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1 Introduction

In recent years, scientific pluralism (SP) has become one of the household positions in the philosophy of science and the philosophy of biology in particular. While pleas for the "disunity of science," against foundationalism, and for epistemological and ontological SP were quite controversial in the last decades of the 20th century, SP is nowadays a rather standard position. In some fields in the philosophy of science, it is becoming increasingly hard to locate any opponents of SP. Has SP, thus, become universal and uncontroversial? And why should we care?

There are two main reasons. First, SP bears on several practical issues in the sciences. Accepting SP entails a particular perspective regarding science policy. SP usually requires a normative commitment. The sciences should become more pluralist. The scientists should adopt pluralist interpretations of target phenomena. The question of how well SP resonates with practicing researchers has, however, not been answered yet. Are researchers pluralists? And if the answer is yes, does their pluralism resonate with philosophers' SP? The first reason why SP needs to be reconsidered is therefore a practical one and requires empirical examination.

The second reason is a theoretical one. SP lies in the vicinity of another position that is well known within philosophy and the philosophy of science: epistemic relativism (ER). Both positions accept a form of "plurality." The scientific pluralist believes that there is, or should be, a plurality of different approaches. The epistemic relativist believes that there is more than one epistemic system and that there is no way of judging between these systems in a system-independent, "neutral" way. Although there seems to be some intuitive vicinity, card-carrying scientific pluralists often demarcate their position from ER. Critics of scientific pluralism often ask whether SP entails ER. Although such "accusations" are quite frequent in the literature, there is no in-depth discussion on these issues.

Most of the time, card-carrying scientific pluralists claim that because they do not "renounce judgment," they do not support relativism. If one studies works by defenders of relativism, it becomes clear that card-carrying relativists do not renounce judgment either. If both positions accept "plurality," and if neither position "renounces judgment," what is, then, the relation between pluralism and relativism? In conclusion, the second reason why SP needs to be reconsidered is that an in-depth account of the relation of SP and ER needs to be provided.

These two main goals are two novel contributions to the literature on scientific pluralism. They will be addressed in two consecutive parts of this dissertation. To conclude this dissertation, I will synthesize practical and theoretical conclusions. In so doing, I will propose a way to formulate a pluralist position that takes into account practical problems of pluralism in the sciences and to reconcile SP and epistemic relativism.

The first part is empirical. I will introduce a particular case study: scientific pluralism in small RNA inheritance. I will proceed in three steps. First, I introduce small RNA inheritance by discussing the history of the research field and its current status. I will particularly explain how small RNA inheritance bears on debates surrounding the Extended Evolutionary Synthesis (EES). I will also propose possible pluralist interpretations of small RNA inheritance and their possible benefits.

Second, I will question some of the pluralist claims I have proposed. I will challenge the normativity attached to scientific pluralism and ask: does SP resonate with researchers' aims and goals? If a field displays plurality, do researchers think that this is a good thing? Third, I will try to answer these questions by employing a sociological case study. In these three steps, I will put SP to an empirical test, and answer the question: what is at stake in a research field that displays plurality?

The second part of this dissertation is theoretical. I will proceed in two steps. First, I will provide an overview and synthesis of the SP literature. Particularly, I will examine ways of classifying pluralist positions. I will criticize the current way of classifying pluralist positions and propose alternatives. Second, I will carefully examine the relation between SP and epistemic relativism. I will argue that common strategies of demarcating SP and epistemic relativism fail. I will particularly examine value-based epistemologies as tools for setting apart pluralism and relativism.

In the concluding section of my dissertation, I will synthesize the ideas and conclusions of the first two parts. I will propose a pluralist position that is informed by the sociological case study as well as by my considerations regarding the relation of SP and epistemic relativism.

Before commencing with the empirical part of this dissertation, I will situate this dissertation in the existing philosophical literature. As outlined above, I will discuss problems that concern different disciplines. Some problems concern particular issues in the philosophy of biology. I will examine small RNA inheritance as a contribution to the debate regarding the Extended Evolutionary Synthesis (EES). Some problems concern issues regarding scientific pluralism in general or scientific pluralism regarding biological phenomena in particular. My discussion on relativism transcends the SP debate and the philosophy of science. I will discuss problems that bear on the epistemic relativism debate in philosophy in general.

Owing to the different nature of the parts of my dissertation, I will employ different methodologies. In the empirical part, I will rely on several strategies. My approach is grounded on an analysis of scientific practice which is common in contemporary philosophy of science (Dupré & Nicholson, 2018). My reconstruction of small RNA inheritance and its history was based on a careful examination of research papers as well as numerous conversations with practicing small RNA inheritance researchers and an examination of the reasoning and practicing in laboratory settings. It is, however, my aim to show that such an analysis of scientific practice can often lead the philosopher of science astray. Positions expressed in papers are often not in line with the status of these positions in the field. I, therefore, complemented the analysis of scientific practice, with a sociological examination. I employed qualitative sociological methods to assess the status of the research field in detail.

In the second part of the dissertation, I will employ traditional methods of philosophy. I will use conceptual analysis to systematically analyze scientific pluralist positions and their implications. I will also use conceptual analysis, to understand the relation between epistemic relativism and scientific pluralism and clarify several issues regarding this relation. Furthermore, I will use conceptual analysis to elaborate on the implications of value-based epistemologies and their success in setting apart pluralism and relativism.

In summary, I will combine several different topics that are prominent in different areas of philosophy. I will also combine different methods, some of which are traditional tools in the philosophers' toolbox, while others are quite new philosophical tools and others have been imported from empirical disciplines.

Owing to the heterogeneity of topics addressed and methods employed, the goals of this dissertation are heterogeneous as well. They range from contributing to a science, analyzing the target scientific field from a sociological perspective to using these insights to contribute to philosophy of science. It is, however, my opinion that this outcome is very much in line with recent developments in philosophy of science and its "practice turn." Making more and more sustainable contacts with the research discipline studied also results in a different kind of philosophical contribution. A close examination of the scientific practices, interviewing and visiting laboratories changes the philosophers' perspective and leads to new focuses and new contributions. In this dissertation, my aim is synthesizing the approach of traditional philosophical problems and the exploration of new grounds when examining scientific practices.

2 Small RNA Inheritance

In this chapter, I will develop a case study on scientific pluralism (SP) in small RNA biology. I will focus on the inheritance of small RNAs and examine several versions of pluralism. This chapter will be somewhat theoretical in the sense that it surveys mechanisms involved in small RNA biology with a specific focus on the inheritance of small RNAs. As part of this approach, I provide some historical context. I do not intend for this account to qualify as history of science as this would go beyond the aims and scope of this dissertation. Nevertheless, some conceptual problems require a historical perspective. Against this background, it is my goal to explain why the discovery of small RNAs and their inheritance was and is a breakthrough in molecular biology. In the first two sections of this chapter, I will answer the following questions:

1) What did Andy Fire and Craig Mello show in their 1998 paper and why was it important?

2) Were Fire and Mello's findings new?

3) How did the field of small RNA biology and small RNA inheritance as a subdiscipline evolve between 1998 and 2018?

In the third section of this chapter, I will propose pluralist interpretations of small RNA inheritance. After reviewing the promise of mechanistic, explanatory and theoretical plurality, I will argue that different theoretical approaches are required to explain DNA and RNA inheritance, respectively. In other words, the inheritance of small RNAs presents an instance of "Lamarckian" inheritance, complementary to "Darwinian" inheritance, which is DNA-based.

The controversy centering around the question of whether there are "real" instances of Lamarckian inheritance is notorious. Many researchers consider the resurrection of Lamarck's ideas a fad (Haig, 2007). I will address this concern by showing that the inheritance of small RNAs is "truly" Lamarckian. For the sake of clarity, I will propose criteria for differentiating between the "Lamarckian" Inheritance of Acquired Traits (IAT) and other forms of IAT (Veigl, 2017). After proposing a pluralist interpretation of small RNA inheritance, I will argue that theoretical pluralism is a fruitful description of the processes that govern inheritance. To elucidate this point, I will enumerate the benefits of adopting a pluralist position.

Having advanced my armchair position on pluralism in small RNA inheritance, I will put my conjectures to the test in the following chapters. In the second part of my case study, I will survey the perspectives of the "core set" (Collins, 1981b) of small RNA inheritance investigators.

2.1 Small RNAs – from "spooky science" to the Nobel Prize

In this section, I will provide an account of the discovery of small RNA-based phenomena. I will describe in detail the findings of Andy Fire and Craig Mello (F&M), who are considered to be the discoverers of RNA interference. I will, however, not take this narrative for granted and will instead investigate the role of small RNA-related discoveries in plants. I will emphasize that the marginalization of plant-based phenomena is inextricably linked to the history of small RNA inheritance.

2.1.1 The 1998 paper – what it said and what it solved

The success story of small RNAs commences with a very short piece in the journal *Nature* in 1998. F&M reported on the "*Potent and Specific Interference of Double-stranded RNA in Caenorhabditis elegans*" (Fire et al., 1998). There is little scholarship on small RNA biology in the history and philosophy of science (exceptions are Ronai, 2017; Scholl, 2016; Théry, 2015). Small RNA biology is biochemically intricate and might be inaccessible to philosophers not well acquainted with the field. It is nevertheless my goal to explicate concepts of small RNA biology in an accessible way. Explaining the importance of the 1998 paper mentioned above is pivotal. It will significantly facilitate the understanding of developments after 1998 as soon as it becomes clear what F&M demonstrated and why this was important. I will explain the 1998 article in two steps: first, I will divide the title of F&M's paper into four sections and explain each of them individually. Second, I will summarize other general positions and speculations by the authors.

There are four different sections in the title that need to be elucidated: 1) "potent and specific"; 2) "interference"; 3) "double-stranded RNA"; and 4) "Caenorhabditis elegans." In the model organism Caenorhabditis elegans (C. elegans), the authors showed that a particular molecular entity – double-stranded RNA (dsRNA) – causes an effect, which is interference. This effect called RNA interference (RNAi) was shown to be both potent and specific. RNAi had a strong effect, and only dsRNA could have caused this effect. In the next sections, I will address the content of these four sections in more detail.

2.1.1.1 "Double-stranded RNA"

F&M showed that a particular entity, dsRNA, interfered with gene expression. This finding solved several problems that had been pertinent to the research field for almost 20 years. Before discussing these problems in detail, I will provide a short introduction to the received view of the role of RNAs in the 1980s and 1990s.

The so-called "central dogma" of molecular biology states that DNA is transcribed into RNA and that RNA, in turn, is translated into protein. The central dogma assumes a one-directional flow. DNA occupies the hierarchical top in this flow of information. In this traditional picture, RNA has the role of the messenger. RNA carries a blueprint of information encoded by DNA to ribosomes. Ribosomes synthesize proteins based on this blueprint. The transmission of information via RNA is possible because RNA and DNA are very similar molecules. Both consist of long sequences of various permutations of two purine and two pyrimidine bases, which are connected by a phosphate backbone. The specific sequence of these four bases defines the genetic code, and every subsequent pair of three bases is called a "codon." One codon encodes for one amino acid, and amino acids, in turn, are the building blocks of proteins. RNA is different from DNA in that it is somewhat unstable (because it has one additional, very reactive chemical group). RNA and DNA also differ in one pyrimidine base (DNA: thymidine (T); RNA: uracil (U)). RNA is often single-stranded. As a consequence, RNAs form many different structures. These structures are essential for catalyzing biochemical reactions.

As indicated above, RNAs were regarded as messengers that transfer information from the nucleus to the cytoplasm. This view changed dramatically in the last twenty years of the 20th century. Let me emphasize at this point that I am doing a historiographical injustice to Barbara McClintock's groundbreaking work on the role of RNAs in gene regulation in the

1950s (Fox Keller, 1984). Not only is the focus of this case study restricted by the temporal focus but also by a spatial focus, as only discoveries in Europe and the USA are considered. It is nevertheless essential to note that the 1980s and 1990s were not the first period in which the central dogma was criticized. Besides McClintock, other researchers conjectured that RNAs would not only function as messengers. For instance, Roy Britten and Eric Davidson formulated a hypothesis in the 1960s, claiming that RNAs are the regulators of gene expression (Britten & Davidson, 1969).

It is often argued that the impact of a specific paradigm or theory can be measured by how much and how long it holds back the progression of a field. One could thus conjecture that it wasn't until the 1980s and 1990s that it became possible to challenge orthodoxy since a sufficient number of anomalies had accumulated. Although experiments and conjectures centering around the regulatory roles of RNAs were formulated before the 1980s, I will focus on the period starting in the 1980s. This period brought about the findings that finally falsified the central dogma of molecular biology.

In the 1980s, evidence about the catalytic activity of certain RNAs had accumulated. Research on "ribozymes" and "splicing" suggested the versatility of RNAs. Ribozymes are complexes in which RNAs have catalytic activities. They catalyze biochemical reactions, similar to the action of protein enzymes (Kruger et al., 1982). The ribosome is a textbook example of a ribozyme: although ribosomes mainly consist of proteins, their active cores consist of RNAs, which are essential for protein synthesis. Besides catalytic activities in ribosomes, RNAs were shown to be able to slice parts of other RNAs or even themselves. This process is called "splicing" (Berget et al., 1977).

Despite these early findings, the discovery of RNAi was the real turning point in the reception of RNA as versatile biological entities. I will take Ian Hacking's quote "If you can spray them, then they are real" (Hacking, 1983, 24) as the embodiment of this change. The versatility of RNAs was fully recognized when theoretical assumptions about RNAs were implemented as tools for the manipulation of gene expression.

In the 1980s, single-stranded RNAs were injected into model organisms to interfere with gene function. At this time, the idea was straightforward and did not involve the catalytical activities of RNAs. Researchers believed that introducing single-stranded RNAs would silence complementary messenger RNAs (mRNAs). If an injected RNA is "antisense" to an mRNA, it potentially binds the complementary mRNA. For every base of the mRNA, or sense RNA, the antisense RNA contains the complementary base. For each U, it contains an A, for each A a U, for each G a C, and for each C a G. Complementarity leads to very efficient binding. If an mRNA is bound to an antisense RNA, it cannot be translated anymore because the region which encodes information is bound to the antisense RNA. The information is masked; the mRNA is "silenced" (see Fig. 1).

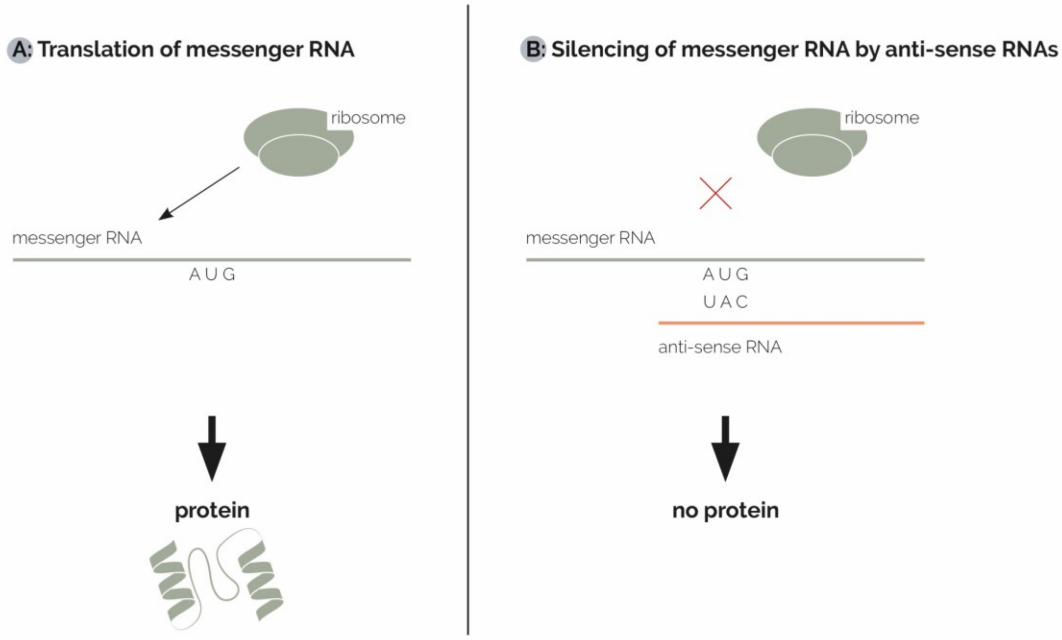

Figure 1 A: In absence of an anti-sense RNA a messenger RNA causes the production of a protein through the ribosome. B: If an anti-sense RNA binds to the messenger RNA, the messenger RNA cannot interact with the ribosome and thus no protein can be produced.

This conjecture provoked two research interests. On the one hand, antisense RNA was used in basic research for so-called "reverse genetics" approaches. Researchers interested in the function of a specific gene injected the complementary antisense RNA to silence the mRNA.

The aim was to infer the function of the silenced gene from the resulting phenotype.[1] On the other hand, antisense RNAs were considered to be potential medical applications (therapeutics). The underlying assumption is that there exists a specific gene product that might cause cancer. One could then insert an antisense RNA to silence the mRNA of this gene! This conjecture was an incentive for companies to develop antisense therapeutics (Oberemok et al., 2018).

It soon turned out that there was a problem with the applicability of anti-sense RNAs. Anti-sense approaches were not successful because effects were not reliably reproducible. Sometimes anti-sense RNA would cause very strong silencing. In other cases, silencing was barely achieved. In 1990, another puzzle was added to the confusion about antisense RNA. Richard Jorgensen et al. published a paper about their attempts of intensifying the color of petunia petals. They wanted to do so by injecting synthetic mRNAs, which encoded the protein responsible for the petal color. Jorgensen et al. hypothesized that by inserting synthetic mRNA, they could increase the number of mRNAs. They conjectured that by increasing the number of mRNAs, more protein would be translated, and the color intensity would increase. The result of their experiment was more than puzzling: the intensity of the leaf color decreased. Jorgensen et al. coined this phenomenon “cosuppression.” In the Material & Methods section of their paper, they reported an experiment that added to the confusion. In a control experiment, they injected anti-sense RNA, complementary to the messenger RNA. The injection of anti-sense RNA also caused cosuppression. These experiments seriously challenged the theoretical validity of antisense techniques. Despite severe conceptual problems, the field did not die out. Problems remained, the field was called “spooky science” and problems were discussed in “spooky science” journal clubs, journal clubs that allowed for discussing phenomena that did not sit well with orthodoxy. The paper by F&M would ultimately provide an explanation for these problems.

[1] “Reverse genetics“ is the opposite of “forward genetics.” Forward genetics approaches are characterized by the interest in a specific trait but not knowledge of the genes involved. In order to find the genes that cause a specific trait, random mutagenization is performed using a specific model organism. The ideal outcome of such a random mutagenizing screen is that each individual organism carries one single mutation in its DNA. Organisms with the desired phenotype are selected and assessed for their changes in DNA structure. The localization of the specific damage to the DNA indicated the location of the gene responsible for the phenotype.

Having addressed the problems faced by anti-sense techniques in the 1980s and 1990s, I now come back to the findings of F&M. Their main finding was that dsRNA, which is sense RNA complementary bound to anti-sense RNA, is responsible for the silencing of mRNA. dsRNA complementary to any mRNA faithfully silences this mRNA. F&M thus showed which molecular entity is responsible for silencing.

F&M were also able to explain why previous sense and antisense experiments worked so poorly. A conjecture concerning these experiments prompted their approach in the first place. Being acquainted with the protocol to synthesize RNAs, they knew that a complete separation of sense from antisense RNA was not possible. Anti-sense RNAs remain in sense-preparations, and vice versa. As a result, dsRNA always "contaminates" preparations. F&M conjectured that the observed effects of sense or antisense preparations were exclusively caused by "contaminating" dsRNA. In previous accounts of successful silencing, effects were not caused by the antisense RNA but by "contaminating" dsRNA. Cosuppression of mRNA by sense RNA was also caused by contaminating dsRNA. In conclusion, F&M not only demonstrated a technique for reproducibly silencing genes but also solved a mystery that had astonished researchers for almost twenty years.

2.1.1.2 "Interference"

In the previous section, I explained the importance of dsRNA for the silencing of an mRNA. "Interference" is the technical term denoting the silencing of mRNAs. dsRNAs interfere with the translation of mRNAs. The term "RNAi" describes all sorts of related phenomena that involve a dsRNA silencing an endogenous mRNA. The term "endogenous" describes all molecules that are native to the organism and not inserted from outside. In turn, the injected dsRNA is exogenous.

2.1.1.3 "Potent and specific"

Why was it important that RNAi is "potent and specific?" I will define these terms and explain how F&M demonstrated the potency and specificity of RNAi. F&M used the term "specificity" to clarify that only dsRNAs that were complementary to a specific mRNA could silence this mRNA. To show the specificity of dsRNAs, they used a reporter gene called GFP – "green fluorescent protein."

A "reporter gene" is a gene that produces a visual readout as an answer to a specific question. Usually, reporter genes are artificially introduced into model organisms and thus qualify as "transgenes." In turn, each model organism expressing GFP qualifies as a genetically modified organism (GMO). Typical questions when using a reporter gene concern the localization of a molecule or the expression level of a particular gene. The nematode C. elegans is an ideal model system for producing visual readouts because C. elegans is transparent. If the reporter gene GFP is "on," parts of the nematode appear green.

F&M showed two things concerning the GFP reporter gene. First, they showed that they could turn a constitutive GFP "off" through the expression of a complementary dsRNA. When the worm carried a GFP reporter gene, it appeared green under the microscope under control conditions. When the researchers introduced a dsRNA complementary to the GFP mRNA, the GFP signal disappeared. This experiment established that dsRNA specifically interferes with the expression of GFP mRNA (see Fig. 2).

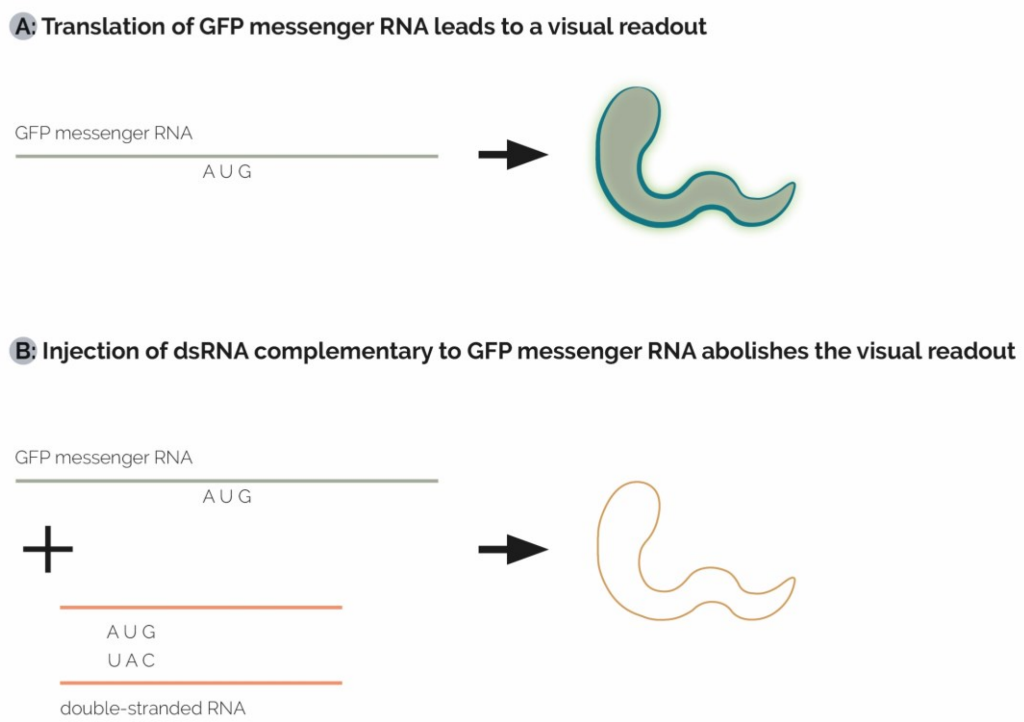

Figure 2 A: Through expression of the GFP messenger RNA, the worm luminesces green. B: After injection of a ds-RNA complementary to the GFP messenger RNA, the worm does not luminesce green, because the GFP protein cannot be produced.

