruhen auf dem Gebrauch der Konzepte durch ihre epistemischen Rollen und den Eigenschaften, die sie mit den zugrundeliegenden Entitäten verbinden; z.B. sind Inkommensurabilitätsargumente fehlgeleitet bei Begriffen von Kontinuität und Referenz und Ideen darüber, was solche Konzepte sind, die alle philosophische Abstraktionen sind und nicht Teil historischer Forschungspraxis. Schließlich resultiert dieses Verständnis in einer besseren Formulierung des Realismus, das besser an die dominanten epistemischen Leben dieser Konzepte in der Praxis angepasst ist.

Abstract- English

This dissertation concerns generally the *epistemic roles* of theoretical entity concepts, aiming at a new account of the historical role of such concepts in scientific practice in terms of their epistemic contributions: that is their roles as conceptual, explanatory or experimental resources and constraints for theory-creation and experimentation. The thesis is that this gives a better historical understanding of what such concepts are and how they should be understood in the specific context of ongoing processes of research, that moves away from treating concepts and conceptual frameworks historically as simply the outputs of research processes, and highlights their roles as means for those very processes, by showing how scientists have relied centrally on unobservable or hidden entity concepts to frame and resource processes of investigation and research. Such concepts cannot thus be understood as simply representations or ring-fenced containers, and have often been employed open-endedly in this respect through their roles in a given process of research for dealing with the contingencies and complexities of its phenomena, and their own inaccessible underlying entities. Their variability is thus in fact an essential aspect of their historical part in the methodology of ongoing research processes. As such this dissertation develops a framework of analysis that can be applied to giving historical accounts in such terms, relying for specific support on the classical gene and early neutron concepts.

In turn this intimate association with ongoing research has philosophical implications, suggesting a basis for the continuity of such concepts and the practices of reference fixing in the research context. These practices rest on their use through their epistemic roles, and their non-determinative nature, and the properties these roles associate with the underlying entities. As such incommensurability arguments are somehow misdirected at notions of continuity and reference, and ideas of what such concepts are, which are the result of philosophical abstractions, not part of historical practice. Finally, understanding this gives us in fact a better formulation of scientific realism, which is more adapted to the dominant epistemic lives of these concepts in scientific practice.

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Konrad Lorenz Institute for Evolution and Cognition
Adolf Lorenz Gasse 2
A-3422, Altenberg, AUSTRIA
Tel.: +43-2242-32390-0
miles.macleod@univie.ac.at

Biographical Details

Born 08/09/77 in Wellington, New Zealand
Nationality: Australia, New Zealand

Education

- BSc (Hons), Sydney University (2002)
- LLB, Sydney University (2001)
- MSc, Program History and Philosophy of Science, Utrecht University (2006) – *cum laude*

Current Research Position

Postdoctoral Fellow (From November 2009) - **Konrad Lorenz Institute for Evolution and Cognition**

Former Research Positions

PhD Program (Oct. 2006 – Oct. 2009) - *Initiativkolleg Naturwissenschaft im historischen Kontext, University of Vienna*

Thesis title: *What is there to be realist about? The epistemic roles of theoretical entity concepts.*

Department of Applied Mathematics, University of Sydney (January 2003 – July 2004)
Position: Research Associate (with Prof. Nalini Joshi)

Publications

'The 'epistemic-only' view of natural kinds: a reply to Thomas Reydon', *Proceeding of the first conference of the PSE (Philosophy of Science in a European Perspective),* (forthcoming, 2009).

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Conferences/Presentations

Organised and authored symposium 'Scientific Concepts in the Research Process' for the *Second conference of the EPSA (European Philosophy of Science Association),* October 2009.

Colloquium Address, *Committee on the Conceptual Foundations of Science – University of Chicago,* March 2009

First conference of the PSE (Philosophy of Science in a European Perspective), December 2008.

3rd International Conference of the European Society for the History of Science, September 2008.

'The Formalist Interpretation of Berkeley's Philosophy of Mathematics', *International Berkeley Conference*, September 2005.

Teaching Positions

Department of Philosophy, University of Vienna (September 2008 – January 2009)
Position: Instructor (introductory course on philosophy of science)

Department of Philosophy, University of Utrecht (February 2006 – April 2006)
Position: Tutor (occasional lecturing) (senior level course on philosophy of science)

Institute for Foundations of Science, University of Utrecht (September 2005-December 2005**)**
Position: Teaching Assistant (junior level course on history and philosophy of science)

Department of Philosophy, University of Utrecht (February 2005 – April 2005)
Position: Teaching Assistant/Tutor (senior level course on philosophy of science)