In the next step, F&M used worms carrying two different GFP reporter constructs. One of these GFP constructs carried a nuclear localization sequence (NLS). If a protein carries an NLS, it will be transported into the nucleus. The other GFP construct contained a mitochondrial localization sequence (MtLS). If a protein carries an MtLS, it will be transported into the mitochondria. Fire, Mello et al. relied on these two constructs to increase their understanding of the specificity of RNAi. They synthesized a dsRNA complementary to a sequence specific to the nuclear GFP mRNA. This sequence was not specific to the mitochondrial GFP mRNA. When they treated worms with this specific dsRNA, GFP in the nucleus disappeared, whereas mitochondrial GFP remained. In conclusion, F&M showed in two consecutive steps that the observed phenomenon is specific to the exact dsRNA sequence (Fig. 3).

Raphael Scholl, a historian and philosopher of science, introduced the concept of "causal contrasts" to describe how "potent and specific" was illustrated. Scholl claims that scientific diagrams operate by showing causal contrasts (Scholl, 2016, 77). Usually, causal contrasts are established between 1) control and intervention or 2) one intervention and another intervention. "Specificity" was demonstrated by two subsequent interventions. The first causal contrast was established between the control and animals injected with a dsRNA complementary to GFP. The second causal contrast was established between two different interventions. Animals treated with a dsRNA targeting a region of the GFP construct present both in the nuclear and the mitochondrial GFP were compared with animals treated with a dsRNA targeting a GFP construct only present in the nuclear GFP construct (2016, 79).

Unlike "specificity," "potency" is quite a vague term. There are two ways of contextualizing it. "Potent" might refer to the fact that the effects of dsRNAs on gene expression were much stronger than antisense RNAs in previous years. The term "potent" could also describe the magnitude of the silencing itself. In that sense, "potency" was also established through a causal contrast. Comparing the control with the dsRNA treated animal reveals that the interference is potent, because it decreases GFP expression by 100%. No GFP is visible.

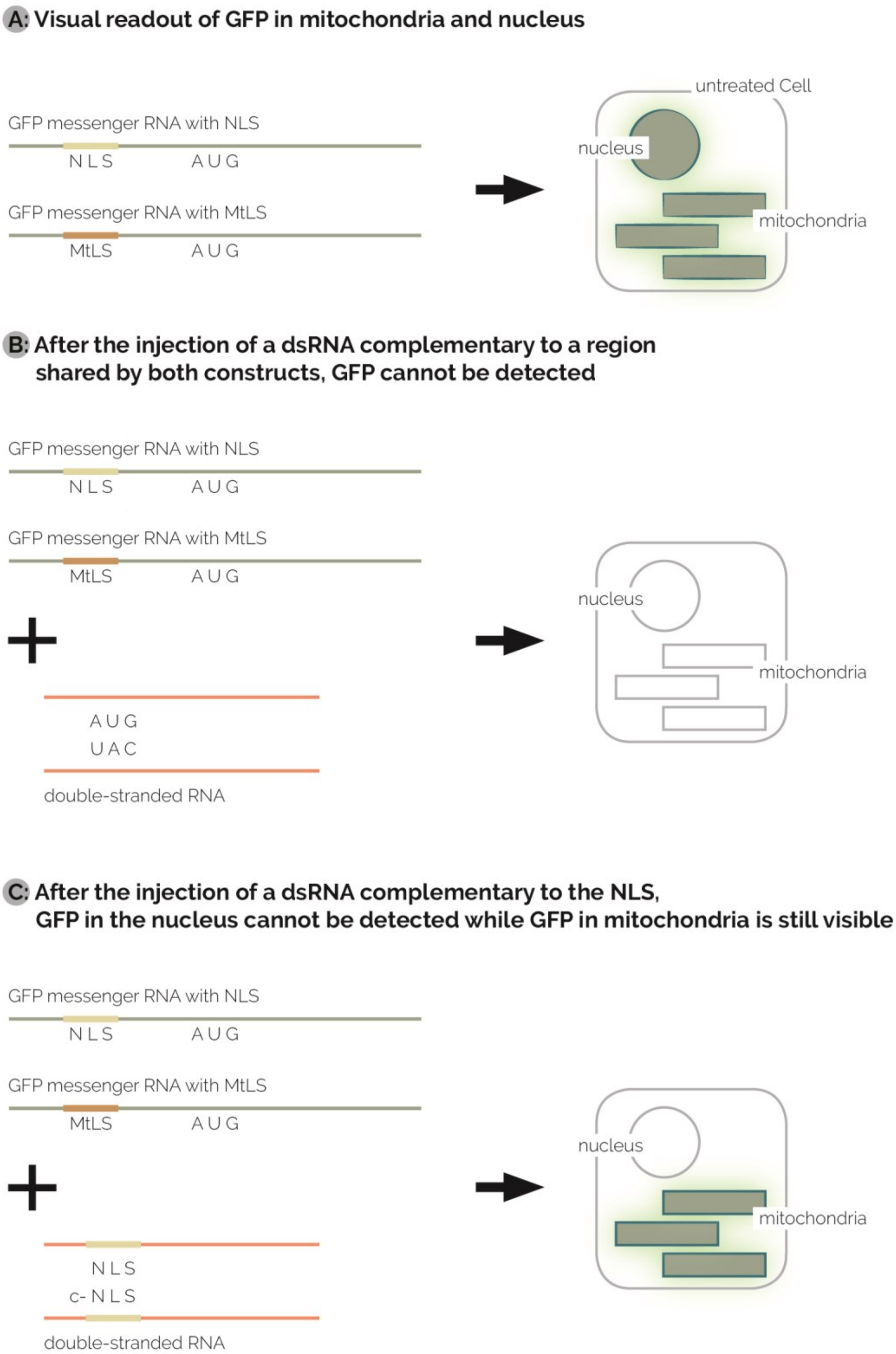

Figure 3 GFP expression A. in control conditions, B. after injection of a dsRNA complementary to a region shared by both constructs, C. After injection of a dsRNA complementary to the NLS, but not the MtLS.

In conclusion, potency was partly established by a causal contrast but also by contextualization. Most researchers who read the M&F paper knew that silencing experiments had failed for twenty years. The relevant research community measured "weak" interference for almost 20 years and was therefore impressed by "potent" interference. Potency is, therefore, relative to the results of previous years.

2.1.1.4 "C. elegans"

In this section, I will provide an account of "Caenorhabditis elegans," the model organism which F&M employed. C. elegans is the most important model organism for the phenomena I describe in my thesis. In recent years, "model organisms" have become a subject of study for philosophers and historians of science. Rachel Ankeny and Sabina Leonelli, both historians and philosophers of biology, define model organisms as a type of "scientific repertoire." In what follows, I will briefly summarize Ankeny and Leonelli's work (2011, 2016) on "model organisms" as "repertoires," the importance of "alignment," as these terms will become instrumental in later parts of this dissertation. I will focus on the model organism C. elegans in particular, focusing on two papers, two by Ankeny (2000, 2001), the other by historian of science Soraya de Chadarevian (1998).

The term "model organism" is relatively young, for it does not often appear in the literature before the end of the 1980s. Ankeny and Leonelli define model organisms as

> "non-human species that are extensively studied in order to understand a range of biological phenomena, with the hope that data and theories generated through use of the model will be applicable to other organisms, particularly those that are in some way more complex than the original model" (Ankeny & Leonelli, 2011, 313).

Ankeny and Leonelli trace the question of whether experimental organisms (of which model organisms are a subgroup of) are "models." They differentiate between "representational scope" and "representational target" of a model. "Representational scope" denotes how extensively results generated with a particular MO can be projected on a broader range of organisms.

While the representational scope is often context-dependent, the representational target is usually well-defined and "characterized along specific disciplinary lines." Ankeny and Leonelli emphasize the importance of the research context and the specific epistemic goals of the investigators. Model organisms have

> "particular experimental characteristics that are closely related to their power as genetic tools: they typically have small physical and genomic sizes, short generation times, short life cycles, high fertility rates, and often high mutation rates or high susceptibility to simple techniques for genetic modification" (2011, 316).

Processes of standardization and the definition of a "wild type" is essential for developing MOs. Historically, the aim of developing a model organism is to provide a detailed genetic account of the standardized organism (ibid.). Surrounding infrastructure is equally essential. Infrastructure enables cross-disciplinary communication, exchanges of standardized materials and instruments, and specific social structures to retain the epistemic value of a model (2011, 317). Infrastructure is both material (stock/strain centers) and immaterial (databases to share results within and across research communities). What makes the MO different from other experimental organisms is the specific infrastructure and community ethos surrounding it. Community databases bring together results, people, and specimens and define what counts as knowledge of an organism.

Having introduced the concept of the MO in general, I will now discuss some specific issues with C. elegans. Sidney Brenner introduced C. elegans as a model organism. His goal was to expand molecular biology to questions concerning neuroscience and development. Ankeny argues that the story of the standardization of C. elegans is significantly different from those of other MOs. Brenner searched for a target organism to study the nervous system; a target organism which has a rapid life cycle, a simple reproductive cycle, a simple genome, and a small size (Ankeny, 2001, 475). Brenner's choice, finally, fell on C. elegans.

C. elegans is a tiny roundworm, and adults are about 1 millimeter long. One of the most significant advantages of studying C. elegans is that it is transparent. The behavior of C. elegans can be observed through a light microscope. Compound or confocal microscopes can provide single-cell resolution. Using fluorescent proteins such as GFP allows visualizing subcellular compartments. C. elegans has a very short generation time. Development from egg to egg-laying adult takes three days. There are two C. elegans sexes. More than 99% of all C. elegans are self-fertilizing hermaphrodites. Less than 1% of all naturally occurring C. elegans are males. C. elegans has an invariant number of somatic cells. When reaching adulthood, every hermaphrodite has the same number of cells (959) and also every male has the same number of cells (1031). For each of these cells, cell fate is predetermined, and researchers were able to track the fate of each cell from fertilization to adulthood. In laboratory settings, C. elegans live on bacteria in culture dishes and grow at temperatures between 15 and 25 degrees. C. elegans hatch from egg to adult through four different larval forms. If there is not enough food during larval development, larvae proceed into an alternative life cycle. They change into "Dauer" stage and can survive this way for several months. "Dauer" is a stage of developmental arrest and minimal metabolic functions. C. elegans can be frozen at this stage for many months, which makes experiments very convenient to plan.

Brenner established a genetic wild type and created biochemical, morphological, and motility mutants. By treating C. elegans with the mutagen EMS, he managed to create the first breedable phenotype, "dumpy," which was shorter than the wildtype. Later, Brenner set out to provide a mapping of all neurons together with synapses of the C. elegans hermaphrodite. In 1986, a 340-page pap er, later coined "the mind of the worm," was published, which reproduced wiring diagrams of all the 302 neurons of the hermaphrodite (Ankeny, 2000, 263). The map was constructed from 4 different worms, yet it was treated as a representation of "the" paradigm worm (2000, 265). After this mapping project, Brenner and collaborators defined a new goal: to provide a complete map of the C. elegans genome (De Chadarevian, 1998, 86). Brenner introduced a computer to the lab in the late 1960s, and digitalization and computerization accompanied C. elegans research right from the start. It was probably a symptom of this close relationship with "computerization" that Brenner differentiated between the "software of organisms" and their "hardware" (genes) (1998, 90).

"Subdivision" of areas of C. elegans research was characteristic of the beginnings of C. elegans (1998, 100). Partitioning not only facilitated research projects but was also a means of avoiding competition amongst C. elegans researchers. By and large, most post-docs who became group leaders emanated from Sidney Brenner's laboratory. C. elegans researchers shared a period of training in Brenner's lab. This contributed to a strong group ethos. There were also other unique assets of the C. elegans community: the Caenorhabditis Genetics Center at the University of Missouri that provided mutants to the worm community, a C. elegans journal, the *Worm Breeder's Gazette*, and a biannual C. elegans meeting (1998, 101). In sections to come, we will see that the group ethos described here was still thriving when RNAi was discovered.

To describe the specific resources and institutions that made the emergence of the C. elegans field unique, Ankeny and Leonelli introduce another concept, "repertoires." "Repertoires" are an alternative to the Kuhnian "paradigm." Ankeny and Leonelli argue that change is not primarily generated by theoretical developments, but by administrative, material, technological, and institutional innovations. It is thus possible to trace research practices without relying on paradigm shifts and revolutions. They demand more attention for the performative aspects of science:

> "research groups have variable degrees of continuity, longevity, and durability, depending on their relation to existing knowledge, materials, technologies, and institutions, as well as on the social dynamics within and beyond their boundaries" (Ankeny & Leonelli, 2016, 19).

For example, Ankeny and Leonelli oppose the Kuhnian notion that "these three classes of problems – determination of significant fact, matching of facts with theory, and articulation of theory – exhaust, I think, the literature of normal science, both empirical and theoretical." (Kuhn, 1962/2012, 34). In short, Ankeny and Leonelli question definitions of communities as shaped by theories. Paradigms, the authors argue, do not capture the dynamics and pace of scientific practice. Neither do they capture shifts in technology, methods, and epistemic activities.

Ankeny and Leonelli define repertoires as the "conditions, which include ways to wield and align specific skills and behaviors with appropriate methods, epistemic components, materials, resources, participants, and infrastructures" (Ankeny & Leonelli, 2016, 19). The adoption of one or more repertoires "has a strong influence on the identity, boundaries, practices, and outputs of research groups" (ibid.). Model organisms are a prime example of repertoires[2].

A repertoire indicates the ability to align its components effectively. The existence of coordinated efforts, together with "funding streams, technologies, and platforms for exchange and discussion," and a strategic vision are hallmarks of a repertoire (2016, 22).

One way to sustain a community is by development or adaptation of a repertoire. But a research community can also utilize more than one repertoire at the same time. Different communities can also use the same repertoire. One prerequisite of a successful repertoire is the alignment of its different components. Participants need to make assumptions about who the relevant actors are, what they know, and what they can do. They need both knowledge and access to these factors (2016, 25).

2.1.1.5 Further claims in the Fire & Mello paper

In previous subsections, I explained how F&M demonstrated specific dsRNA-based RNAi. In what follows, I will address further observations and speculations of the 1998 paper. F&M showed that if single-stranded RNA was purified to remove dsRNA contaminations, no interference was observable. In light of this view, they showed convincingly that the anti-sense RNA concept did not work. When F&M introduced sense and antisense strands at the same time or within one hour, RNA interference was observed. They explained this by arguing that dsRNA formed within cells. When they introduced antisense and then sense

[2] "Thousands of researchers from a variety of locations across the globe came to be involved in enacting and developing a repertoire that included the conceptualization of specific organisms as 'model systems,' with related theoretical assumptions and commitments around which research questions to pursue; strategies to acquire blue-skies funding support particularly from the US and UK governments, which enabled research to develop within relatively well-resourced conditions; specific norms and behaviors, and particularly an ethos of sharing data and techniques prior to publication, which were attractive to like-minded researchers and contributed to the continuity of the research efforts and their abilities to accrete over time; the standardization and centralization of the production, use, and dissemination of specimens in stock centers; and the establishment of databases to gather both published and unpublished data in a standardized manner" (2016, 21).

strands after one hour, no RNAi took place. From these observations, F&M deduced that single-stranded RNAs rapidly degrade within cells.

F&M inquired whether the structure or stereochemistry of dsRNA contributes to RNA interference. To address this question, they fused an unrelated double-strand to a single-stranded antisense sequence. No RNA silencing was observable. F&M concluded that the effect of silencing was not caused by double-stranded RNA structures, but only by the specific RNA sequence.

I now turn to one of the most intriguing findings in F&M's paper. The authors reported that progeny of dsRNA-treated worms showed even stronger silencing of the complementary mRNA than the parental generation. They tested the inherited effect of RNAi for four different genes. The mRNAs the authors targeted encoded genes whose functions were well known. In turn, phenotypes that were caused by the absence of these genes were also well described. It was also known that in order to manifest a phenotype, most of the worm's cells needed to be defective in expressing the respective protein. Because the phenotypes were strong in the offspring of treated worms, the authors conjectured that dsRNA might spread systemically throughout the worm tissues. The systemic spreading of dsRNA means that dsRNAs spread throughout all cells of the worm and execute silencing in all cells.

The system spreading of dsRNAs pointed to another essential requirement for the inheritance of RNAi. Fire, Mello, and colleagues injected dsRNA into the body cavity, which consists of somatic cells. Information is, however, only transmitted through the germline. Soma-to-germline transfer is one of the most critical problems for the inheritance of acquired traits (IAT). It is commonly believed that soma to germline transmission is not possible. The separation of soma and germline is coined the "Weismann Barrier." The "Weismann Barrier" is considered one of the rare law-like behaviors in biology. In my discussion on further advances on RNAi and RNAi inheritance, I will show how small RNAs were shown to violate this law-like behavior.

F&M's paper not only solved existing problems, but it also created new problems. The most tantalizing challenge that would keep researchers busy for the next two to three years was understanding the mechanism underlying RNAi. F&M did not provide definite answers to this question but hinted at a few possibilities. The authors discussed three scenarios: RNAi could happen before transcription, during transcription, or after transcription. Several experiments pointed to the scenario that RNAi is a post-transcriptional process. First, after the injection of dsRNA, a rapid decrease in mature mRNA of the targeted gene could be observed. This suggested that dsRNA targets mRNA. Second, many regulatory regions are part of the gene product but not part of the mature mRNAs. In addition, the gene sequence consists of introns and exons. Introns are regions that are transcribed but are cut out of the messenger RNA after transcription. This process is called "splicing." To test whether RNAi is a pre-/co- or posttranscriptional process, F&M synthesized dsRNA complementary to regulatory regions and introns. dsRNAs complementary to regulatory regions or exons did not elicit RNAi. This led F&M to conjecture that RNAi might be a posttranscriptional process.

The transgenerational effects and the systematic spreading of RNAi hinted at a significant problem. In the experiments, a limited number of dsRNAs was injected. Such amount would not be sufficient for dsRNAs to spread throughout all body parts and cause effects in the progeny. Considering the number of eggs each hermaphrodite lays, the amount of dsRNAs would be diluted tremendously. F&M conjectured that dsRNA is either involved in a catalytic mechanism that requires only very few molecules or is involved in an amplification mechanism.

In conclusion, F&M solved a long-standing technical problem in RNA biology. Also, RNAi was shown to be an effective tool for silencing genes. This suggested that RNAi could be used for studying gene function in general. Although the experiments were only performed in C. elegans, the results incited widespread excitement about the possibility that RNAi would also be effective in other organisms. If true, RNAi would be preferable to other tools for studying gene function. Such techniques involved time-consuming and laborious preparations. In contrast, dsRNA can be easily administrated by injection. The silencing effect was shown to spread systemically from the site of injection. Moreover, the findings strongly suggested that RNAs might have catalytic functions or are involved in amplification loops. Before further investigating the underlying mechanisms of RNAi, I will make a short detour and investigate whether F&M's discovery was "new."

2.1.2 Was RNAi theoretically new?

When a paradigmatic discovery has occurred, people often ask: “Was it *in the air*?”. RNAi was such a paradigmatic discovery. The question of “being in the air” rests on the assumption that shortly before a scientific breakthrough, many scientists are aware of the same irregularities. They conduct similar experiments to solve these problems. Many of these scientists are quite close to a breakthrough. After such breakthroughs, the community claims: “If X had not found it, Y would have.”

The discovery of RNAi was *in the air*. By 1998, several researchers could have tried to inject dsRNA. Besides such general issues concerning contingency or inevitability of the discovery, there are also more specific questions to be asked. One pressing question is the following: were there other researchers who proposed RNAi-related mechanisms prior to F&M? In this case, the answer is “yes.” The success story of RNAi-based techniques involves two stories of neglect. One concerns contemporaries of F&M, the other concerns researchers that proposed similar conjectures in earlier decades. I will focus on two examples. What they have in common is that the marginalized scientists studied the same types of organisms: plants.

2.1.2.1 “Plants got screwed!”

The first story of neglect involves plant researchers who experimented with RNA-based gene-silencing throughout the 1980s and 1990s. Scientists like David Baulcombe and Richard Jorgensen belong to this group. C. elegans was not the first organism in which RNA-induced silencing was studied. Previous to C. elegans, the phenomenology was well known in model organisms such as maize, arabidopsis, and petunia[3]. Many contributions that advanced the field were produced in plant labs, both before and after 1998.

In 2006, F&M won the Nobel Prize for the discovery of RNAi. Although it could have been shared among three scientists, no third laureate was selected. Many suspected that a researcher from the plant field would share the award with F&M. When this did not happen, many plant scientists felt neglected. Sentiments like “plants got screwed” (Couzin,

[3] I lend the term “phenomenology” from its usage in molecular biology, as an in vivo term. The term “phenomenology” denotes observed qualities of a phenomenon. The “phenomenology” of a phenomenon does not admit for hypotheses about possible mechanistic underpinnings.

2006, 34) were voiced loud and clear. A look at *Letters to the Editor* sections in the months after the Nobel Prize was awarded illustrates the controversy. In a letter entitled "RNAi Nobel ignores vital groundwork on plants," Bots, Maughan, and Nieuwland argue that by not nominating David Baulcombe, the committee "has undermined the values at the center of the prize and is sending a discouraging message, especially to young researchers" (Bots et al., 2006, 906). Leading plant epigeneticists Marjorie and Antonius Matzke criticized that animal researchers frequently downplay the contributions of plant science to the discovery of RNAi. In many reviews on the history of RNAi, results obtained in plants were characterized as vague and puzzling. Matzke and Matzke objected to this characterization and emphasized that plant scientists understood most of the underlying mechanisms (Matzke & Matzke, 2006). In response, Richard Jorgensen, whose aforementioned experiments on petunia leaf color were defining for RNA biology, reassured that the Nobel Prize was well earned by F&M because none of the plant discoveries "had the same catalytic impact on biology as did F&M's insight and elegant experimentation" (Jorgensen; et al., 2006, 1242).

For the time being, suffice it to say that in the 1980s and 1990s, plant scientists produced a significant part of the knowledge and understanding of RNA-based gene-silencing. This is both true for the description of the phenomenology and for gaining mechanistic insight. Nevertheless, until 1998, RNA-induced silencing was considered a "weird plant thing." Only after similar "weird" phenomena were found in C. elegans, RNA-based silencing interested a broader research community.

2.1.2.2 Ilan Sela – a mysterious anti-viral factor in virus-infected plants

The neglect of plant sciences did not start with David Baulcombe and Richard Jorgensen. In this section, I will investigate the case of Ilan Sela, who suggested that RNA might have other roles than that of the messenger back in the 1960s. Many other researchers could be included in this section. While I will not be able to provide a comprehensive account, I will try to show that ideas about specific capacities of RNAs were not necessarily "theoretically new" in 1998. I will keep this section as concise as possible and only focus on what Sela discovered about the capacities of RNA. I will discuss how he identified anti-viral effects of RNA and how this relates to RNAi. The take-home message of this section is that prior to the period, there were other researchers who asserted that RNA could have other functions. Despite their prescience, these researchers' conjectures were not picked up in a way that changed RNA research significantly at their time.[4]

Ilan Sela was a professor of agriculture at the Hebrew University of Jerusalem. He worked on virus infections of plants and later in his life on the virus infection of honey bees. In 1962, Sela reported the isolation of an anti-viral factor (AVF) in plants after exposing plants to viruses. The experiment was straightforward: first, he infected a plant with a plant virus. For example, Sela infected the tobacco plant *Nicotiana tabacum* with the Tabaco mosaic virus (TMV). Then he "macerated," that is, soaked, leaves from infected plants, thereby obtaining a sap. The obtained solution was then used to treat healthy leaves. When treated leaves were later infected with TMV, they showed resistance to the infection (Sela & Applebaum, 1962). Sela developed a method to separate and purify the AVF in order to increase the antiviral activity (Sela et al., 1964). He determined that both protein and nucleic acid were present in the final preparation and showed that the nucleic acid in the preparation was RNA and not DNA (1964).

As a next step, Sela asked whether RNA, protein, or both were the active component of the AVF. In 1965, Sela determined the active component. Sela extracted RNA from the AVF using a standard protocol: phenol extraction. He showed that the obtained RNA consistently caused antiviral activity. As the RNA extract might still contain some traces of protein, Sela treated the extract with trypsin, an enzyme that destroys proteins. The anti-viral activity of the extract persisted. As a control step, Sela treated the extract with ribonuclease, an enzyme that digests and destroys RNA. After the ribonuclease treatment, the anti-viral activity of the AVF was no longer detectable. Sela thus confirmed that RNA is the active component of the AVF.

Sela showed that the amount of total RNA extracted from virus-infected plants was 5 to 9 times higher than the amount of total RNA in control plants. This suggested that RNAs are

[4] Other examples that require separate discussion include Barbara McClintock or Roy Britten and Eric Davidson, among many others.

amplified after the infection. Sela also noticed that the active fraction of the AVF was remarkably stable when frozen, a property not natural for RNA (Sela et al., 1965).[5]

Sela provided two explanations for the effect of RNA on the viral infection. Either RNA "blocks some sites of action essential for the virus multiplication." Or RNA "acts as an antimetabolite to the biosynthesis of virus nucleic acids" (1965, 78). An "antimetabolite" is a molecule whose chemical structure is similar to a certain metabolite. Because of this similarity, the antimetabolite binds to the enzymes which usually bind the metabolite. Competing with the metabolite for binding sites, the antimetabolite decreases or blocks the activity of the metabolite (see Fig. 4).

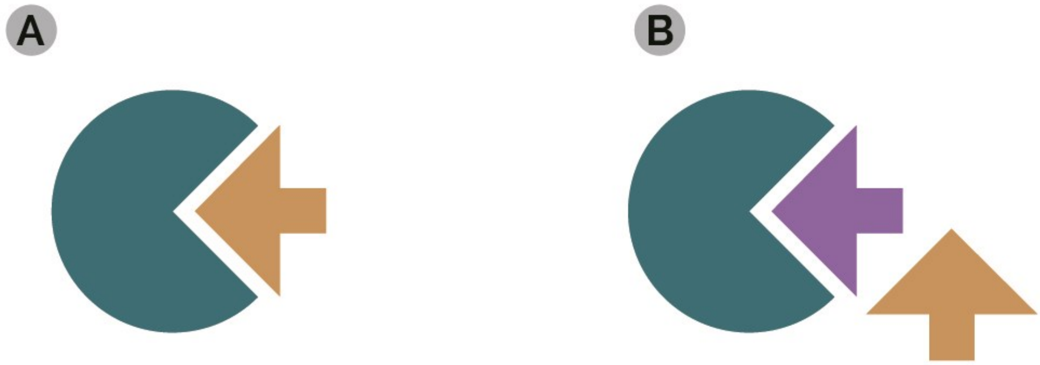

Figure 4 A metabolite binds to its substrate. B: The anti-metabolite outcompetes the metabolite and instead of the metabolite is bound to the substrate.

In conclusion, Sela suggested that RNA has capabilities other than that of the messenger. He proposed that RNA either acts as an inhibitor or a competitor. The phenomena Sela observed fit with cutting-edge textbook interpretations of plant defense against viruses. When a virus attacks, viral RNAs are released into the cell. Viral RNAs are recognized;

[5] One explanation in hindsight is that the active component was dsRNA as dsRNA is much more stable than single stranded RNA molecules, as their reactive groups are shielded from exposure. In single stranded RNA molecules, the reactive groups are exposed, and through their reactivity with other molecules they cause the instability of the RNA. However, other explanations are certainly also possible, such as protein-RNA complexes that are both more stable and could shield the protein from trypsin-induced degradation.

recognition initiates the anti-viral response. During this response, dsRNAs are formed, which are intermediates in targeting the virus for degradation. Taken together, it is quite conceivable that Sela's AVF was dsRNA complementary to viral RNA. If plants were administered virus-derived dsRNA, their viral defense would be primed. Because the plant would not have to synthesize dsRNA on its own, its viral defense would be accelerated. Despite his findings, Sela's work has only four citations after 1998 and is not mentioned in introductions to plant immune responses against viruses.

2.2 Small RNAs: tools, regulators, trajectories

In this section, I will sketch developments in small RNA biology in the last twenty years. Fire and Mello's paper heralded two separate paths of research: 1) developing RNAi as a tool to influence gene expression in model organisms; and 2) investigating the physiological relevance of RNAi and identifying effector proteins.

2.2.1 RNAi as a tool

The development of new tools is pivotal to the advancement of molecular biology. RNAi is one of the most illustrative examples of this process. In this section, I will review general aspects of tools in molecular biology and then specifically discuss RNAi as a tool. In so doing, I will challenge Isobel Ronai's interpretation (2017) of RNAi as a tool.

In many instances, molecular biological tools are adapted from naturally occurring effector mechanisms. Therefore, understanding the characteristics of natural systems and developing tools to modify them are inextricably linked. Isobel Ronai divides the "developmental trajectory of novel techniques in molecular biology" (2017, 1) into four characteristic phases:

1) Discovery of a phenomenon
2) Identification of the mechanism's trigger(s)
3) Application of the technique
4) Maturation of the technique (2017, 9)

Ronai defines the first stage as curiosity-driven research. At this stage, researchers encounter phenomena whose underlying mechanisms are not well known. Studying the phenomenology of an unusual phenomenon is followed by the identification of the triggers. Ronai characterizes a trigger as a "causally specific actual difference-maker." The identification of the trigger(s) is a crucial stage in the design of techniques. Once they are identified, researchers can set out to investigate underlying mechanisms in more detail. Once mechanisms are discovered, the next stage is the application of the technique. The last phase, maturation of the technique, involves improving the performance of the technique. In so doing, researchers broaden the scope of applicability and characterize the underlying mechanism (2017, 10).

Ronai argues that the development of novel techniques drives progress in molecular biology. Molecular biologists aim to "manipulate the components of the mechanism to observe the resulting effects" (2017, 1). Molecular techniques derived from natural systems are therefore not produced by rational design but evolved for a particular biological function. Even though techniques derived from natural systems might evolve for one particular function, they might also be cooptated to fulfill other functions in an organism.

Most techniques derived from natural systems are developed in model organisms. Researchers presuppose a fundamental unity of living systems and thus suspect techniques developed in one organism to work in other organisms as well (2017, 5). In principle, no comprehensive understanding of the mechanisms underlying the techniques is needed in order to implement them. Molecular biology techniques rely on two characteristics derived from natural systems: the effector's activity and the effector's biological specificity. Effectors can be proteins or RNAs. Biological specificity guarantees minimal off-target events and is most often obtained through nucleic acid sequence (2017, 6).

Ronai interprets the contribution of F&M's 1998 paper as "testing the specificity of RNA molecules to control the RNAi mechanism" (2017, 8). Ronai identifies Jorgensen's experiment on petunia leaves as the paper which discovered the RNAi phenomenon. As discussed in section 3.2, this paper showed that both sense and antisense strands cause the downregulation of an mRNA responsible for leaf color. F&M's paper fulfilled steps 2) and 3) of tool development. They identified the mechanism's specificity (dsRNA) and showed how to apply the technique. Ronai identifies two further papers that were important for the identification of specificity and effectors. In 1999, David Baulcombe and Andrew Hamilton identified small interfering RNAs as the processed product of dsRNA. These small RNAs were shown to interfere with the expression of the mRNA. In 2001, Scott Hammond identified a protein complex that he called the "RNA induced silencing complex" (RISC). Hammond and colleagues showed that RISC was necessary for RNAi. The last step is the maturation of the technique. Ronai argues that a paper by Sayda Elbashir, Tom Tuschl et al. heralded innovation because the authors demonstrated that RNAi works in mammalian and fly cells (2017, 11).

It is important to note that for RNAi – following Ronai's presentation – the four phases took place within eleven years. This is an extremely short period. Ronai also lists other techniques and measures the time it took from discovery to maturation in these cases. For CRISPR-Cas, the period is 26 years, and for induced Pluripotent Stemcells (iPS), the period is 45 years. Compared to other techniques, the development of RNAi was thus relatively fast.

I do, however, not agree with Ronai's analysis that Jorgensen's experiment on petunia leaves represents the discovery of the mechanism. As pointed out before, this experiment debunked the idea that anti-sense RNAs could modulate gene silencing. The paper was trailblazing and an essential link between early-stage anti-sense experiments and F&M's 1998 paper. Jorgensen and colleagues discovered the phenomenon of cosuppression but not RNAi. In its historical context, the discovery of cosuppression can be interpreted as the identification of an anomaly.

There are two possibilities: the very first anti-sense experiments in the 1980s could be considered the "discovery" of RNAi. These experiments identified the phenomenon – RNA based gene silencing – but provided little to no understanding of the mechanism. Also, they were not robust in that they were not reproducible. Alternatively, F&M's paper could be considered the discovery of a phenomenon. F&M discovered dsRNA-mediated RNAi. I prefer identifying the F&M paper as the one that discovered RNAi because silencing was not produced by accident. This characterization decreases the timespan from discovery to maturation to only 3 years. As Jorgensen put it, no discovery in the field had such an impact on molecular biology as had the 1998 paper by F&M. This, in turn, could be explained by the fact that it combined the discovery, the identification of the effectors, and the application of a technique on only four pages.

2.2.2 RNAi as a phenomenon

In this section, I will trace the history of small RNA biology throughout the last twenty years. At this point, it is essential to acknowledge the role of gene sequencing as a facilitator of early small RNA research. In an article written in 1999 for *Bioessays,* Ronald Plasterk, who collaborated with Mello after the discovery of RNAi until his assignment as Dutch minister of science in 2007, argues:

> "1998 is the year of the worm. The genome of the nematode C. elegans has been sequenced, and with that, the nematode is the first animal, also the first multicellular organism, to enter the age of postsequence genetics" (Plasterk, 1999, 105).

In the same article, Plasterk mentions the discovery of RNAi, but the focus lies on the sequencing of the C. elegans genome. Most of my interviewees agreed with Plasterk's analysis. They identify sequencing as the technique that helped small RNA biology get off the ground. Sequencing the genome of C. elegans facilitated two methodological approaches:

> 1) searching for regions in the genome that might encode small RNAs and thus elucidate their physiological function;
>
> 2) conducting targeted knock-out screens to search for possible effector molecules that are involved in RNAi.

Further developments in sequencing enabled researchers to sequence RNAs, key here was a method called "deep sequencing." In its early days, deep sequencing was restricted to very short reads. As small RNAs are very short, the length restrictions were no problem for small RNA biology. Thus, small RNA researchers could use this technique well before any other field could.

When discussing Fire and Mello's paper, I highlighted several discoveries and claims of the paper. For example, they observed the persistence of silencing effects for several generations. Although this finding might seem very intriguing to the reader, it did not receive as much attention as did other discoveries of the paper. With a few exceptions, the persistence of RNAi was not picked up until almost ten years after the discovery. There is thus a “lag phase” between the initial description of the persistence of RNAi and RNAi inheritance becoming a research interest in the mid-2000s. Most researchers whom I asked about this “lag phase” provided a similar explanation. They emphasized that although the persistence of RNAi was observed throughout these years, it was not the most important focus of their research.

Most investigators prioritized understanding the mechanism underlying RNAi in one generation. This aim is straightforward: before starting to investigate the scope of the phenomenon, effectors of the underlying mechanism needed to be discovered. In addition, the application of RNAi as a technique was of much broader interest. RNAi as a technique could be used to address almost any molecular biological question. The question about a mechanistic basis of RNAi remained restricted to one research field. It is still surprising that there was not more interest in the transgenerational effects of RNAi, given the general interest in epigenetics at the time.

2.2.2.1 The rise and demise of epigenetics

In the last section, I expressed my surprise that the inheritance of RNAi did not provoke immediate excitement despite a general interest in epigenetics.[6] “Epigenetics” was a buzzword in the 90s and 2000s. “Epigenetics” means

1) gene regulation through modification of DNA and histones and
2) persistence of such modifications throughout cell divisions or generations.

The meaning of “modifications on DNA and histones” requires further explanation. The human genome consists of about 20,000 genes. Many other organisms have much larger genomes. Most of these genes are lineage-specific: they are necessary for a specific task which is executed by a specific subset of cells. Take neurons and immune cells as an example. Specific genes are necessary for immune cells to fulfill a specific task but not for neurons of the brain. Different genes are needed for fighting bacteria than for storing

[6] The story is, yet again, different in the plant field. In the plant field, cosuppression was known to be related to “classical” epigenetic mechanisms such as DNA methylation throughout the 90s.

memories. It would be a waste of resources to express genes that are not needed. For example, a neuron does not need to express genes for fighting bacterial infections. In order to avoid this waste of resources, specific genes are turned "on," whereas other genes are turned "off." In what follows, I will explain how stretches of DNA are turned "on" and "off."

Each eukaryotic cell contains a nucleus, in which DNA is stored. An average animal-cell nucleus is about six µm in diameter. For comparison, a human hair has a diameter of at least ten times the size. One DNA molecule is three meters long, thus presenting a storage problem to each nucleus. To fit three meters of DNA into a nucleus of six µm diameter nucleus, DNA is wrapped around histones. Histones are proteins that scaffold DNA very tightly. Histones and DNA combined are called "chromatin" (Fig. 5).

If a stretch of DNA is wrapped very tightly around histones, it is not accessible. When a gene needs to be turned on, the DNA sequence needs to be unwrapped and the folding around the histones needs to be loosened. The tightening or loosening of chromatin is achieved through covalent modifications on histones or DNA. A covalent modification is a small chemical compound, such as acetyl or methyl groups, which are attached to DNA or histones. Covalent modifications change the chemical properties of DNA or histones. Through a change in chemical properties, DNA and histones associate tighter or looser. For example, acetylation is a covalent modification that is usually associated with turning genes "on." An acetyl-group is acidic and negatively charged. DNA also has a negative charge. Two negative charges repel each other. If a histone is acetylated, then the DNA that is wrapped around it is repelled by the negative charge of the acetyl group. As a result, DNA is wrapped looser around the histones and becomes more accessible for transcription. As a result, the specific stretch of DNA is turned "on" (Fig. 5).

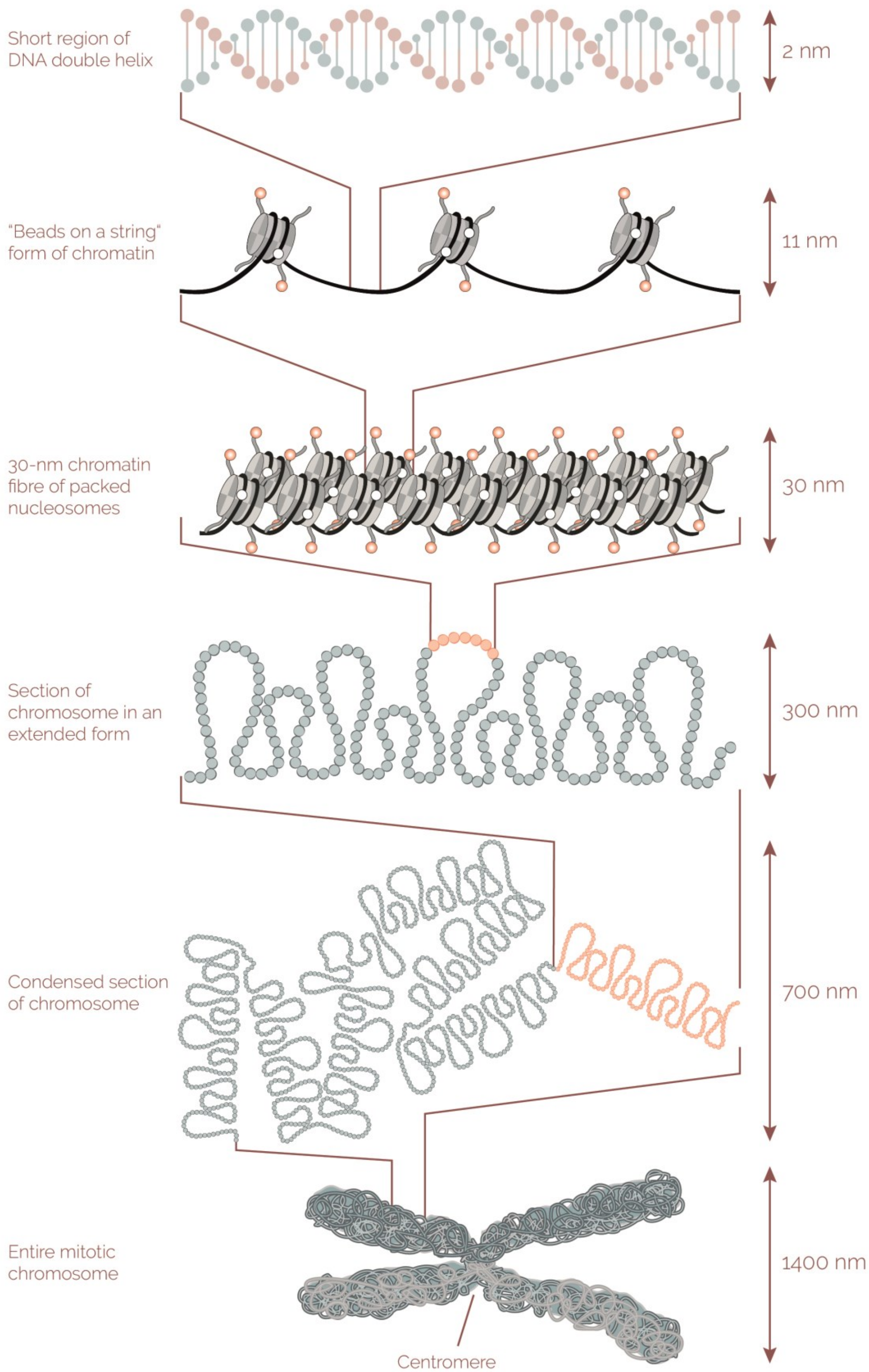

Figure 5 Scheme of the organizational layers of the chromosome.

Inspired by the phenomenology of these processes, the idea of the "histone code" used to play a significant role. This idea was especially popular in the early 2000s. After the human genome was sequenced, it became clear that the concept of the "genetic code" could not explain the differences between, e.g., humans and chimpanzees. Therefore, the notion of the "histone code" was introduced (Jenuwein & Allis, 2001). To account for the differences, it was therefore conjectured that not the material basis – genes – but the regulation of this material basis is what makes the difference. Information is not only encoded in DNA but also in covalent modifications. In analogy to the "genetic code," the term "histone code," referring to the sum of all covalent modifications on a specific stretch of chromatin, was introduced. These modifications, in turn, were shown to be transmitted through cell divisions. Scientists hoped that if they determined the function of each chromatin modification, they would know when and how each gene will be expressed in each cell lineage. Metaphors of epigenome "readers" and "writers," molecules that could encode or decode the histone code, became very popular (Baedke, 2018; Deichmann, 2015; Oftedal, 2019; Stegmann, 2016).

Histone "readers" and "writers" were considered to be proteins, falling under the broader category of "chromatin modifiers." Researchers remained focused on proteins as the effector molecules of epigenetics, although research in plants suggested the involvement of RNAs before 1998 (Matzke & Matzke, 1995; Wassenegger et al., 1994). Keeping the research focus on proteins was "tame" in one sense but highly problematic in another. It was tame because protein-based mechanisms did not pose any significant challenge to Neo-Darwinism. If all proteins responsible for epigenetic mechanisms are encoded in DNA, then these effectors are all subject to Darwinian evolution. Darwinian evolution, in turn, is believed to operate exclusively through DNA mutation. Admitting the transmission of the histone code throughout generations did not challenge orthodoxy in any significant way. Even the most outspoken defenders of Neo-Darwinism, such as the evolutionary biologist David Haig, contributed to the field of protein-based epigenetics (Haig, 2000).

But there is also something deeply problematic about proteins as effectors of epigenetics: they lack specificity. Histone modifications do not encode specific information. This shortcoming is often highlighted by researchers who built their careers around describing "transcription factors," proteins that are specificity determinants. These researchers call the "histone craze" a fad. Histone modifications cannot be the causes of epigenetic inheritance; at best, they are unspecific cogs (Henikoff & Shilatifard, 2011; Ptashne, 2013).

The field of small RNA-based inheritance is still small. RNAi is not the main focus of epigenetics. In the 2007 version of the textbook "epigenetics," one of twenty-four chapters discusses RNAi, but only in the context of chromatin modification. In the second edition of 2015, there is still only one chapter on RNAi, but RNAi is mentioned in many other chapters (Allis et al., 2007/2015). What explains this transition? In what follows, I will explore how small RNAs became recognized as essential players in epigenetics.

Before introducing the next section, I want to introduce a differentiation. The term "epigenetic" can be misleading. Although many researchers try to define what "epigenetic" actually means, there is no clear consensus. Many misconceptions make the use of the term problematic. When talking about the inheritance of small RNAs, I will thus refrain from the term "epigenetic inheritance" and use the expression "heritable responses," as suggested by Rechavi (Rechavi & Lev, 2017). I will use the term "epigenetic" to denote the canonical effectors of epigenetic inheritance – histone modifications and DNA methylation. For example, I will refer to modifications of histones and DNA as "epigenetic marks."

2.2.2.2 Early RNAi research: genetics vs. biochemistry

In the first years after the discovery of RNAi, there were two primary goals: 1) identifying the mechanism underlying RNAi; 2) showing that RNAi is a phenomenon ubiquitous in many model organisms. Two questions were central in the search for the RNAi mechanism: how are dsRNAs cleaved? How can mRNAs be targeted for silencing?

Many of the researchers I interviewed emphasized a dichotomy of two distinct research approaches to answer these two questions. When asked who made the discoveries which were crucial in establishing small RNA biology as a field, common answers were "The geneticists!", or "The biochemists!" I did not expect this differentiation and I had not considered it before I conducted my interviews. It demarcates experimental strategies to illuminate the processes underlying molecular biological phenomena.[7] Interviewees referred to “the geneticists” to describe researchers that conduced knock-out screens. They knocked out different genes of the C. elegans genome and observed how the capacity of C. elegans to exert RNAi would change. Interviewees referred to “the biochemists” to describe researchers who investigated the biochemical properties of the effectors (both protein and RNA) of RNAi. For example, they extracted proteins and performed cleavage assays in the test-tube to determine the biochemical prerequisites for RNAi. One key biochemical approach was to precipitate protein complexes involved in RNAi and to investigate what the components of these protein complexes were. As a next step, complexes or components were incubated in test tubes together with dsRNAs to see whether dsRNAs would be cleaved.

My account of early developments in small RNA biology commences with the observations of “the geneticists.” One of the very first advances in studying RNAi mechanisms was the identification of RNAi deficient mutants. Through a random mutagenizing screen, genes whose knock-out made worms “resistant” to RNAi were identified. When these genes were knocked out, the injection of dsRNA did not cause RNAi. What came as a surprise was that RNAi-resistant worms showed the deregulation of transposons. Scientists conjectured that proteins involved in RNAi were also responsible for transposon regulation.

Transposons are “selfish” elements encoded in DNA, which are mostly of viral – parasitic – origin. Transposons can change their location on the chromosomes through “cut-and-paste” or “copy-and-paste” mechanisms. Either a transposon gets excised from DNA and locates at another position. Or a transposon gets transcribed, the resulting RNA is then “reverse-transcribed” (from RNA into DNA) and reinserted into DNA. Transposons, therefore, have RNA phases. Keeping the number of transpositions (that is, cut-and-paste or copy-and-paste events) limited is pivotal for securing the stability of the genome. If transposons could transcribe and reinsert in an uncontrolled manner, they would disrupt coding sequences, and information would be lost. Maintaining the integrity of coding sequences is especially crucial in the germline. If a transposition happens in the germline, information is lost for the

[7] The differentiation of course not only rests on differences in experimental strategy. Rather, “biochemistry” and “genetics” look back on more than a hundred years of disciplinary history, demarcation strategies, and struggles for primacy, for example culminating in the “exclusion” of biochemistry in the modern synthesis.

next generation. If there are too many transpositions, the germline becomes "mortal," organisms with too many transpositions become infertile. As the proteins involved in RNAi were shown to play a role in limiting transpositions, the first physiological function of RNAi was suggested: transposon control. This realization also led to another hypothesis. As transposon control has to happen in the nucleus, small RNAs are not only in the cytoplasm but can also enter the nucleus.

Another early and groundbreaking finding was made in plant sciences. Baulcombe and Hamilton detected that antisense RNA was responsible for gene silencing in 1999. They also detected antisense RNAs in response to virus infections. Thereby they solved the problem of how mRNAs can be targeted. They conjectured that the production of RNA antisense to the messenger RNA is the necessary step between the injection of dsRNA and the silencing of an mRNA. Baulcombe and Hamilton reported unexpectedly high numbers of antisense-RNAs. They hypothesized that an amplification mechanism after the injection of the dsRNA is required (Hamilton & Baulcombe, 1999).

Another early discovery was the linkage of small RNAs to the family of lin- and let-RNAs. In the 1980s, the let-7 (letal-7) gene was shown to regulate the expression of the lin-21 gene. The regulation of lin-21 is pivotal for development. Down-regulation of lin-21 ensures that roundworm larvae develop into adults. In 2000, it was shown that the let-7 sequence is complementary to lin-21. Researchers suspected that lin-21 and let-7 form a dsRNA. Additionally, it was shown (Reinhart et al., 2000) that lin/let family genes are conserved in most living organisms. This finding linked RNAi phenomena to an area of interest of a broad range of biologists.

In conclusion, three physiological roles of small RNAs were conceivable by 2000: control of transposons, defense against viruses, and regulation of development. Later it was shown that different types of small RNAs execute these three tasks. miRNAs are regulators of development and other physiological processes. piRNAs are regulators of transposons, especially in the germline. siRNAs are pivotal for the immune defense of invertebrates and vertebrates. This classification needs to be taken with a pinch of salt. There is an overlap between the functions of miRNAs, piRNAs, and siRNAs. In different organisms, different classes are pivotal. For example, miRNAs play a much more significant role in humans than they do in worms. siRNA-mediated virus defense, however, is pivotal in invertebrates, whereas probably dispensable in mammals, as they also have an adaptive immune system.

At this point, it may be useful to illustrate the basic mechanism of action of small RNAs. Small RNAs function similarly to a keyword search in an electronic book. Imagine you want to know where in this book a specific person is mentioned. In that case, you would type the name of the person in the search box, and the result would be a list of all the hits within the electronic resource.

The number of mRNAs within a cell is larger than the number of words in any book imaginable. This creates a similar problem in terms of information management: the location of specific mRNAs needs to be known. Small RNAs are an implementation of a keyword search. Small RNAs are of 22 nucleotides length; they contain a 22-letter keyword. This keyword enables them to search the complete pool of mRNAs in the cell for the presence of this keyword. When they find it, they bind it. Binding can have different outcomes, for instance: finding and eliminating transposons, finding and fine-tuning developmental mRNAs, and finding and eliminating RNAs of viral origin.

So far, I have discussed the findings of the "geneticists." One of the most important early discoveries by the "biochemists" is the discovery of RISC. RISC is the "RNA induced silencing complex," which mediates the degradation of target mRNAs. After a successful keyword search, RISC acts on the information provided by the search. Scott Hammond discovered the activity of RISC while he was a post-doc in Greg Hannon's lab in 2000. RISC is a multi-protein complex and contains the nuclease Argonaute. Hammond and colleagues conjectured that RISC is guided by anti-sense RNAs to degrade a specific mRNA, but the origin of these guide RNAs was unknown (Hammond et al., 2000). One year later, the enzyme that produces guide RNAs was discovered by Emily Bernstein, also a grad-student in Greg Hannon's lab. The endonuclease Dicer was identified (Bernstein et al., 2001). With the identification of Argonaute and Dicer, the process of RNAi could be divided into two consecutive steps: first, dsRNA gets processed into guide RNAs by Dicer. Such guide RNAs are of approximately 22 nucleotides (nt) length. Second, these guide RNAs associate with Argonaute (RISC) and guide it to the complementary mRNAs. Association of RISC with mRNAs leads to silencing.

By 2001 it was possible to explain the processes that underlie the discoveries of Fire and Mello's paper. In their initial experiment, they injected dsRNA into the worm, which was then cleaved by Dicer. Small 22nt fractions of the cleavage products then associated with RISC to target an mRNA for degradation (Fig. 6).

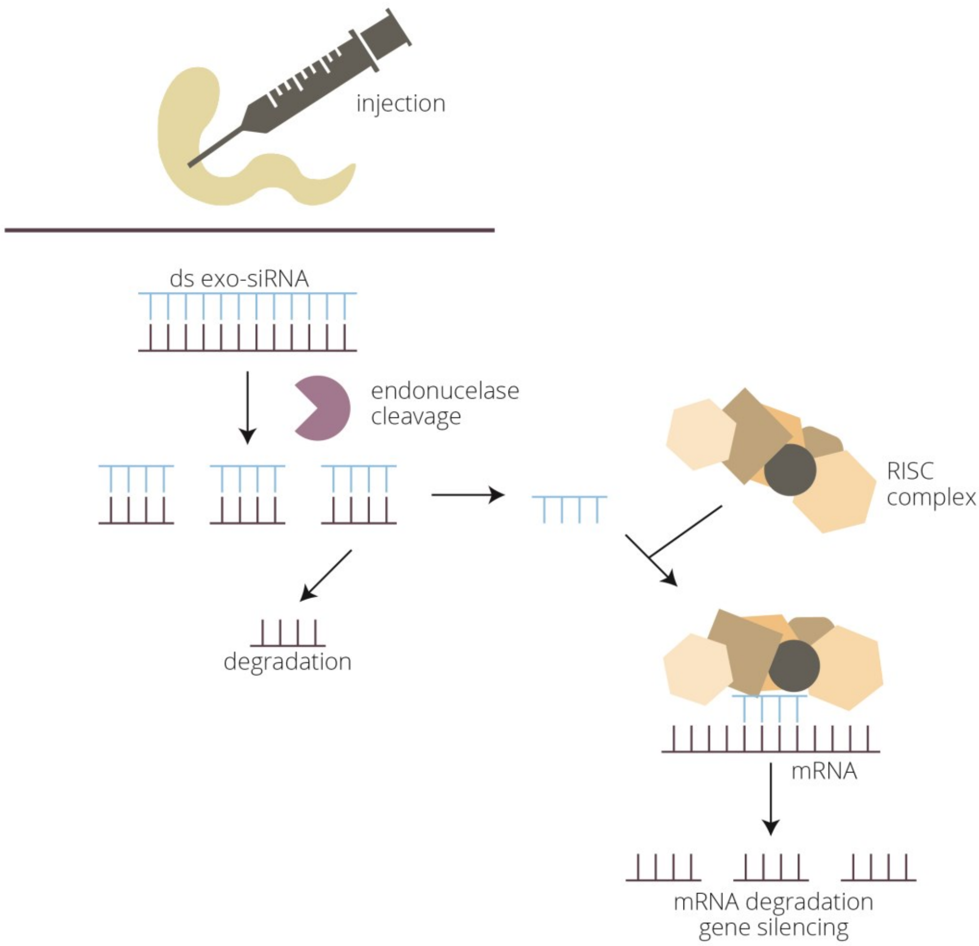

Figure 6 The injected doublestranded exo-siRNA gets cleaved by an endonuclease. One cleaved single stranded fragment associates with the RISC complex. The fragment guides the RISC complex to a complimentary mRNA. RISC cleaves the targeted mRNA.

It also became possible to infer the basic steps of endogenous small RNA induced gene silencing. Endogenous small RNAs are encoded in DNA. Exogenous small RNAs derive from DNA or RNA sequences entering the organism from the outside. For example, injected dsRNA or RNAs of viral origin are exogenous RNAs. In the case of endogenous small RNAs, RNAs are transcribed, edited, and processed by Dicer, and then they associate with the RISC complex. As a next step, these endogenous small RNAs guide RISC to complementary messenger RNAs. In turn, these messenger RNAs are silenced (Fig. 7).

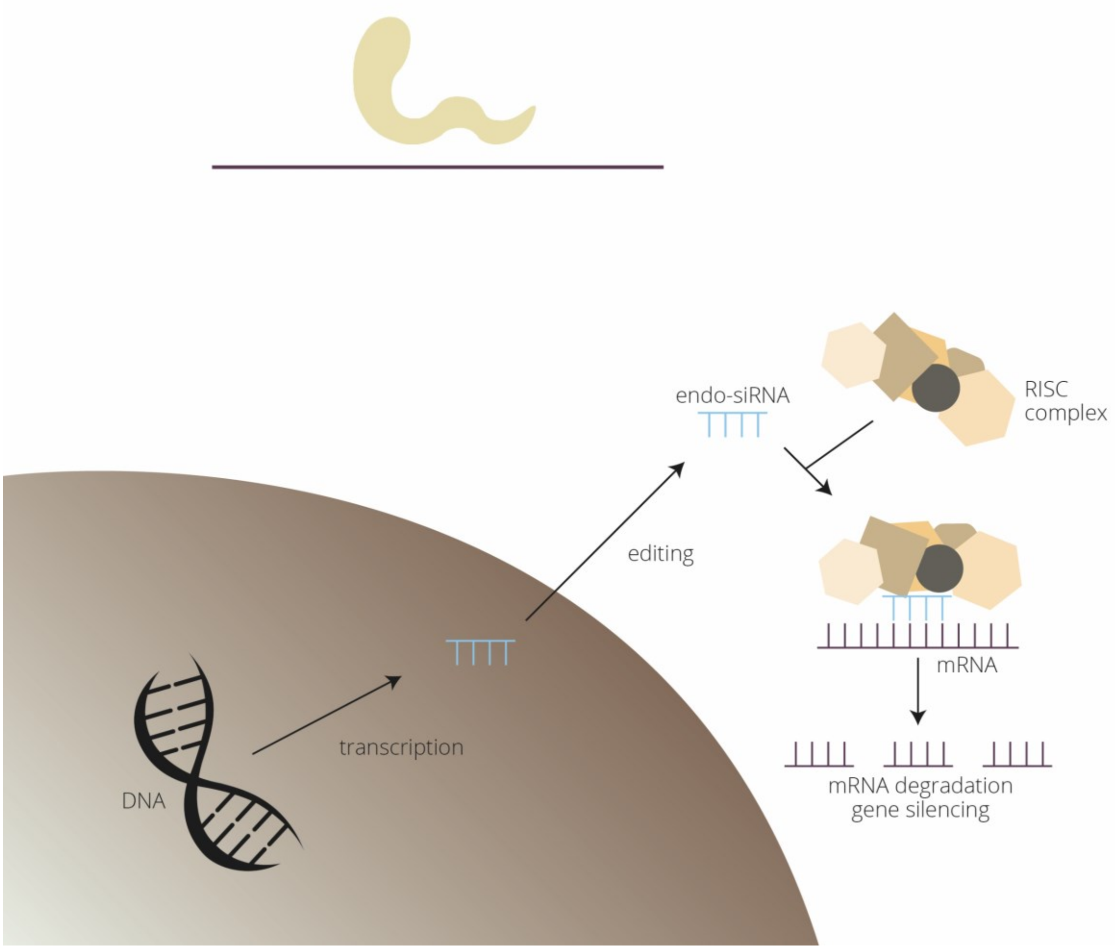

Figure 7 Endogenous small RNAs are transcribed from DNA and processed. The mature endo-siRNA associates with the RISC complex and guides the RISC complex to a complementary mRNA. The RISC complex cleaves the targeted mRNA.

The Dicer/RISC model also suggested how amplification of small RNAs could work: a dsRNA has an approximate length of about 200nt, Dicer cleaves the dsRNA into several 22nt pieces. Thus, a rather high number of small RNAs can be obtained from a relatively low number of dsRNA precursors. This process of amplification occurs in almost all organisms that are known to have RNAi function. There is, however, another aspect to amplification which is unique to organisms that have specific polymerases: RNA-dependent RNA polymerases (RdRPs).

F&M reported in their 1998 paper that RNAi could systemically spread, that is, spread from the locus of injection to distant cells. By then, it was also clear that systemic spreading requires an amplificatory step. Dicer-cleavage based amplification explained some aspects of amplification, yet it remained a mystery how the amounts of small RNAs necessary for systemic spreading could be produced. RNAi researchers proposed, therefore, a second amplification. This amplification mechanism involves RNA-dependent RNA polymerases (RdRP). RdRPs use single-stranded RNAs as a template and synthesize the complementary strand. An mRNA can have a length of several 1000nt. A single small RNA only binds a small fraction of it. RdRPs can bind to such mRNA-small RNA complexes and use the small RNA as a primer. They start to synthesize a full-length complementary strand. This gives rise to a dsRNA. This dsRNA is again cleaved by Dicer. Cleavage by Dicer gives rise to more small RNAs. These small RNAs again target complementary mRNAs (Lipardi et al., 2001; Sijen et al., 2001). In this way, RdRPs mediate amplificatory loops.

Before outlining the role of small RNAs in inheritance, I would like to sum up the core concepts of RNAi: 1) dsRNAs elicit RNAi; 2) dsRNAs are of exogenous or endogenous origin. Virus-derived RNAs and injected dsRNAs are exogenous. siRNAs, miRNAs and piRNAs are endogenous; 3) the endonuclease Dicer cleaves dsRNAs into small 22nt pieces; 4) small RNAs associate with the RISC complex. Small RNAs guide the RISC complex to the complementary RNA. 5) Amplification mechanisms exist in all organisms. In some organisms, RdRPs synthesize antisense RNAs that form a dsRNA with the target mRNAs to reiterate the RNAi cycle. This amplification is extremely efficient. 6) Different small RNAs have several different functions. 7) Small RNA systems are conserved in almost all phyla.

2.2.2.3 The inheritance of small RNAs – a twist for epigenetic inheritance

I will start my discussion of small RNA inheritance with a discovery made in plants called "co-transcriptional gene silencing." In contrast to post-transcriptional gene silencing (PTGS), which takes place in the cytoplasm, co-transcriptional gene silencing happens in the nucleus. While mRNAs are transcribed, they are silenced at the same time. Co-transcriptional gene silencing involves the establishment of histone marks.

In the early 2000s, small RNAs were shown to be involved in co-transcriptional gene silencing in several species (Dernburg et al., 2000; Mette et al., 2000; Pal-Bhadra et al., 2002; Sijen et al., 2001). Deleting Argonautes, Dicers, and RdRPs in yeast leads to aberrant histone marks (Volpe et al., 2002). If specific stretches of DNA carry the “wrong” histone marks, genes that are supposed to be turned “on” are turned “off” and vice versa. In other words, aberrant histone marks cause wrong gene expression. A link was thus established between small RNAs and epigenetic gene regulation (Fig. 8).

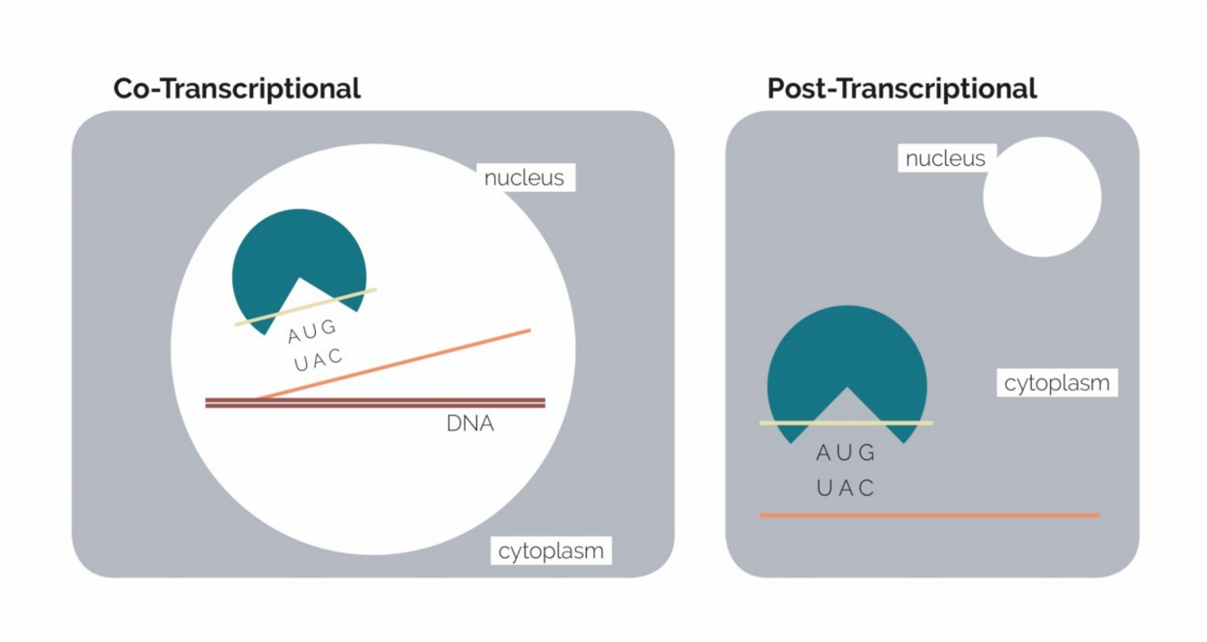

Figure 8 During co-transcriptional silencing, a small RNA binds to a nascent mRNA in the nucleus. The mRNA will not be translated. During post-transcriptional silencing a small RNA binds to a mature mRNA in the cytoplasm. The mRNA will not be translated.

A similar phenomenon was also reported in Drosophila melanogaster, the fruit fly. Knock-out of one specific Argonaute affected both transcriptional and posttranscriptional gene silencing (Pal-Bhadra et al., 2002). At the same time, Argonautes were shown to be involved in genome rearrangement in the ciliate Tetrahymena (Mochizuki et al., 2002). In short, small RNAs were associated with gene silencing on the DNA/histone level in many organisms. This indicated that not only piRNAs, who are primarily found in the germline, can enter the nucleus. Other regulatory functions beyond transposon control involve small RNAs in the nucleus. Researchers suspected that the presence of small RNAs in the nucleus was not only restricted to the germline but also in somatic cells.

In 2004, researchers proposed several possible models for the role of RNAi in the nucleus. The core theme of these mechanisms was the specificity of small RNAs. Small RNAs can not only bind to "mature" mRNAs in the cytoplasm. They can also bind to "nascent" mRNAs in the nucleus. Nascent mRNAs are mRNAs that are in the stage of being transcribed; they are still in association with the stretch of DNA that encodes them. Silencing in the nucleus works differently than PTGS but also involves Argonautes. In analogy to the RISC effector complex, its nuclear version was identified in yeast and coined the RNA induced transcriptional silencing (RITS) complex (Bühler et al., 2006). RITS consists of Argonautes and RdRPs but also comprises well-known chromatin modifiers such as "readers" and "writers."

In nuclear RNAi, small RNAs perform a "keyword search" and guide RITS to the query. They find and bind complementary nascent mRNA. Through the physical association of nascent mRNAs with RITS, a binding platform for many other proteins develops. Proteins involved in epigenetic regulation bind to RITS. As a result, stretches of chromatin surrounding the target region get modified. In most cases, RITS leads to the silencing of targeted regions. Genes are turned "off." RITS-directed gene silencing was implemented as a technique soon after its discovery (2006). Marc Bühler and colleagues showed that gene silencing could be induced by artificially tethering RITS to a specific region. It is important to note that the term "RITS" is specific to yeast. Similar pathways exist in plants and worms. To avoid confusion, I will use the term "nuclear RNAi pathways" when I refer to processes in the worm that are similar to the effector functions of the RITS complex.

As discussed above, during gene silencing in the nucleus, small RNAs look for short keywords. They find these keywords on mRNAs that are in the process of being transcribed. The metaphor of the "keyword search" can thus be taken further. While small RNAs "only" conduct keyword searches in the cytoplasm, they act as "librarians" in the nucleus. For the sake of the analogy, each mRNA transcribed from a specific locus represents a copy of a book. Through their complementarity, their keyword, small RNAs can find each book. But that is not all. Co-transcriptional gene silencing is associated with chromatin modification, that is, turning genes "on" or "off." Assuming that small RNAs, the librarians, take an essential part in this, we can understand turning a specific locus "on," as increasing the stock of copies. On the other hand, turning a specific locus "off" means taking it out of the shelf, either to send it for service or to take it out of stock for good. In the following, I will explain how the “librarians” increase the stock of copies or take them out of stock.

In 2006, Minnoo Rassoulzadegan and colleagues published a groundbreaking paper on small RNA-induced co-transcriptional gene silencing. They showed that the injection of small RNAs leads to changes in chromatin structure. This effect was heritable for several generations. They could trace this effect through a change in the skin color of the mice. The silencing was thus not only inherited through the germline but also affected somatic cells of the next generation. Similar phenomena were known in plants and were coined “paramutation.” It was nevertheless surprising that a similar mechanism operates in mammals (2006).

Amongst other papers, Rassoulzadegan’s work initiated a wave of research on epigenetic effects of small RNAs. Another paper in 2006 established this phenomenon for C. elegans. Nadine Vastenhouw of Ronald Plasterk’s lab reported that RNAi induced silencing of a transgene could be inherited in C. elegans for 80 generations (Vastenhouw et al., 2006). Alla Grishok, a Ph.D. student of Craig Mello, had published a paper in 2000 where she identified genes required for the persistence of RNAi. But it was the Vastenhouw paper that catalyzed a wave of research into these phenomena. It shifted how the contribution of small RNAs to transgenerational gene silencing was conceived. In the early 2000s, researchers were interested in how small RNAs affect the histone code or DNA methylation. It was generally thought that information was then transmitted via the histone code. Starting in 2006, it became tempting to ask whether RNAs and not histones could carry the information into subsequent generations.

Nowadays, numerous studies indicate that the answer might be affirmative. Over subsequent years, consensus formed that transgenerational effects of small RNAs usually last for 3 to 5 generations in C. elegans. Research demonstrated that the inheritance of small RNAs not only occurs but is sometimes necessary. For example, the inheritance of maternal piRNAs in Drosophila is pivotal. If piRNAs are not inherited, the offspring becomes sterile (Brennecke et al., 2008). In analogy, the disruption of small RNA inheritance in C. elegans was shown to result in mortal-germline phenotypes (mrt) (Greer et al., 2014). Mrt-phenotypes are viable but progressively lose fertility over generations. In other words, their germline is mortal in the long run, as it cannot be maintained.

One essential requirement for the inheritance of small RNAs is the ability to enter the germline. Entering the germline is believed to be the only way of contributing to gene regulation in the next generation. As RNAi-induced gene-silencing can spread systemically, this is possible in principle. I will hint at ways how RNAs could enter the germline later in this chapter. In most cases, the effectors of RNAi inheritance interact with well-known epigenetic mechanisms. But some researchers speculate that well-known epigenetic processes are only epiphenomena. They believe that nuclear small RNAs establish histone-modifications in each generation. This sentiment indicates a significant shift in thinking about causation in epigenetic inheritance.

2.2.2.4 Physiological implications of RNAi inheritance

In the previous sections, I explained the processes involved in RNAi inheritance. In the next paragraphs, I will discuss the physiological relevance of RNAi inheritance. Most researchers hypothesize that RNAi evolved as an immune response against intruding viruses (Obbard et al., 2008). Most viruses encode their information in RNA or need RNA stages in their life cycle. I outlined plant-defense against viruses in the section on Ilan Sela. The immune response of invertebrates works similarly. When the recognition of viral RNA occurs in an organism, viral RNA gets cleaved into small pieces by Dicer. RdRPs amplify virus-derived small RNAs (viRNAs). If the organism does not express RdRPs, the anti-viral response is sufficiently effective by Dicer/RISC-dependent amplification. The resulting viRNAs then associate with the RISC complex and target viral RNAs for degradation.

RNAi-based defense against viruses hints at a crucial point of RNAi in general. RNAi response to exterior stimuli (such as a virus) is adaptive. Assume that a virus attacks C. elegans. If RNAi against it is successful, viRNAs remain in the cells of the worm. If the same species of virus attacks again, the virus will be eliminated rapidly, as these viRNAs can perform a keyword search to find the virus RNA. C. elegans is thus “immunized” against this virus (Rechavi et al., 2011). RNAi is also responsive to other environmental stimuli such as adverse temperatures or starvation (Houri-Ze'evi et al., 2016; Ni et al., 2016). In such scenarios, small RNAs are synthesized in response to an exterior stimulus. These small RNAs help to adapt to the new situation. For example, upon starvation, small RNAs are expressed that search for keywords present in mRNAs that regulate the synthesis and transport of yolk proteins. Yolk is an essential nutrient for C. elegans eggs and larvae (Houri-Ze'evi et al., 2016).

If there is a responsive memory of adverse environmental conditions in one generation, it is intriguing to ask whether this information can be inherited. Through the transmission of small RNAs synthesized in response to a particular stimulus, information on how to address a specific situation could thereby be transmitted. This would be an instance of the Inheritance of Acquired Traits (IAT) because the resistance to a virus or another adverse stimulus is acquired through a small RNA response.

In C. elegans, yeast, ciliates, plants, and possibly flies, evidence points into the direction that small RNAs might transmit adaptive information to subsequent generations. It is thus intriguing to ask whether such phenomena also happen in humans. Small RNA inheritance in mammals is not the topic of my dissertation. There is extensive literature on epigenetic inheritance in mammals, although there is less evidence. Although epigenetic inheritance might take place in humans, I want to highlight the limitations of small RNA inheritance in mammals. Mammals live much longer than worms or yeast. If one generation of worms experiences a cold shock, then the next generation might very well be confronted with this cold shock as well. After all, worms have a generation time of three days. Mammalian gametes are developed during embryogenesis (women) or in puberty (men). It is far from clear whether environmental conditions are stable for ten years or longer. There is yet no study that shows a positive effect of epigenetic inheritance in mammals. In addition to the question of whether IAT takes place in humans, the even more pressing question is whether IAT can be adaptive, given the long generation times.

2.2.2.5 Principles of small RNA inheritance – a hypothesis

In the remainder of this section, I will discuss some concepts of RNA inheritance in more detail. I will provide facts about small RNA inheritance with an interpretation that is a further development of thoughts which I have published elsewhere (Veigl, 2017).

Amplification

In the previous section, I presented how small RNAs are synthesized in response to environmental stimuli. Besides their role in "post-transcriptional gene silencing" (PTGS) in the cytoplasm, small RNAs get carried into the nucleus and execute nuclear RNAi. Nuclear RNAi is responsible for the installation of epigenetic marks and leads to the amplification of small RNAs complementary to the nascent mRNA silenced. RdRPs are the effectors of nuclear RNAi. Therefore, nuclear RNAi is a self-amplificatory system. Self-amplification facilitates the transmission of small RNAs throughout generations without dilution in each generation. By entering the nucleus in the next generation and establishing the same self-amplificatory cycle, small RNAs can establish histone marks, and in turn small RNAs are amplified.

RNA-dependent RNA Polymerases (RdRPs) are critical elements in RNAi inheritance pathways, as they help overcome dilution effects. But, are all small RNAs inherited? For instance, there is little evidence of the inheritance of miRNAs in C. elegans. Heterogenous affinities of small RNAs to bind RdRPs explain why not all small RNAs are inherited. Depending on their "keyword," small RNAs have slightly different biochemical properties, and therefore better or worse affinities to bind RdRPs. Small RNAs that can associate with RdRPs are more likely to produce transgenerational RNAi effects than small RNAs that cannot associate with RdRPs (Rechavi & Lev, 2017). Those small RNAs that have higher affinities for associating with RdRPs are more likely to produce transgenerational RNAi effects than small RNAs that have lower affinities.

RdRP-mediated amplification of small RNAs requires a specific nomenclature for small RNAs. "Primary" small RNAs are small RNAs that are the "first" effectors of RNAi responses. RdRPs amplify "primary" small RNAs and thereby produce "secondary" small RNAs. In the germline, RdRPs can amplify "secondary" small RNAs (Pak & Fire, 2007). The products of the amplification are then called "tertiary" small RNAs (Sapetschnig et al., 2015).

Crossing the Weisman Barrier

When introducing Fire and Mello's discovery, I mentioned the problem of the Weismann Barrier. The concept of the Weismann Barrier prohibits signals from passing the barrier between soma and germline. There is much more research to be done to elucidate the requirements for crossing it. Here I will discuss a model of small RNA soma to germline transmission, based on a recent review on the principles of small RNA inheritance (Rechavi & Lev, 2017). In addition, I will introduce the "reconstitution" view of inheritance.

Small RNAs are most likely inherited as dsRNA. From the intestine, dsRNAs are transferred together with vitellogenin, a yolk protein, to the oocyte. In the developing embryo, dsRNAs give rise to primary and secondary siRNAs in the cytoplasm. These small RNAs associate with a specific argonaute, which shuttles them into the nucleus. In the nucleus, they can execute nuclear RNAi and engage in gene silencing and a self-amplificatory loop.

At this point, I want to explain why the discovery of small RNA inheritance made such an impact on the field of epigenetics. For decades it has been unclear why epigenetic phenomena are observable at all. It is a well-known fact that epigenetic marks that accumulate throughout the lifetime of an organism get erased in the fertilized egg (in animals). If epigenetic marks are erased in the fertilized egg, it is highly unlikely that epigenetic effects will be observable throughout generations. Nevertheless, the effects persist.

RNAi inheritance provides a solution to this problem. I coin this solution the "reconstitution" view of inheritance. The "reconstitution" view is the opposite of the "replication view." The replicational view is the received view on how information is maintained and transmitted: transmission is secured through the faithful replication of DNA. Through faithful replication, epigenetic marks on DNA can be maintained. Small RNA inheritance suggests a new perspective on epigenetic inheritance because it suggests the "reconstitution view." Small RNAs are inherited in parallel to DNA (Fig. 9). In each generation, small RNAs interact with chromatin structures and cause the deposition of histone marks. This interaction with chromatin also guarantees the amplification of small RNAs. Faithful replication of histone marks is not needed. If small RNAs reconstitute histone marks faithfully in each generation, then histone marks need not be inherited. As a result, small RNAs are the effectors of epigenetic inheritance. I will come back to the "reconstitution" view in chapter seven, in which I will develop a normative pluralist position for small RNA inheritance.

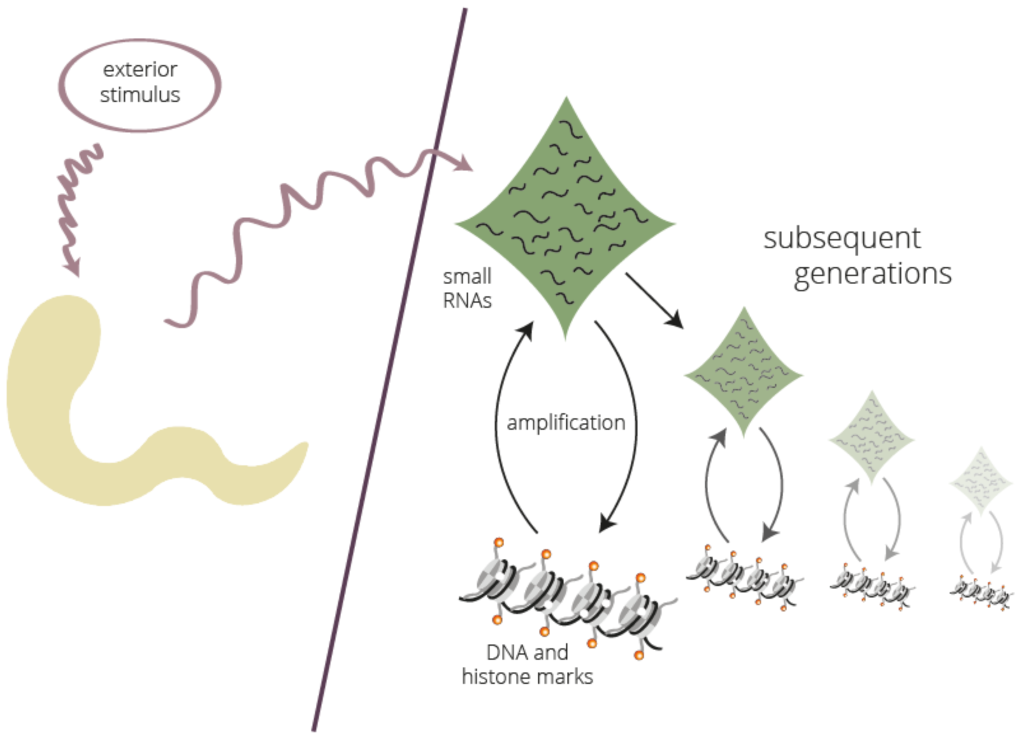

Figure 9 Small RNA based effects are inherited through reconstitution. One generation is exposed to an environmental stimulus, small RNAs inherit the response to the stimulus. In each generation, they reconstruct histone marks to sustain their amplification.

Adaptation

How is small RNA inheritance adaptive? Assume that an unchallenged worm is in a steady-state equilibrium. RNA interference is a physiological process that regulates many endogenous genes as well. There is a particular pool of small RNAs that contain specific concentrations of individual small RNAs, each with its respective targets. There is a certain rate of maintenance; there is a certain rate of decay. Specific small RNAs enter the nucleus and execute their steady-state maintenance function for specific stretches of DNA. They thereby amplify themselves. Endogenous small RNAs regulate their inheritance. They do this by targeting effector proteins that mediate RNAi inheritance. If the number of inherited endo-siRNAs increases, the number of endo-siRNAs targeting the mRNAs of effector proteins increases as well. As a result, these mRNAs are downregulated. Therefore, endo-siRNAs limit their inheritance if they are in an equilibrium state (Veigl, 2017).

Assume that this equilibrium changes. A specific stimulus enters the cell. A virus infects the worm. It gets cold or very hot. Food is rare. In response, the worm synthesizes small RNAs that can address this situation. Thereby the amount of total small RNA changes and the concentration of individual small RNA species changes as well. Argonaute proteins and other effectors are crucial for small RNAs to execute their functions. If only the amount of small RNAs increases, the result will be a shortage of such argonaute proteins. Different species of small RNAs, for example, endogenous vs. exogenous small RNAs or "environmental" vs. "physiological" small RNAs, compete for binding to these proteins. Small RNAs that bound to an argonaute under steady-state conditions will have no binding partner anymore. "Environmental" RNAs will, in turn, be bound to an argonaute. The equilibrium changes. Steady-state processes that were maintained by physiological small RNAs will now be disrupted. New epigenetic marks might be installed if environmental RNAs are complementary to other sequences on DNA. The result is the amplification of environmental small RNAs. In the case of viruses, RdRPs amplify viRNAs in the cytoplasm. Adaption is the result of the change in the equilibrium. The resulting pool of small RNAs reflects an "adapted state." If this adapted state is inherited, it provides adaptive information to future generations (Veigl, 2017).

Silencing vs. activation

So far, I have focused on transgenerational silencing of genes or viral RNAs. For the sake of simplicity, I will focus on silencing also in sections to come, as most scholarship on RNAi inheritance reports transgenerational silencing effects. A growing body of evidence, however, suggests that RNAi can also lead to the promotion of transcription. Researchers hypothesize that there might be different silencing and "licensing" argonautes. If a small RNA would associate with a licensing argonaute, the transcription of the complementary target is promoted and not inhibited (Rechavi, 2014). In the language of the library metaphor, licensing small RNAs are "librarians" who take individual books back in stock. "Licensing" capacities of RNAi inheritance would widen the scope and potential of RNAi inheritance.

2.3 Small RNA inheritance – a case for theoretical pluralism

In the last section, I discussed several phenomena and mechanistic insights into the inheritance of small RNAs. Although C. elegans was the focus of the last section, I also pointed to phenomena in other model organisms. In this section, I will focus on mechanisms in C. elegans exclusively and show that small RNA inheritance qualifies as an instance of Lamarckian inheritance. I will proceed in four consecutive steps. First, I will argue that RNA inheritance qualifies as Inheritance of Acquired Traits (IAT) by demonstrating how RNA inheritance meets three criteria: information, adaptation, and amplification. Second, I operationalize Lamarck's thoughts on inheritance. Third, I show that instances of small RNA inheritance fit the operationalization of the Lamarckian scheme and thereby count as a legitimate case of Lamarckian inheritance. Fourth, I differentiate between different versions of pluralism and set my focus on theoretical pluralism. In so doing, I review the potential benefits of a pluralist position.

2.3.1 RNA inheritance is IAT

In this section, I will propose three criteria that qualify a process as a form of IAT. These necessary and sufficient criteria can be realized differently by different systems. I aim to show that RNAi inheritance fulfills these three criteria and thus is an instance of IAT.

For the inheritance of acquired traits, three requirements need to be met: First, information needs to be stored and maintained. Second, the entity that is inherited must react to exterior stimuli to guarantee adaptation to these stimuli. Third, adaptive information needs to be amplified as dilution effects from one generation to the next might occur. If the information is to be maintained transgenerationally, amplification in each generation is necessary. In summary, the three criteria are:

1) Information
2) Adaptation[8]
3) Amplification

[8] The nomenclature for differentiating "adaption" and "adaptation" is not fixed. In this dissertation, I will adopt the following nomenclature: while "adaption" designates the process of adapting, I take "adaptation" to denote the result of adaption.

In the following, I will discuss how small RNAs realize these three criteria.

Information – complementarity

It is quite straightforward how small RNAs store information. As small RNAs consist of a four-letter base code (A, G, C, U), they can store information with this code, similarly to DNA. This code designates the specific "keyword." A small RNA is always complementary to a target. A specific small RNA thus stores the information of a target or a family of sufficiently similar targets (Fig. 10).

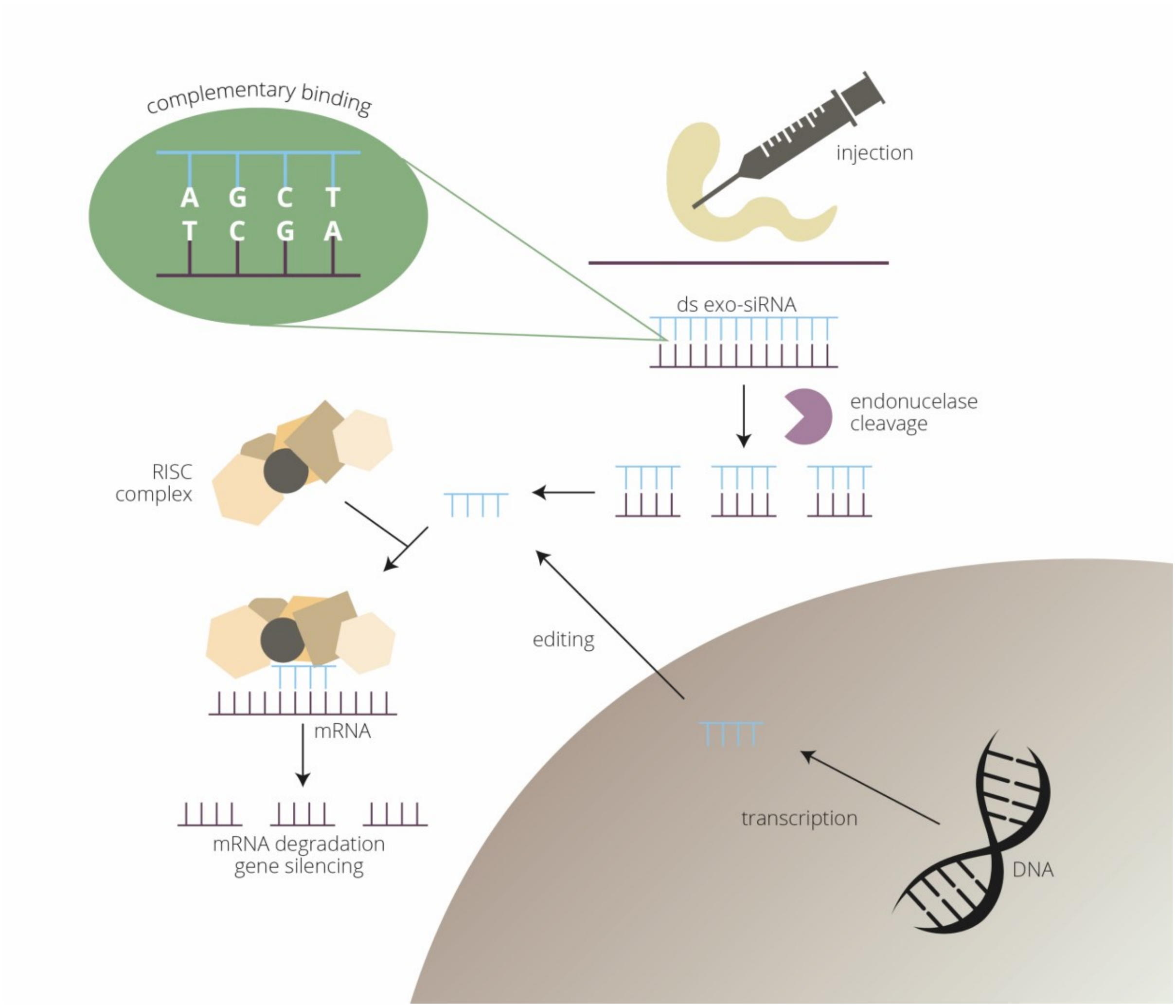

Figure 10 The inheritance of information is realized through small RNA's four-letter base code, as well as its complementarity to the targets.

Adaptation – responsiveness

In order to confer adaptive information to future generations, the small RNA system needs to respond to exterior stimuli. Through its responsiveness to stressors such as heat shock, cold shock, food deprivation, or virus infection, RNAi provides an “adaptive state.” I will illustrate the process of adaptation using the example I discussed in the last section.

Assume a situation in which C. elegans is not challenged by any exterior stimulus. In this case, only endogenous small interfering RNAs (endo-siRNAs) are in the system. These endo-siRNAs maintain a steady-state in that they limit their inheritance by downregulating effector proteins of the RNAi inheritance machinery (Fig. 11).

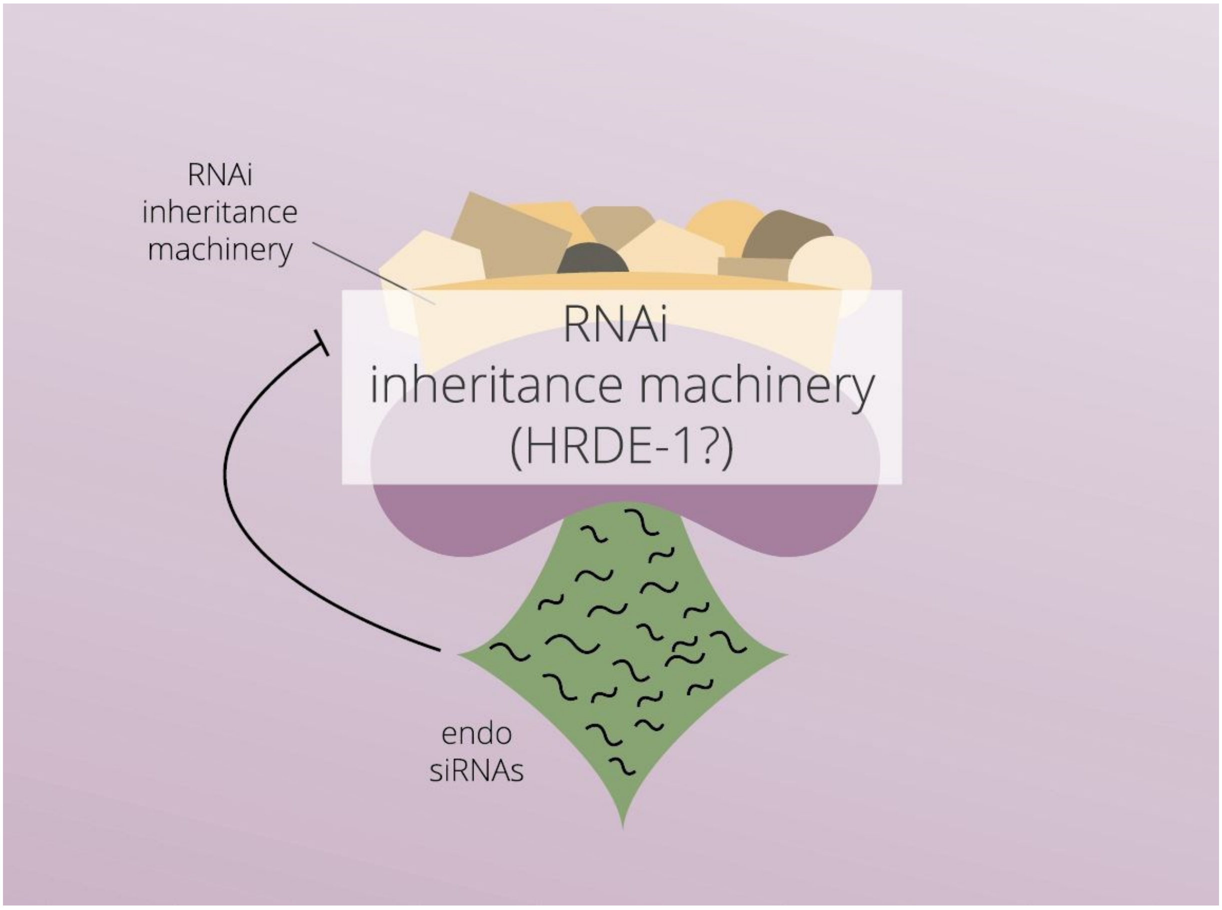

Figure 11 Under steady state conditions, endo siRNAs regulate their own inheritance.

Assume that the worm is challenged by an exterior stimulus. The worm responds to this stimulus by synthesizing small RNAs that can respond accordingly. I coin these small RNAs "exogenous small interfering RNAs" (exo-siRNAs). Technically, this term is not entirely correct, as "exogenous" refers exclusively to RNAs that come from the outside. For the scenarios I described, only viRNAs are genuinely exogenous. RNAs expressed in response to heat shock, cold shock, or starvation are technically endogenous. For this section, I take "exogenous" to include all RNAs synthesized in response to an exogenous stimulus (i.e., "exogenously induced" small RNAs). In response to this stimulus, a considerable number of exo-siRNAs is produced. Exo-siRNAs have the same effector proteins as endo-siRNAs. As a result, exo-siRNAs and endo-siRNAs compete for binding with these effector proteins (Fig. 12).

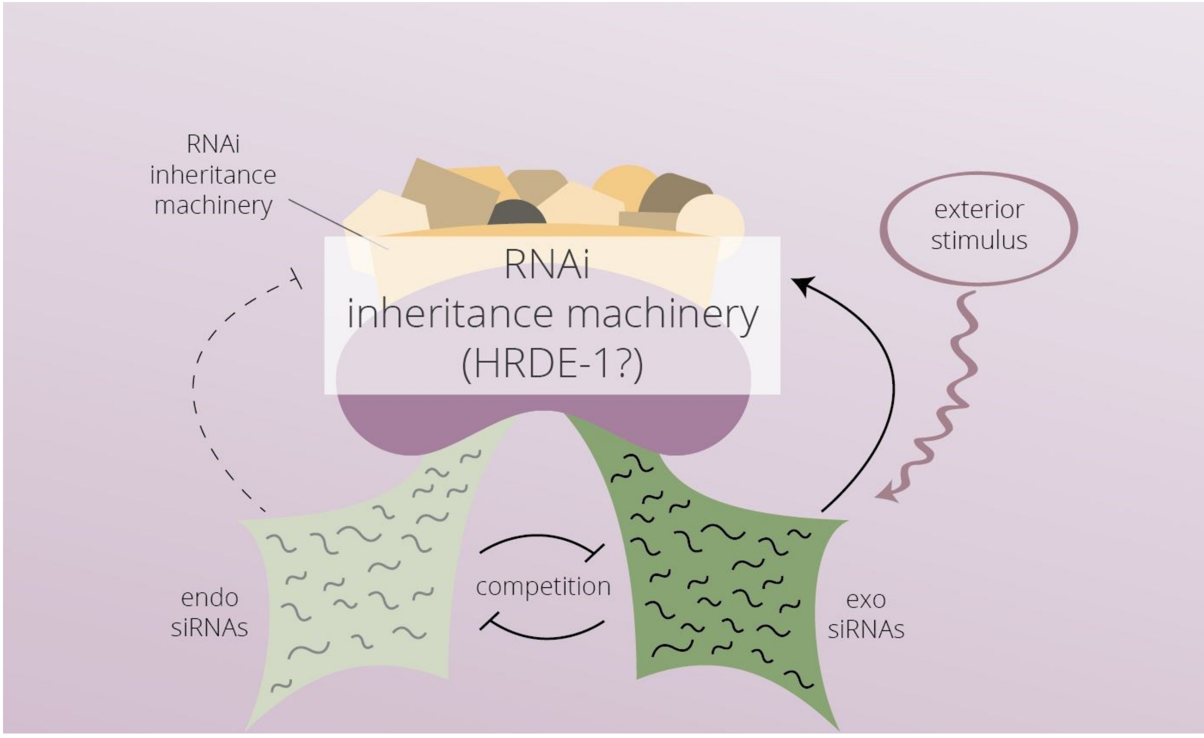

Figure 12 In response to an exterior stimulus, exo-siRNAs are produced which compete with endo-siRNAs for binding to the RNAi inheritance machinery. As a result, endo siRNAs can no longer regulate their own inheritance.

This competition creates a new equilibrium state. Some endo-siRNAs which were bound to effector proteins before are not bound to them anymore. Exo-siRNAs which were not bound to an effector protein are now bound to an effector protein. This new equilibrium state is the result of the responsiveness of small RNA systems to exterior stimuli. Because fewer endo-siRNAs are bound to their effector molecules, they cannot inhibit the RNAi inheritance machinery as effectively as before. More endo- and exo-siRNAs are inherited. The new equilibrium state is an "adapted" state. By transmitting information through the "adapted state," RNAs deliver an adaptive potential to the next generation. If the new equilibrium state is inherited, information on adverse stimuli in the last generation, and how the organism responded to it is passed on to the new generation. This information might be adaptive (Fig. 13).

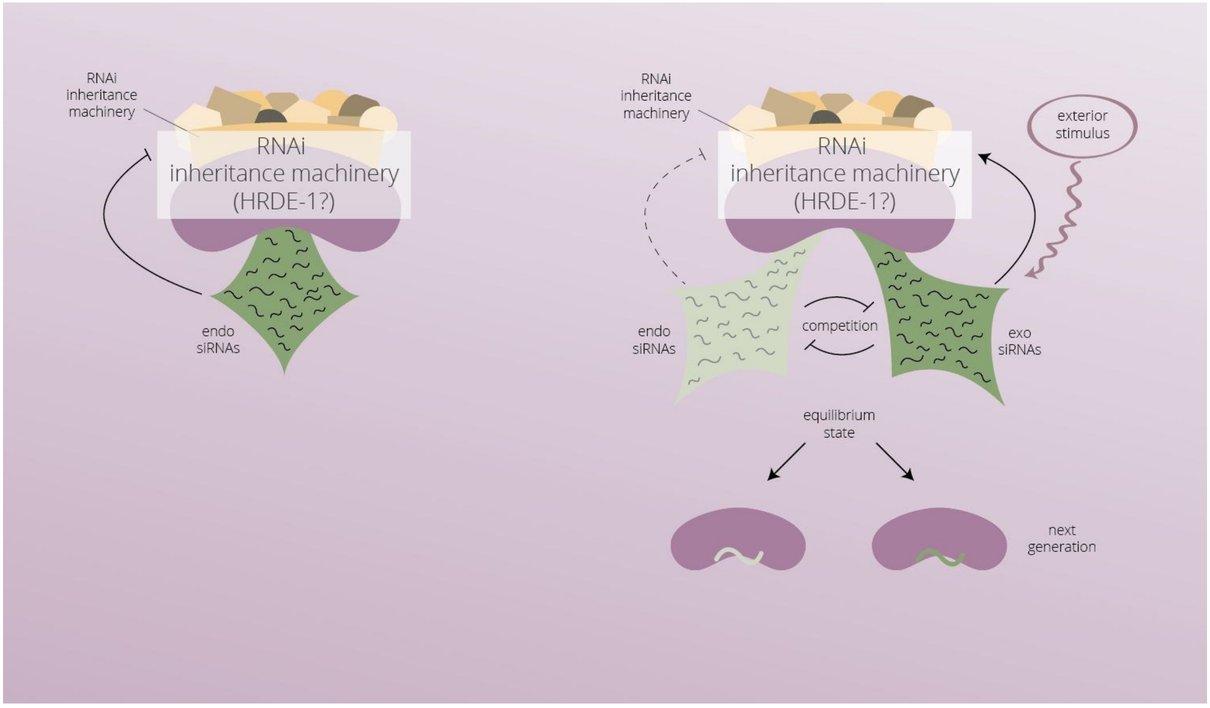

Figure 13 The reached equilibrium state of endo siRNAs and exo siRNAs is inherited to the next generation, providing information on how the organism responded to a particular stimulus.

Amplification – the use/disuse paradigm

The last criterion is individuated by the Lamarckian character of this system. In the next section, I will explicate in more detail why small RNA inheritance fits a Lamarckian use/disuse paradigm. In this section, I will emphasize how small RNAs are amplified in general.

RNA-dependent RNA polymerases (RdRPs) amplify small RNAs. RdRP-dependent amplification of small RNAs can be nuclear or cytoplasmatic. Both Argonautes of the RNAi inheritance machinery as well as RdRPs are limited resources. The synthesis of exo-siRNAs impedes the synthesis of endo-siRNAs as they compete for the same resources, the "RNAi-inheritance machinery." If fewer endo-siRNAs are bound to their effector proteins, fewer of them can enter the nucleus and secure their self-amplificatory system. In turn, through binding with effectors of the RNAi inheritance machinery, exogenous small RNAs promote their amplification (Fig. 14).

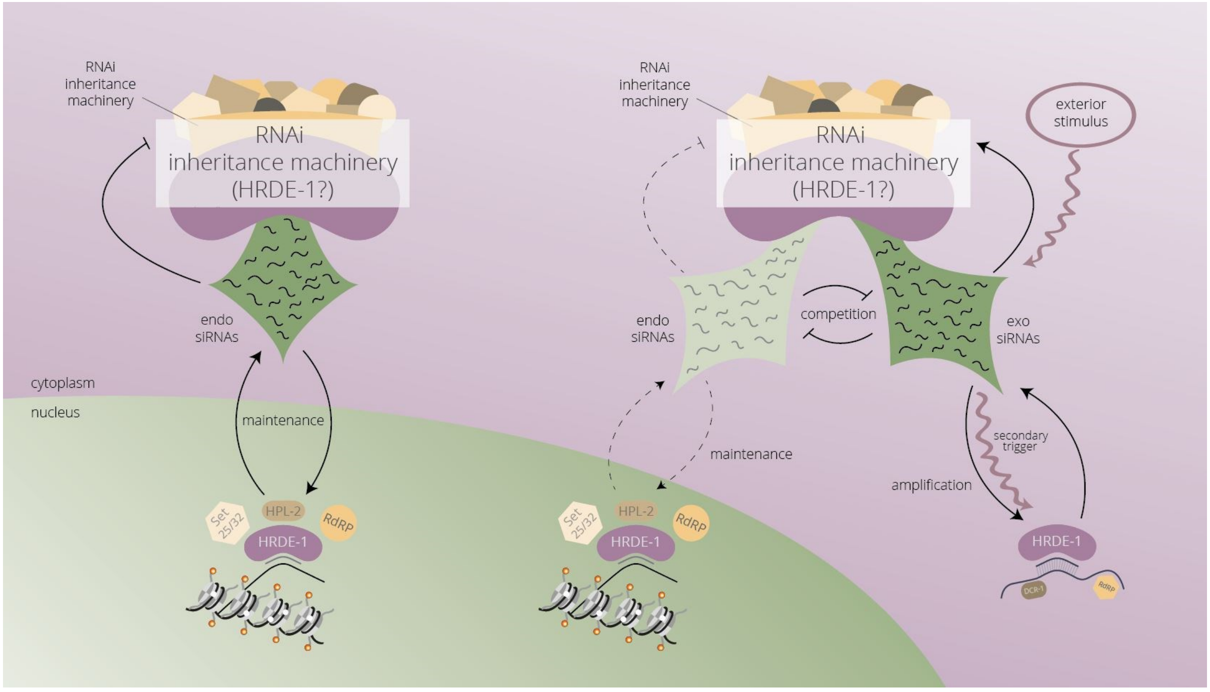

Figure 14 Both exo- and endo siRNAs can be amplified. Endo siRNAs are amplified through co-transcriptional gene silencing (see Fig. 8). Exo siRNAs are amplified in the cytoplasm after exposure to a second exterior stimulus.

2.3.2 Operationalizing Lamarckian inheritance

The use of the term "Lamarckism" is relatively controversial amongst biologists. There is no precise terminology. Many terms refer to a vague concept: "Neo-Lamarckism," "Lamarckism," "quasi-Lamarckism" and "soft inheritance." Most of these terms mean "the inheritance of acquired traits, in one way or the other." This loose and too general terminology has rightly been criticized.

On another occasion, I suggested to free the term "Lamarckian" from this baggage (Veigl, 2017). We should abstain from terminology that superficially equates Lamarck's propositions with cutting-edge phenomena. The term "Lamarckism" should denote the correspondence of a phenomenon with Lamarck's proposed mechanism for the inheritance of acquired traits, which he provided in 1809 in the *Zoologie Philosophique*. Lamarck suggested a mechanism that induces gradual change in response to environmental stimuli. Lamarck's account has three different levels:

> "(1) the inheritance of acquired traits as a very broad and general concept;
>
> (2) the "use" and "disuse" of "organs" leading to their reduction or augmentation as a mechanistic explanation; and
>
> (3) the "fluides incontennables," i.e. non-discrete, subtle fluids as the mediators of the reduction or augmentation of organs" (Veigl, 2017, 1700).

Differentiating between these three levels helps to determine at which level to look for resonance with molecular phenomena. The first level is quite general and not unique in Lamarck's approach. Darwin believed in IAT as well. It is a common mistake to call any IAT "Lamarckian." The third level is also not helpful. Lamarck proposes a causal entity: "fluides incontennables." It is unclear whether it is even possible to examine which molecular entities could correspond to "uncontainable fluids." It is important to note that "fluids" were a popular explanatory tool in Lamarck's time. Lamarck's concept of fluids needs, therefore, to be contextualized as a peculiarity of his time. The second level is the most feasible level for looking for resonances. Lamarck suggested a mechanistic paradigm: the use/disuse paradigm. In what follows, I will investigate whether use/disuse guides small RNA inheritance.

2.3.3 Small RNA inheritance is an instance of Lamarckian inheritance

To explain how small RNA inheritance fits the Lamarckian use/disuse paradigm, I need to come back to the competition model explained in the previous sections. I reviewed the competition of different species of small RNAs for a limited number of proteins. This competition results in a shift from a steady-state equilibrium to an "adapted equilibrium" or an "adapted state." At the same time, the competition model points to an essential aspect of RNAi inheritance in general. In the case of RNAi inheritance, it is impossible to extrapolate singular molecular entities that inherit information. Pools of small RNAs inherit a specific adapted state.

There is a substantial quantitative aspect to RNAi inheritance. This quantitative nature has many instantiations. On the one hand, small RNAs do not have strict one-to-one relationships with their targets. Different small RNAs can target the same mRNA. Or one small RNA might target several mRNAs if the mRNAs are similar enough. These quantitative aspects are necessary for fine-tuning and flexible responsiveness to environmental stimuli. On the other hand, environmental stimuli are no binary commands. In principle, the organism is confronted with environmental stimuli all the time. There needs to be a spectrum of strong to weak small RNA responses to these stimuli. “Strong” or “weak” stimuli are resonant with the extent of the resulting changes in pools of RNAs.

These quantitative aspects suggest how to integrate small RNA inheritance with the use/disuse paradigm. In what follows, I will present my interpretation of use and disuse in terms of competition of small RNAs for effector proteins. “Use” denotes the association of a small RNA with its respective effector molecule. “Disuse” denotes the absence of association of a small RNA with its respective effector molecule. Use and disuse of small RNAs lead to “augmentation” or “reduction” in the next generation, respectively (Fig. 15).

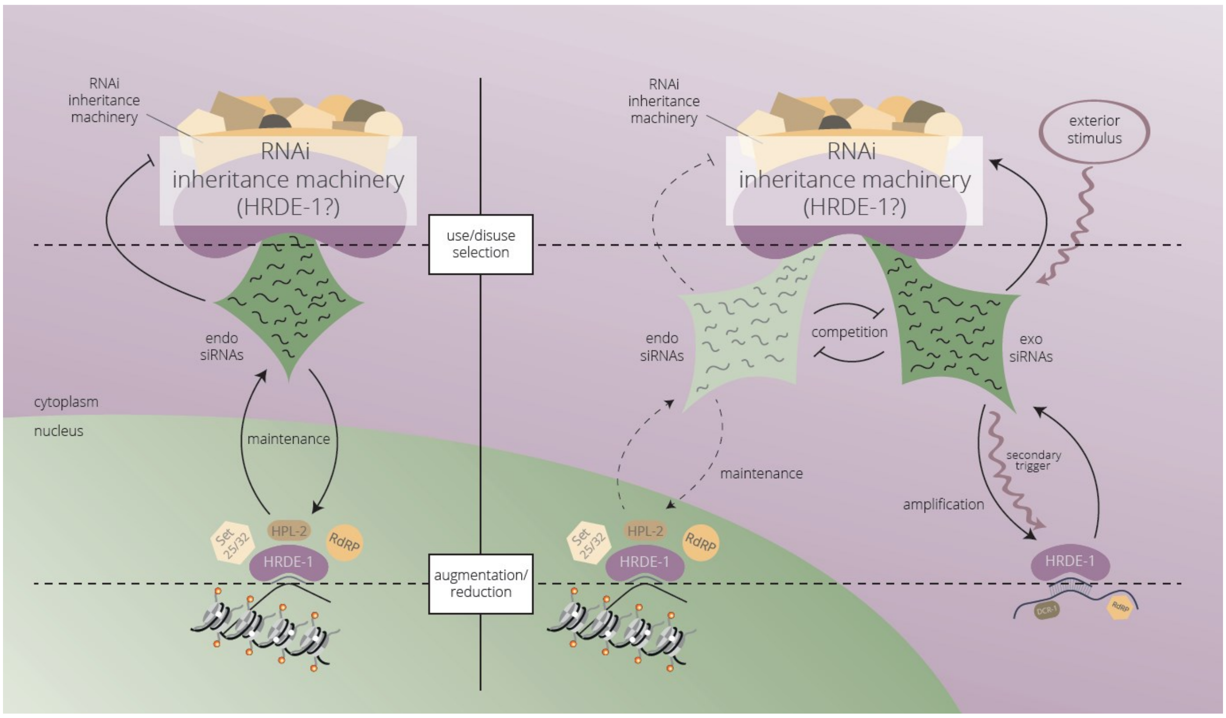

Figure 15 Implementation of the Lamarckian use/disuse scheme on two different levels.

In a steady-state condition, "use" of endo-siRNA leads to nuclear RNAi and a self-amplificatory loop. Through this amplificatory loop, the concentration of endo-siRNAs in the pool is increased; in other words, the amount of endo-siRNAs is augmented. This augmentation persists in the next generation through the inheritance of this pool of small RNAs. In a situation where the organism is confronted with an exterior stimulus, pools of endo-siRNAs are "disused." They are disused because exo-siRNAs compete with endo-siRNAs for binding, and thus fewer endo-siRNAs can bind their effector proteins. As fewer endo-siRNAs occupy effector proteins, they can engage less in their self-amplificatory loop. As a result, the concentration of endo-siRNAs decreases and fewer endo-siRNAs are inherited. Because the concentration of endo-siRNAs was reduced through disuse, more exo-siRNAs are used. The use of exo-siRNAs through association with their effector proteins leads to the amplification and thus augmentation of exo-siRNAs. Exo-siRNAs are amplified for at least 3-5 generations (Fig. 15). If there is a second trigger in the second generation, amplification is boosted again, because even more endo-siRNAs are "disused" and more exo-siRNAs are used. If there is no second trigger, exo-siRNAs will be disused after 3-5 generations, and their amount will be reduced.

In summary, the "use" and "disuse" equilibrium of pools of small RNAs can result in the augmentation or reduction of a trait. "Trait", for example, are particular responses to stressors. Anti-viral immunity is a trait. This trait corresponds to the respective endo/exo RNAi equilibrium state. This specific trait is "acquired" since it depends on exterior stimuli and exo-siRNAs. Lamarck's use/disuse paradigm provides RNAi inheritance with a mechanistic explanation. If RNAi inheritance is governed by a use/disuse paradigm, then RNAi inheritance is not only an instance of IAT in general but an instance of Lamarckian IAT (Fig. 15).

Elsewhere, I have hinted at two more examples to which the use/disuse paradigm might apply as well: "Lamarckian use/disuse paradigms might qualify as a ubiquitous concept that helps to understand several different processes within the cell" (Veigl, 2017, 1002). At this point, I need to address the question of whether the use/disuse paradigm should be considered the "real" mechanism underlying small RNA inheritance. I think that it captures most phenomena of RNAi inheritance very well. There is no other mechanistic model that captures or explains the phenomena equally well. Therefore, I want to substantiate my claim epistemologically: so far, the use/disuse paradigm provides researchers with the best understanding of the processes of RNAi inheritance.

2.3.4 Small RNA inheritance and scientific pluralism

In this section, I advance my claim that RNAi inheritance is a case for scientific pluralism. I will discuss three different types of scientific pluralism: the plurality of mechanisms, the plurality of kinds of explanations, and the plurality of theories. I will focus on advancing a claim for theoretical pluralism.

2.3.4.1 Plurality of mechanisms

The first level of plurality is the plurality of mechanisms. For the sake of this elaboration, I will rely on the framework on mechanism developed by the "new mechanist" philosophy. Along these lines, four essential features of mechanism are central: a phenomenon, parts, causings, and organization. New mechanist philosophers have focused their examinations around the question of how scientists organize their work in the search for mechanisms. The new mechanist philosophy of science is practice-based. Usually, new mechanists do not provide necessary and sufficient criteria for what a mechanism consists of. Instead, they try to offer definitions that are resonant with how scientists use the term. One standard definition, by Peter Machamer, Linley Darden, and Carl Craver, goes as follows: "Mechanisms are entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions" (Machamer et al., 2000).

To argue for the plurality of mechanisms, it is pivotal to define the target phenomenon. I define the inherited phenotype as the target phenomenon. Plurality of mechanisms requires that there is more than one mechanism necessary to produce the inherited phenotype. For my concrete example, I propose DNA-based and RNA-based mechanisms of inheritance as two mechanisms that produce the inherited phenotype. There are two separate arrays of entities and activities which are both organized in a specific way and which together produce the inherited phenotype.

I anticipate a counterargument: there is only one mechanism responsible for the phenomenon of inheritance. The sum of “RNA-based” and “DNA-based” entities, activities, and organizations constitute one mechanism. It might even be possible to link or reduce “RNA-based” entities to “DNA-based” predecessors. In the end, RNA is encoded in DNA, isn’t it?

One way of addressing this counterargument is to highlight the frequent confusion about the role of DNA. This confusion concerns the hierarchical position of DNA within and between generations. DNA has biochemical primacy within one generation, as DNA is the biochemical basis of almost all molecules that are synthesized in the cell. Biochemical primacy must not be mixed up with primacy in inheritance. This becomes clear when zooming in on the time point when molecular entities are inherited. At the time point of inheritance, there are two different kinds of entities, with no hierarchical relation and not necessarily interactions. DNA and RNA are inherited in parallel.

At least two mechanisms, therefore, produce the inherited phenotype. Here it is necessary to differentiate between “the inherited phenotype,” which is the sum of all inherited traits, and a specific inherited trait. Inherited traits can be produced by one mechanism, or the other, or both. Because the inherited phenotype is the sum of all such inherited traits, and each trait is produced by one or two mechanisms, the inherited phenotype is always produced by two mechanisms[9] (Fig. 16).

Many individual traits are exclusively produced by stretches of DNA. But other phenotypes are the product of DNA- and RNA-based inheritance. Certain individual traits, such as the resistance to a specific virus, can be produced by RNAs exclusively. One could also imagine scenarios in which small RNAs mask traits that are produced by DNA. Here is an example. Assume that a particular phenotype requires two homologous alleles. If the phenotype is absent, the two alleles in the respective organism are heterologous. But there is also another possibility. Even though two alleles could be homologs, one allele could be silenced by small RNAs. This scenario also works the other way around. Small RNAs could implement

[9] Alternatively, it is possible to talk about two “modules” of one mechanism. I am much indebted to Hernan Bobadilla, with whom I had the chance of extensively discussing the plurality of mechanisms in small RNA inheritance.

a compensatory mechanism even though the two alleles are heterologous. I conclude that for each organism and each phenotype, it needs to be assessed whether one or both trajectories of inheritance produce the phenotype (Fig. 16).

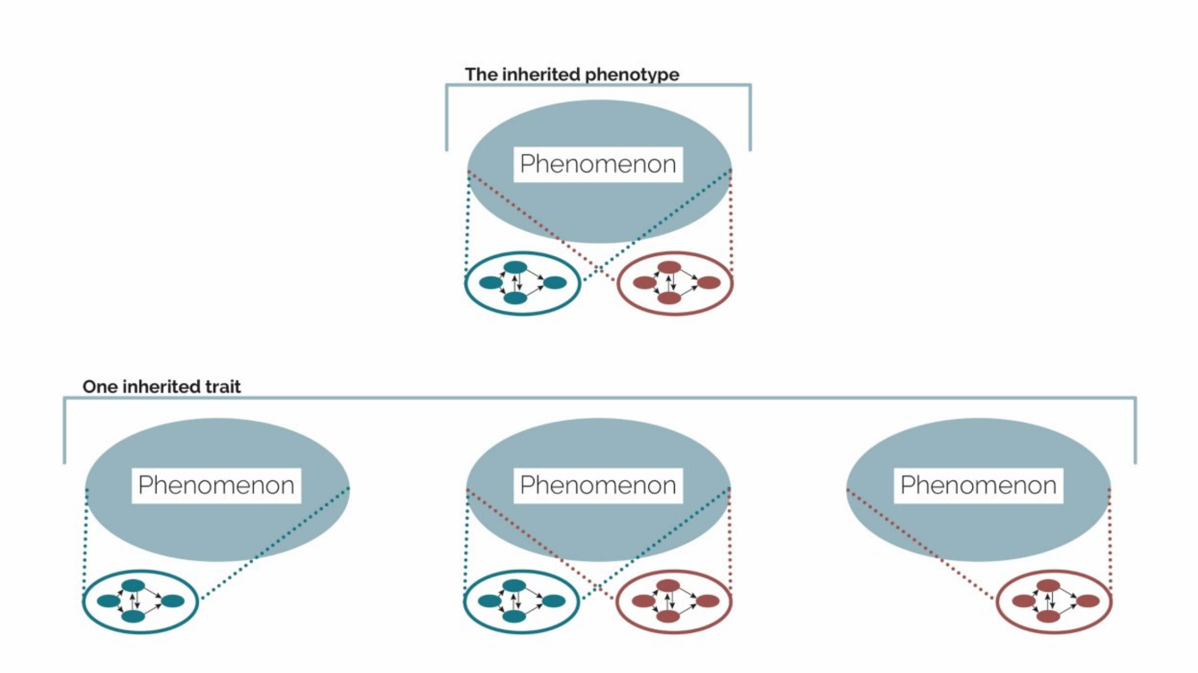

Figure 16 Differentiating mechanistic plurality regarding the inherited phenotype and one specific inherited trait.

In the last paragraph, I hinted at the "masking" of a specific phenotype. Masking has another consequence as well. Assume that a deleterious phenotype is produced by a DNA-based mechanism but is covered up by a small RNA-based mechanism. The small RNA response usually lasts for 3 to 5 generations. In such a scenario, organisms would carry a deleterious mutation that has no chance of being detected and selected against as it is masked. This also works the other way. Assume an organism acquires a mutation of adaptive value, but small RNA-based effects mask this mutation. Or the adaptive value is also realized by a small RNA based mechanisms. In such a scenario, the beneficial mutation cannot be selected. In conclusion, small RNAs could work against long-term evolution.

In this section, I have reviewed the significance of mechanistic plurality for approaches towards small RNA inheritance. The mechanisms of small RNA inheritance are instances of two mechanisms, that in parallel contribute to the inherited phenotype. While it is possible to characterize them as two (or three, taking into account mechanisms that require the interaction of DNA- and small RNA based mechanisms) mechanisms, it is also possible to regard them as modules of the same mechanism. The assumption that different mechanisms can be causal for a specific effect seems however quite uncontroversial. Are not sperm and eggs, for example, two factors that cause the development of the zygote? It would be hard to formulate a position disagreeing with this notion. Mechanistic plurality seems thus quite uncontroversial for the particular case of small RNA inheritance. I will thus explore other instances of plurality and examine them for their bite.

2.3.4.2 Explanatory pluralism

Literature on explanatory pluralism is currently skyrocketing. Some prominent authors are, amongst others, Carla Fehr (2006), Angela Potochnik (2017) or Catherine Elgin (2017). Linked to the idea of explanatory pluralism is often a particular notion of "scientific understanding." Different explanations, these authors argue, although they might be incompatible, or even contradict each other, might at the same time increase scientific understanding of a particular target phenomenon.

In this section, I will propose that small RNA-based inheritance and DNA-based inheritance require different kinds of explanations. DNA-based explanations are mostly qualitative: the presence or absence of a trait is explained by referring to its presence in a stretch of DNA. By contrast, RNA-based explanations also have a quantitative character.

RNA-based inheritance has both a qualitative and quantitative component. Small RNAs carry qualitative information through their code. The effects of small RNAs are, however, entirely dependent on their respective quantities. In other words, an effect cannot be explained by referring to the presence or absence of a molecule. An effect can only be explained by the pools of small RNA species and their respective concentrations.

It is also impossible to state the following: if 3,000 small RNAs are inherited, effect X will be the result. Much more information is needed. It is necessary to know how many other small RNAs of which exact kind are inherited. As argued before, small RNAs compete for effector molecules. If there are several *sufficiently similar* small RNAs they could execute the same effect – slightly less or more efficient. Therefore, I claim that a trait that is caused by small

RNA or by small RNAs and DNA always requires a quantitative explanation in addition to a qualitative one. Thus, there is a plurality of kinds of explanations: qualitative and quantitative. In her 2015 paper, Frederique Thery even suggests that in addition to qualitative and quantitative explanations, some effects of small RNAs need to be understood through a topological explanation (Théry, 2015). I will briefly consider topological explanations of small RNA based phenotypes in Chapter Seven. It might be quite trivial to most small RNA researchers that some aspects of small RNA based effects are caused by quantitative aspects, and others are caused by particular qualitative properties. I will continue to examine theoretical plurality, because I believe that theoretical plurality offers the most potential for controversy.

2.3.4.3 Theoretical pluralism

In this section, I will advance my claim about the plurality of theories: Lamarckian and Neo-Darwinian theories of inheritance constitute a case for theoretical pluralism. I will avoid using the "e-word" because I am not talking about theories of evolution. When I use the term Neo-Darwinism, I refer to a consensus on how traits are inherited. Neo-Darwinism as a theory consists of a combination of Mendelian rules of inheritance with Darwinian concepts of variation and selection. I avoid the issue of evolution because there are no studies conducted yet that show the influence of small RNAs on evolution. Nevertheless, such influences are certainly conceivable, as I have hinted at in the section on the plurality of mechanisms.

To demonstrate the plurality of theories, I will review three reasons why small RNA inheritance cannot be accounted for by a Neo-Darwinian framework: DNA is not the material basis of small RNA inheritance; small RNAs do not follow Mendelian rules; changes in inherited small RNAs happen nonrandomly.

1) *DNA is not the trajectory of RNA-based inheritance.* DNA may encode small RNAs. But it is not possible to infer anything about their inheritance by knowing that they are encoded. Most types of small RNAs are encoded in DNA – that does not qualify them for being inherited. For being inherited, a dsRNA stage might be required. Also,

being encoded in DNA does not define which small RNA will be inherited and under which conditions.

There are even more extreme examples that show that RNAs do not need to be encoded in DNA in order to be inherited. RNAs have their polymerases for amplification in each generation. In principle, a DNA stage is not even necessary, yet it is often made use of. For instance, viRNAs do not have DNA phases and are nevertheless inherited for several generations. Thus, it is no coincidence that small RNAs are inherited in parallel with DNA: this cements the complementarity and relative independence of both paradigms.

2) *RNA inheritance does not follow Mendelian rules.* When a cell divides, the two daughter cells will inevitably be heterogeneous concerning species and concentration of small RNAs. There exists no mechanism in the cell to separate and distribute small RNAs faithfully. Small RNA can even hijack routes of Mendelian inheritance. As discussed above, small RNAs can silence stretches of DNA and thereby mask traits. Even if a trait separates according to Mendelian rules, small RNAs can silence it and thereby produce a non-Mendelian phenotype. The absence of faithful distribution will come in again crucial in Chapter Seven.

3) *RNA inheritance is non-random.* RNAs are responsive to exterior stimuli and inherit this state of responsiveness to the next generation. By contrast, changes in DNA happen randomly through replication errors.

Small RNAs organize chromatin. Small RNAs might even influence which stretches of DNA mutate. If a small RNA dictates the deposition of a chromatin modification through nuclear RNAi, then the targeted histone and its environment are much less accessible for random mutations. A chemical group (e.g., methyl) protects the region. Additionally, small RNAs are essential for structuring DNA. Without nuclear RNAi, DNA loses its compaction, especially at the ends (telomeres) and the centers (centromeres) of the chromosomes. Losing compaction is a disaster because the integrity of these regions is pivotal for cell division. In certain species there are even more extreme cases in which small RNA can “cut out” and delete stretches of DNA. Therefore, there is no one-way hierarchy of DNA over RNA. Small RNAs can shape, structure, and organize DNA.

"Die-hard" Neo-Darwinists argue that the proteins which are essential for RNAi inheritance are encoded in DNA. They are subjected to random mutations and Neo-Darwinian principles. I acknowledge that this argument is partly valid at best. Small RNAs can regulate the accessibility of different stretches of DNA, amongst them probably also stretches encoding RNAi effector molecules. RNAi effector molecules can thus be subjected to DNA-based random mutations as well as RNA-based targeted modifications that influence the rate of mutations. Therefore, the argument of Neo-Darwinists can be rejected.

In the last paragraphs, I showed that small RNA inheritance requires a second theoretical perspective. This results in theoretical plurality: Neo-Darwinian and Lamarckian theories of inheritance both approach the phenomenon of inheritance. Both theories are needed to account for all processes of inheritance. In the remainder of this section, I will provide arguments for why one should become a pluralist about theories of inheritance. I will advance a version of epistemological pluralism. I will not argue that theoretical pluralism of inheritance shows something about what the world is like. But I will argue that there are epistemological benefits of using both DNA- and RNA- based theories of inheritance in order to understand phenomena of inheritance.

2.3.4.4 Benefits of scientific pluralism for small RNA inheritance

It is no coincidence that the term "heresy" is often used to describe accounts of small RNA inheritance. Many evolutionary biologists claim that it is heretical to believe in a mode of inheritance other than the Neo-Darwinian. These authors seem to take it for granted that it is compulsory to believe only in one mode of inheritance, not two. I acknowledge that the emphasis on the replicational model of the central dogma has kept scientists from engaging with other approaches and other theories of inheritance. There is something intrinsically wrong with prohibiting accounts that explain phenomena well only because they do not follow a ruling paradigm. At the same time, I do not believe that it is wrong to use Neo-Darwinist explanations for some phenomena. The philosophical position in accordance with this stance is scientific pluralism (SP).

A pluralist about theories of inheritance accepts both theories of inheritance as partly successful in providing an account of inheritance. In what follows, I will substantiate this position by listing the benefits of adopting a pluralist account. The philosopher and historian of science Hasok Chang, who is one of the most ardent defenders of scientific pluralism, provides a list of the benefits of scientific pluralism. There are “benefits of toleration” and the “benefits of interaction.” In the remainder of this section, I will demonstrate the benefits of being a scientific pluralist about small RNA and DNA inheritance accordingly, by working my way through Chang’s list.

Hedging the bet

Even for those who believe that in the end, there must be only one theory, pluralism is the more productive strategy. “Since the course of scientific development is unpredictable, it will be helpful to keep multiple lines of inquiry open in the hope that one of them will lead us to the right answer.” (Chang, 2012, 270). We do not know whether small RNA inheritance, or DNA inheritance, or both will lead us to our future attainments, we study both.

Division of domain

Different approaches might only partially fulfill specific aims. “If our best theory covers only some of the known observable phenomena, then we need to have other theories that cover the rest” (2012, 272). Small RNA inheritance and DNA inheritance cover different phenomena. Sometimes they cover different aspects of the same phenomenon. By entertaining both approaches, the division of the domain can be ensured and more phenomena can be accounted for.

Satisfaction of different aims

Chang contends that multiple legitimate values or aims drive science. Individual scientists might even be driven by different sets of values. “Once we grant that there are multiple human needs that science is called upon to satisfy, it is easy to recognize that we will most likely not be able to come up with the perfect scientific system that satisfies all needs” (2012, 274). Different scientists might weigh the importance of problem-solving ability, accuracy, simplicity, consistency, fruitfulness, scope, elegance, simplicity, completeness, unifying power, and explanatory power differently. Different approaches, such as RNA and DNA inheritance might fulfill different goals better or worse. If both are maintained, different aims can be satisfied. .

Multiple satisfaction

Chang's argument for multiple satisfaction goes as follows: "Plurality enriches knowledge… Such epistemic abundance should delight us" (2012, 276). This argument will not convince a reader who is not already a pluralist, as it appeals to taste. Nevertheless, from my perspective, the abundance of possible theoretical frameworks that explain heredity (DNA and RNA inheritance are by far not the only ones available) is delightful.

Integration

"There are situations in which not one of the available systems by itself, not even all of them additively, can achieve a certain aim" (2012, 279). In these cases, we should aim for ad hoc integration. The idea of ad hoc integration goes as follows: because all theories, explanations, and models are idealizations, they cannot go together on an abstract level. On the level of action, for example, to provide a specific prediction, they need to be integrated.

Most of the techno-scientific objects produced in molecular biology are built pre-supposing Neo-Darwinian theory. Classic genetics is without any doubt the bread and butter of laboratory investigations. And there is no need to change that. Scientific practices and experimenting are a pioneering example of how ad-hoc integration of both Neo-Darwinism and Lamarckism works: desired traits of laboratory animals are produced employing Mendelian genetics. Average mutation rates per offspring are calculated minding random mutations through polymerase infidelity. But insights obtained from research on transgenerational inheritance of small RNAs have also contributed tremendously to methods used on various model organisms. For example, effects caused by small RNA inheritance need to be considered when crossing animals, as RNAi might mask DNA-based phenotypes. As most effects of small RNAs are detectable up to three to five generations after the first stimulus, laboratory animals need to be kept in extremely stable conditions for at least five generations in order to avoid unwanted small RNA effects. The case of laboratory practice thus shows the benefits, if not necessity, of ad hoc integration.

Cooptation

Cooptation is another benefit of pluralism. Cooptation concerns knowledge produced or techniques developed in one theoretical paradigm that can be used by a different paradigm as well (2012, 281). Co-opting research on nuclear RNAi is a good example. Tethering the nuclear RNAi machinery to a specific locus to silence it is a method that might also be employed by those who do not believe in small RNA inheritance. Researchers can make use of methods even though they reject the theoretical assumptions behind those methods.

Competition

"Even if there is neither integration nor cooptation, there can be a productive competitive relationship between co-existing systems of practice" (2012, 282). RNAi inheritance researchers are often heavily criticized, for example, by evolutionary biologists. This criticism and the need to address it helps proponents of small RNA inheritance to sharpen their conceptual tools and identify robust methods and procedures.

In conclusion, several possible benefits of adopting a pluralist position concerning small RNA inheritance become apparent. This dissertation could stop here. I should be happy – from my armchair, I have demonstrated yet another case of scientific pluralism in biology. There are, however, pressing questions yet to be answered that have not been approached in the scientific pluralism literature so far. Are scientists pluralists? And should they become pluralists? In the following chapter, I will review problems of "pluralism in science" and the normativity of philosophers. In Chapter Four, I will move on to answer the questions outlined above.

3 Pluralism, Scientists and Normativity

In this chapter, I will discuss the problems of scientific pluralism that are often overlooked. Particularly, I will focus on two issues. The first issue concerns the question whether scientists who work in fields that display plurality are pluralists. The second issue follows directly from the first. What does it mean for the philosopher's normative perspective if researchers are not pluralists? After addressing these issues in a theoretical manner, I will provide an empirical case study in Chapter Four.

3.1 Pluralism in the sciences

In this section, I will examine the relation between scientific pluralism (SP) and the sciences. That is, SP, an analyst's category, will be assessed for its trajectory within scientific disciplines. I will assess and criticize a notion that is quite popular within the canonical scientific pluralism literature. Card-carrying scientific pluralists suggest that there are genuine cases of "integrative" and "non-integrative" pluralism. That is, they believe that specific (natural) phenomena require either an "integrative" or a "non-integrative" pluralist account. Specific essential characteristics of these phenomena determine whether they require an "integrative" or a "non-integrative" pluralist account.

I will argue that the respective pluralist interpretation of a phenomenon is determined by the respective research tradition and other historical or sociological factors. The "nature" of the respective phenomenon does not determine the pluralist interpretation. I will demonstrate that decisions for or against integration are influenced by socio-professional goals of scientists in the respective field.

First, I will examine several examples from the biological disciplines to show that the admission or rejection of plurality and the interpretation of plurality as integrative or non-integrative is negotiated within the scientific community. Second, I will argue that "integration" is not unique to biology or the life sciences. In the third part, I will examine the reasons for decisions for or against plurality and integration. I will produce a decision tree to illustrate my argument.

3.1.1 Integration or non-integration? Biological examples

Intuition often tells us which things go together and which things do not. This sentiment also seems applicable to theories or explanations. But should such intuition determine our stances regarding "integrative" or "non-integrative" pluralism? Let me start with an example. The historian and philosopher of science Ian Hacking highlights the theoretical plurality throughout the history of medicine. He compares two theoretical accounts of how to cure syphilis. Hacking characterizes these two distinct approaches as two different styles of reasoning.

In the 16th century, syphilis was treated "by a salve of mercury and by internal administration of the metal, because the metal mercury is the sign of the planet, Mercury, and that in turn signs the market place, and syphilis is contracted in the market place" (Paracelsus, cited in Hacking, 1983, 71). Since the first half of the 20th century, however, syphilis is treated by other substances (e.g., antibiotics). It is a common belief that germs cause disease. Antibiotics cure diseases because antibiotics interfere with and kill germs. For Hacking, there is no way of expressing what Renaissance medicine wanted to say about the treatment of syphilis in the language of a 20th-century physician. Also, it proves challenging to reconstruct the cosmologies that are involved in Paracelsus' account of syphilis.

The case of Paracelsian and 20th-century approaches of curing syphilis appear to exclude one another. Syphilis is cured either by mercury or by antibiotics. It is not apparent how to reduce one of the theories to the other. It is hard to express one theory in the language of the other, and integration in a way that acknowledges both paradigms seems somewhat awkward. Our intuition is clear. Hacking's example is a case of "non-integrative" pluralism. Two different theoretical accounts do not go together; integration is not possible. "Integrative" pluralism, that is, for example, Changian *ad hoc* integration, does not seem feasible.

Several card-carrying scientific pluralists champion "integrative" pluralism. In so doing, they often go beyond the descriptive. That is, they claim that within a research field, a specific pluralist interpretation should be adopted. In many cases, philosophers arguing for SP evaluate a research field based on whether it displays enough plurality and whether theories are integrated or regarded as non-integratable. Some philosophers praise integration. Other philosophers find integration non-desirable. They use case studies to provide arguments for or against integration. But *are* there genuine examples of either integrability or non-integrability? Is there something about the “nature” of certain phenomena that makes them examples of integrative or non-integrative versions of SP? Let me examine three case studies in more detail to claim the contrary.

3.1.1.1 Three case studies from different realms of biology

In what follows, I will examine three case studies on plurality in biology. I will review scientific pluralist Helen Longino’s account of theoretical pluralism in the study of behavior; scientific pluralist Sandra Mitchell’s case study of explanatory pluralism concerning the evolution of the clitoral orgasm; and sociologist of science Harry Collins’ work on the study of responses of plants to remote stimuli. Longino argues for “non-integrative” pluralism; Mitchell argues for “integrative” pluralism. Collins demonstrates that replication cannot solve scientific disputes.

Explanations of behavior

Longino describes four “schools” that try to explain behavior. All agree that both nature and nurture influence behavior. They disagree on how to articulate this relation (Longino, 2006, 102). Their explanatory goals differ in the emphasis of causal factors. The behavioral-genetical approach tries to identify the genes contributing to a particular behavior. The socio-environmental approach aims at identifying the influence of environmental factors on behavior. The neurobiological approach tries to assess either anatomical features or specific neurochemical substrates as causes of behavior. The developmental-systemic approach seeks to examine how the interplay of genetic determinants and developmental constraints constitute behavior (2006, 103ff). These four approaches differ in “the structure of causal spaces within which they frame their investigative strategies” (2006, 103). Each approach treats certain factors as causes and others as irrelevant (2006, 126). Longino claims that the study of behavior is theoretically pluralist. Some non-shared assumptions, she argues, make the differences too profound to aim for integration of these approaches (2006, 117).

In addition to these considerations, Longino surveys the relations between the approaches. Proponents of all schools believe that proponents of other schools weigh the factors defining behavior wrongly. But proponents of all programs agree that the other factors play a role and that the contribution of each factor can only be evaluated relative to other factors (2006, 111). Longino explains that there are "natural allies." Proponents of specific approaches form alliances with proponents of other approaches, some "more rhetorical than actual" (2006, 107). Some of these links can be explained by an assumed relation of specific approaches. Proponents of the neurochemical and the genetic approach ally because they assume an intrinsic relation between neuroanatomical structures or molecules and the genes that encode them. Most of the criticism that these approaches level against each other focuses on individual methodologies. According to Longino, these criticisms do not fairly apply as the methods of the one approach do not fit the purposes of the other. This is because each of the four approaches has a different explanatory goal and its methods were developed to achieve this specific goal (2006, 111). If a method is designed to elucidate how a particular gene influences behavior, it might not be able to answer questions on how social factors influence aggression. Each approach generates partial knowledge (2006, 127). Contrary to her pluralist intuition, Longino admits that all actors would be open to finding a theory all four approaches can be reduced to (2006, 117).

Evolutionary origin of the clitoral orgasm

Mitchell discusses a dispute about explanations of the evolution of the clitoral orgasm. One explanation is adaptionist in that it holds that the clitoral orgasm had adaptive value. For instance, it could serve to strengthen the bond between male and female and thus elevate the chance of male contribution in childcare. Proponents of this explanation believe that there was sexual selection for this trait.

The competing explanation is developmental. Proponents of the developmental explanation claim that the clitoral orgasm had no adaptive function. It exists because of developmental constraints between males and females. As the clitoris is the homologue of the penis, and as male and female of one species are the same in terms of their "Baupläne[10]," the clitoral

[10] The german term "Baupläne," meaning "building plans" of organism, is a technical term also in the English speaking literature.

orgasm is brought about by developmental constraints. An example that works the other way around are nipples: having nipples is of adaptive value to women; due to developmental constraints, both sexes develop nipples (Mitchell, 2003, 195). Therefore, Mitchell describes two explanations of one phenomenon: the developmental and the adaptionist account.

There was an additional level to the debate as well. Participants of the dispute raised the possibility of integrating both explanations. They claimed that both explanations work on different levels and that there is ultimately no real exclusiveness. Both explanations were "compatible accounts of a multidimensional body" (2003, 200). Proponents of the developmental approach argued that adaptionists were so preoccupied with their paradigm that many explanations escape their notice. Furthermore, the "developmentalists" argued that they never campaigned for having *only* developmental explanations around. In conclusion, the dispute shows asymmetries. I will expand on the importance of asymmetries in the chapters to follow.

Responses of plants to remote stimuli

Collins describes a case in which the capacity of plants to respond to remote stimuli was investigated. Via the application of electrodes, the plants' response to strong language, threats of burning them, and the killing of brine shrimp in hot water was measured (Collins, 1992, 114). The fluctuation of measured leaf resistance correlated with the input of emotional stimuli. Collins characterizes this finding as on the very edge of the phenomena worked within parapsychology and also a little "beyond the border of science" (1992, 113).

Two different views on plant physiology emerged. One held that plants could respond to direct physical stimuli only. The other held that plants could respond to direct physical stimuli as well as to remote emotional stimuli. The debate extended over the boundaries of parapsychology. Investigators with other specializations tried to replicate initial results. The debate was closed in favor of orthodoxy after several studies failed to replicate the initial findings. But both sides agreed that it never became clear whether replications were correctly done.

The study of plant responses to emotional stimuli did not die out. It lived on in heterodox institutions, remote from mainstream plant physiology. Collins explains this by stating that dialogue did not take place anymore because "technical arguments belonging to the two communities had become so different" (1992, 124). To accommodate such phenomena for a longer time, "either phenomena or science have to change" (1992, 125).

At this point, I will briefly comment on my use of Collins' case-study. Studies in the "Sociology of Scientific Knowledge" in general, and Collins' work in particular do not explicitly target the problems surrounding SP. Nevertheless they describe situations of disputes, that involve the plurality of interpretations. Often, but not all the time, these studies investigate how a state of monity is reached after the disputes end. I use, thus, Collins' case-study "against its grain," by using it to make an argument the study was not initially intended to make.

3.1.1.2 Interpreting scientific disputes

In the section above, I provided three case studies. Each exemplifies a case in which scientists were confronted with a plurality of theories or explanations in their field. Sometimes "integration" was preferable, at least to some of the scientists. Sometimes, "non-integration" was favored. These disputes might seem quite similar. The outcomes were, however, very different. Plurality, that is, the presence of several theories or explanations does not necessarily lead to SP.

We must also not forget how these disputes are situated in broader contexts. In many cases, a heterodox newcomer challenges an orthodox account. Such situations are necessarily asymmetrical in terms of power relations. The final decision how theories are accommodated (for example, are they integratable or non-integratable?) might involve boundary work (Gieryn, 1983), the mobilization of social capital (Bourdieu, 1986), competition for funding sources as well as disputes centering around replication and trust (Collins, 1981).

When reviewing Longino's case study, I argued that her pluralism does not sit well with practitioners' positions. Proponents of all four approaches would favor finding a way to integrate their approaches and accommodate all factors. Reduction to one theory does even seem desirable. But Longino argues for “non-integrative” pluralism. If the relevant researchers in the field welcome integration or even unification, does it make much sense to argue for non-integrational pluralism?

In the second part of this chapter, I will expand on normativity and pluralism. In the remainder of this section, I will focus on interpretations of plurality: integration or non-integration, plurality, or pluralism? I will argue that the outcome is not predetermined by the phenomenon at stake. The specific interpretation is a contingent result. I will argue that the integrability or non-integrability of two research programs or theories is the result of scientists negotiating the contents and boundaries of their disciplines (or subdisciplines). I believe that the outcome of each of the three disputes reviewed was not pre-determined by certain features of the “facts” at stake. Wouldn’t it be possible for one of Longino’s accounts to become a heterodox school because its methodologies become too remote? Was it impossible to keep parapsychological phenomena within the realm of science? Couldn’t the adaptionist and the developmental account diverge so much that their aims become so different that one cannot speak of genuine disagreement anymore?

Plurality depends on whether the relevant set of researchers recognizes a particular theory or explanation as competing or as a counterexample. Philosopher and sociologist of science David Bloor points out that “counterexamplehood” is a social achievement. Counterexamples are recognized in the same way as specific individuals were recognized as “knights” (Bloor, 1978, 274). Plurality is the result of recognizing a competing theory or explanation. Whether approaches will be integrated, remain alternatives, or eliminated from orthodoxy is negotiated within the field. I believe it is crucial to extend the literature on scientific pluralism by focusing on how scientists create plurality and how they erase it. In the following sections, I will look at this problem in detail.

3.1.2 Interlude: integrativity is not specific to biological phenomena

Although philosophers of science propose pluralist interpretations for all branches of science, some philosophers attest a unique status to biology. Sandra Mitchell argues that biological explanations are peculiar. Biological regularities are different from regularities in physics. Because biological phenomena have specific properties, biological phenomena require integrative pluralism. In this section, I aim to counter this view. I will review three arguments for the special status of biology and argue that none of these lead to the conclusion that biological phenomena *per se* give rise to specific versions of pluralism (e.g. integrative pluralism). In so doing, I will review two arguments by Sandra Mitchell and one argument by Alexander Rosenberg.

Complexity

Some philosophers argue that the intrinsic complexity of biological phenomena is the reason why biology is fundamentally different from physics. Biological phenomena have many different levels, many different causes (Mitchell, 2003, 4). I do not believe that complexity is a necessary criterion for plurality. Phenomena regarded as somewhat "simple" things can also have pluralist interpretations. For example, one might argue that the process of combustion is "simpler" than a metabolic network. This did not keep the philosopher of science Chang from formulating a pluralist position about explanations of combustion.

Complexity is also not necessary for an *integrative* pluralist interpretation. Chang showed that during the "Chemical Revolution", some actors provided integrated explanations of combustion. On the other hand, complexity is not sufficient for plurality, as many biologically complex phenomena do not have a pluralist interpretation (yet). One example might be metabolic circuits, like the Calvin Cycle or Glycolysis. Although metabolic circuits are very complex in terms of causal factors, regulatory circuits, levels of regulation, environmental influences, and fine-tuning, there was not made a case for plurality or pluralism yet. In conclusion, the "complexity" of phenomena does not determine their (pluralist) interpretation.

Contingency

Another reason why Mitchell thinks that biology has a special status within the sciences is the contingency of biological phenomena. Biological regularities are contingent in two ways. On the one hand, they are contingent because they have exceptions. On the other hand, they are contingent because their causal processes are historically contingent. "Historically contingent" means that every biological process that constitutes a regularity is itself the result of a biological process. To give an example: if it is a biological regularity that DNA is divided 50:50 amongst daughter cells, then the mechanism required for installing this separation is itself the product of a historical contingency. DNA is not necessarily separated 50:50. Certain historical contingencies caused the evolution of a mechanism that separates DNA 50:50 (2003, 116ff).

I doubt that contingency is an inherent feature of biological phenomena. Instead, I believe that the biological research tradition determines the contingency of phenomena. Historically, individual branches of sciences developed to become disciplines that focus on anomalies and variations, whereas others developed to focus on regularities. Some might incorporate counterexamples better than others. One could thus argue that "biology" as a discipline developed in a way that accommodates exceptions better.

This type of contingency is, however, not necessary for making a case for pluralism, as the example of Hasok Chang above shows. Even if chemistry admits fewer exceptions than biology, it is still possible to make a case for pluralism there. "Contingency" is, in the end, also a question of perspective. Regularities in physics, for example, are contingent on the big bang. In addition, there are several "contingent" biological phenomena that have no pluralist interpretation (again, metabolic circuits are a good example). Even if certain research areas try to account for more exceptions than others, it is not clear whether integrative or non-integrative plurality would be the outcome. In conclusion, the "contingency" of phenomena does not determine their (pluralist) interpretation.

Instrumentality

Philosopher of science Alexander Rosenberg claims that biological phenomena are particular because biology is instrumental. If (biological) organisms were fundamentally different from us, biology would look much different (Rosenberg, 1994, 5). Biology is interest-driven and implemented not only to understand living systems but to manipulate them in order to guarantee the well-being of the manipulators. Therefore, interpretations of biological phenomena might not be as global and covering because we want them to cover us. Rosenberg argues that "instrumentality" differentiates biology from chemistry or physics.

I agree with Rosenberg that the range of phenomena studied would be different if different organisms did biology. But the shift to different organisms is not even necessary for making this claim. Humans with different "styles of reasoning" (Hacking, 1983) and different sets of aims would be sufficient. Imagine Azande or Voodoo scientists. They might employ different "systems of practice." "Systems of practice" are sets of epistemic activities that are coordinated in order to fulfill specific aims (Chang, 2012, 16). Different systems of practice also exist in cutting-edge physics. Bohmists conceptualize the atom quite differently than the Copenhagen Interpretation.

Even if the interpretation of biological phenomena might be more aim-driven than that of physics, it is not true that the interpretation of physical phenomena is not aim-driven. Various projects in the realm of physics were undoubtedly driven by human interests. This was, for example, demonstrated by philosopher and sociologist Andrew Pickering's work on *Constructing Quarks* (1999).

In conclusion, instrumentality might not decide whether phenomena are pluralist, and if so, whether they are integrative or non-integrative. At most, one could conjecture that (western) biologists might favor integration over non-integration. However, this notion needs empirical testing and begs the question: *which* theories do biologists like to see integrated, and which are recognized as non-integratable? I will focus on this question in the following section.

3.1.3 Scientific pluralism and the scientists

In this section, I will investigate how plurality emerges and vanishes within research fields. I will compare plurality in small RNA inheritance with Chang's case study on the Chemical Revolution. In so doing, I will have a particular focus on how actors interpret plurality. Second, I will review Mario Biagioli's work on the anthropology of incommensurability. Biagioli's work highlights asymmetries between orthodox and heterodox parties in times of scientific discourse and considers the importance of institutional and political aims for scientific decision making. Third, I will propose a model for the trajectory of plurality within a research field.

3.1.3.1 Either integration or non-integration?

I will briefly come back to small RNA inheritance and theoretical pluralism. In the previous chapter, I have argued that small RNA inheritance supports a Lamarckian account of inheritance and evolution. This constellation produces two "camps" of researchers. One camp holds that Neo-Darwinist evolutionary theory is the only right account of evolution and inheritance. The other camp of researchers accepts Neo-Darwinist accounts, but they think that an additional theoretical account is needed for a particular set of phenomena.

This case highlights the importance of asymmetries in situations of plurality. In the case just reviewed, one group works with both accounts and the other group does not. This is not only an observation about scientific practice. One group also believes that one should not consider the Lamarckian approach. The other group thinks the Lamarckian and the Neo-Darwinian approach are not exclusive. But who is right about the relation between the two approaches? And how does the phenomenon at stake, small RNA inheritance, determine who is right?

In the example of small RNA inheritance, we find two different positions. Proponents of the one position do not accept specific causal factors proponents of the other approach propose. The proponents of this other position find all causal factors compatible. Chang's analysis of the Chemical Revolution provides similar findings. During the Chemical Revolution, there were "phlogistonists" and "oxygenists." Amongst other things, they believed in different causal entities that were causally relevant for chemical reactions. "Oxygenism" ultimately won. In the time of the Chemical Revolution, some practitioners included both "phlogiston" and "oxygen" in their explanations. They were thus also "dualists." In the case of the Chemical Revolution, all "dualists" became "singularists" after a certain time, and the die-hard phlogistonists "died out." But what happens if all "singularists" become "dualists" after a certain time? Can we still detect plurality? Can we still talk about pluralism if everyone agrees about integration? I will try to address this issue in the sections to follow

3.1.3.2 Biagioli – The Anthropology of Incommensurability

In "The Anthropology of Incommensurability," Mario Biagioli studies how two paradigms "become" incommensurable (Biagioli, 1990). Usually, the meaning of the term paradigm does not allow for the use of the word "become." Two paradigms *are* incommensurable or the systems at stake are not paradigms. But paradigms do not *become* incommensurable. In many cases of scientific pluralism, one might not even speak of paradigms. "Different theories" or "different explanations" are much more often in focus. "Different theories" or "different explanations" can, however, become focal aspects of emerging paradigms. Paradigms are no stable, abstract entities but constantly negotiated systems that incorporate theories, explanations, values, activities, and institutions.

Biagioli criticizes the traditional notion of paradigm and Kuhn's emphasis on meaning incommensurability. He argues that

> "while incommensurability is a specific and fairly uncommon linguistic phenomenon in science, non-communicative behaviours, rhetorical strategies of non-dialogue, and strategic uses of bilingualism are common, non-specific phenomena related more directly to inter-group dynamics than to lexical structures" (1990, 208).

The "traditional" Kuhnian meaning of incommensurability can be a particular outcome that is produced by various processes to position institutional and professional standings (1990, 187).

Biagioli characterizes episodes of plurality as struggles for (institutional) power. In most cases, "orthodoxy" aims to defend its authority, whereas "invaders" try to find a place within the reward system – funding or other forms of capital (1990, 191). As an additional factor, the maintenance of socio-professional or disciplinary boundaries is of crucial importance.

In that spirit, Biagioli uses a "Darwinian metaphor" to characterize the power struggles between different paradigms: paradigms are not different conceptual schemes, but they are inhabited by different "species of practitioners" (1990, 186). Paradigm change is the point when the different species of practitioners cannot interact anymore – they became "sterile." To vanish from existence, a paradigm need not necessarily "die out," it need not be entirely rejected. Rather, the theory becomes unfit: it drops out of the reward system of science (1990, 184f).

Notions about the respective incommensurability of two competing paradigms often lack symmetry. For example, proponents of one paradigm might blame proponents of the other for being incapable of understanding their theorems. At the same time, these proponents might claim that they are perfectly capable of understanding the concepts of the adverse paradigm. Biagioli argues that newcomers often make use of this "bilingualism" to force their way into the funding systems of orthodoxy. They use the language of orthodoxy without adopting the associated identity (1990, 205). Bilingualism thus indicates power structure. While the invader has to learn the other language (or was brought up in it), the authoritative dogma need not or cannot learn the invader's language (1990, 209). Thus, proponents of the ruling paradigm might often claim meaning incommensurability. They do not need to or do not want to engage with the other approach (1990, 185).

Illegitimate trespassing of disciplinary or socio-professional boundaries often causes the breakdown of communication. For example, Galileo was rejected by the Aristotelians for talking about philosophical phenomena because he was "only" a mathematician. Galileo did not only trespass a disciplinary boundary but also a boundary of social class. Others might have reasons to downplay incommensurability in order to preserve group identity. Biagioli points to the example of Copernican astronomers who advanced radically different views. However, to preserve identity and defend themselves to the outside, they abstained from any claims of incommensurability and did not engage in strategies to provoke a breakdown of communication (1990, 186). The breakdown or maintenance of communication between two (opposing) views is thus the result of power struggles of several groups to keep specific programs out of funding and to keep socio-professional cohesion in the inside.

In conclusion, the interpretation of plurality, that is, whether accounts are integratable, non-integratable, or even "incommensurable," is the result of boundary work, the positioning of political and institutional aims, and the facilitation or break-down of communication. "Strategy" is also a critical factor. "Monism" or absence of dispute within a group can also be a strategy to preserve plurality in the inside. Plurality might often be considered a hallmark of instability and lack of power. Thus, pluralism might be a rhetorical problem for orthodoxy. Monism or "singularism" of orthodoxy might not mirror the diversity of theoretical accounts that comprise orthodoxy. In conclusion, plurality and monity, singularism and dualism, pluralism, and monism are determined by negotiations of different "species of practitioners."

3.1.3.3 Towards a model of plurality in the sciences

In the last sections, I discussed the reasons for favoring particular interpretations of plurality. I argued that it is a contingent matter whether two theories are recognized (in the Bloorian sense) as incommensurable or not. Breakdown of communication is brought about by groups of scientists as an outcome of the positioning of institutional aims. In this section, I would like to propose a processual model to describe the flow of these decisions (Fig. 17).

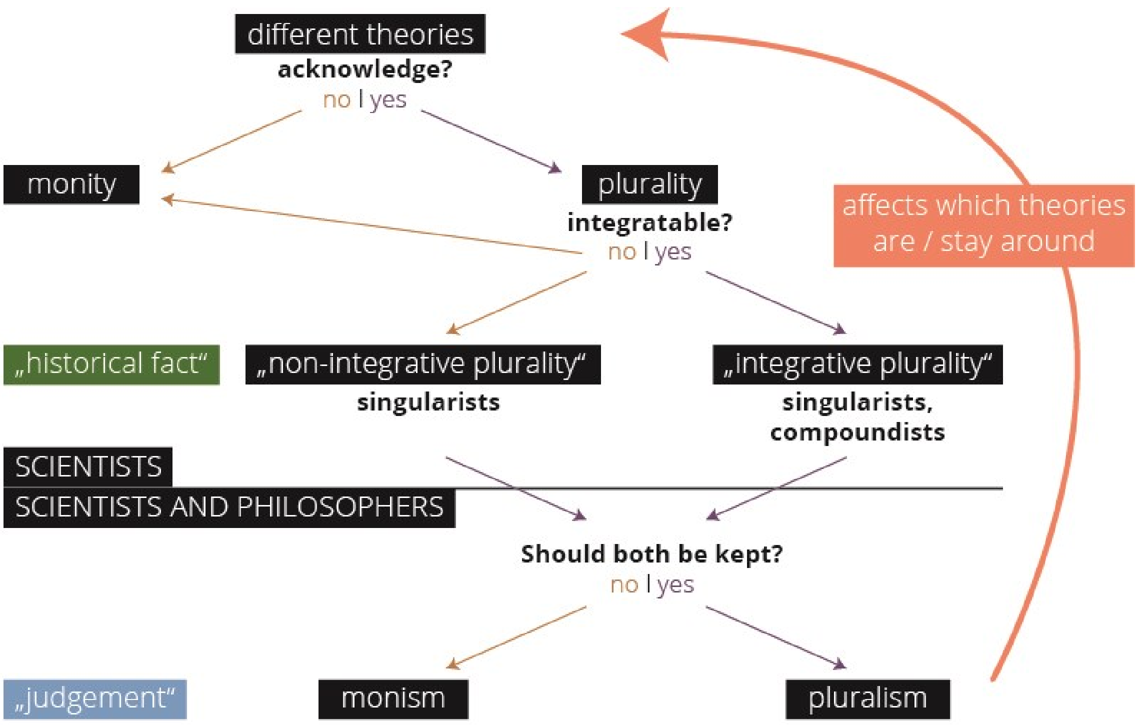

Figure 17 Decision tree for scientists and philosophers after being exposed to a plurality of theories.

The proposed "pathway" has to be interpreted quantitatively. For each decision, some scientists will say "yes" and some will say "no." Claims of plurality, incommensurability, integrability, non-integrability, etc. are often non-symmetrical. The proportion of researchers answering “yes” or “no” will define the details of every analyzed dispute. The public form of the consensus will reflect the power relations between those who say “yes” and those who say “no.”

The starting point of the decision tree is a situation in which more than one theory is around. Naturally, one theory would be “orthodox,” whereas others are “invaders.” “Being around” in a broad sense is not good enough. Orthodoxy needs to “recognize” invader theories as causing problems. For instance, a particular experiment could attract the relevant attention. In the case of plant response to remote stimuli, one experiment performed by parapsychologists had sufficient traction to cause disputes within orthodoxy. A crisis of explanatory power of the orthodox theory could make scientists more attentive towards other approaches. In conclusion, theories need to be recognized as challenging orthodoxy to complete the first step.

In most examples of scientific “revolutions,” the answer whether the invading account has to be acknowledged eventually turned into “yes”. The answer, however, is quite often “no.” The current versions of creationist and intelligent design approaches have been around for decades but were never recognized as alternatives to evolutionary theory. Creationists would undoubtedly argue that they provide a valid alternative and are wrongfully marginalized. Such examples illustrate my claim about the quantitative character and asymmetry of such decisions. In this, as in many other cases, we would have “one-sided” plurality, meaning that one “species of practitioners” recognizes plurality, whereas the other does not. This also reflects power structures. Anti-creationists might argue that they “need not” engage with creationism. Creationists lack that sort of power and need to speak the language of orthodoxy in certain situations in order to criticize orthodoxy’s theories. They might even argue that they do not require a rejection of orthodox evolutionary theory but want to be accepted as well. In a sense, they would be strategically pluralist.

The next step is to decide how to arrange and relate to the different approaches. One way is to try to keep only one theory. For example, orthodoxy might want to get back to being the only theory. The same might be true for newcomers. Instances that are called “paradigm-changes” or “revolutions” offer examples of newcomers taking the place of orthodoxy after a period of plurality. Collins’s example is one where the orthodox theory manages to exclude the newcomer after a certain amount of time. For the theory that is losing out, it might be strategic to argue for integration as a last resort. I take “at a loss” to mean various things, from fitting worse with the evidence to being not favored by the relevant monarch that provides money.

If not all theories are successfully expelled from the relevant reward system, another question concerning the relation of the two theories needs to be asked. Are they integratable or non-integratable? Treating different theories as non-integratable could be a means of preserving the identities of groups of researchers. Treating theories as integratable or at least non-contradictory might serve to present a particular research enterprise as stable to the outside. Bilingualism or integrability might, however, always be considered a lack of power. Also, claims of integrability could be a means of forcing one’s way into orthodoxy. Non-integrationism could be a way of keeping other theories away from specific funding opportunities.

Whether accounts are integratable or non-integratable can be judged differently by different researchers. In Mitchell’s case, some scientists made claims about the integrativity of the adaptational and the developmental explanations, whereas others considered them non-integratable. These judgments might change over time. If, for example, claims of integrativity or bilingualism are used to enter the reward system, once the reward system is entered, they might be changed. Over time, periods of integrative or non-integrative plurality might return to a state of monity. In other cases, plurality might remain. It is important to point out that the decision tree does not indicate a temporal and irreversible flow.

The next step of the process is usually the step where philosophers of science participate. Both scientists and philosophers might try to answer the question of whether a state of “non-integrative plurality” or “integrative plurality” is beneficial or should be maintained. Arguing for its benefits is pluralism, rejection is monism. Again, it is important to remember that the attitude scientists have towards pluralism or monism determines which theories are recognized as possible alternatives. For instance, if scientists adopt a pluralist viewpoint, they might claim even more theories should be available. This is also true vice versa: if a substantial part of scientists aims at eradicating plurality, then one might enforce strategies to not let alternative theories gain ground in the first place. In addition, as pointed out above, such decisions might not be fully symmetrical, as most scientific enterprises accommodate influential hierarchical structures along with enclaves that foster innovation.

Concerning these decision processes, one question becomes pressing. What impact can or should the philosophical judgment have? Should the philosopher aim to influence these decision processes, for example, to enable pluralism? In this section, I focused on how situations of plurality come about. In the remainder of this chapter, I will review scientific pluralism as a normative, philosophical position.

3.2 How to be a good normative pluralist

Normativity is a central issue for the scientific pluralist. If philosophers also endorse scientific pluralism (SP) as a normative goal, they will require that science should become pluralist. SP often entails policies to change science. One thus needs to ask: how can the philosophers' armchair judgments be implemented in the research field? Can the philosopher persuade scientists at all? How much data on the benefits of SP does one actually need in order to propose SP as a normative goal?

There are three different ways of motivating the pluralist argument. Scientific pluralists usually pick one or more of these ways: conducting an empirical (sociological) case study, armchair analyzing the current situation of a specific research field, or conducting a historical case study. Such case studies can have (A) two different outcomes and (B) two different levels of generality:

A) The outcome of the case study can either be positive or negative: SP was/is/would be a desirable status. SP was/is/would not be a desirable status.

B) The generality of the conclusion can also differ. Either, the conclusion is limited to the scope of the case study, or it goes beyond this scope. It is possible to claim that because of case study X in field X, field X should become pluralist. It is also possible to claim that because of case study Y in field Y, all scientific fields should become pluralist.

In what follows, I will discuss several problems that arise with A) and B) and normative scientific pluralism in particular.

3.2.1 The justification of global normative SP

Scientific pluralism (SP) comes in different flavors. One key differentiation is whether SP is “descriptive” or “normative.” “Normative pluralism” requires a research field to remain or become pluralist. If a research field is in a phase of plurality, this plurality should be preserved. If a field lacks plurality, more theories and explanations should be introduced. Thus, the predicate “normative” rests on two attitudes. One is the value judgment that pluralism is always beneficial. The other is the demand that fields should become pluralist because pluralism is beneficial.

Descriptive SP takes a different standpoint. Descriptive pluralists take plurality as their starting position. They point to existing plurality in a specific branch of science or science as a whole and argue that this situation is beneficial. Often, one aim of descriptive SP is to raise awareness for plurality in various disciplines. Another aim of descriptive SP is to provide arguments against reductionism or foundationalism by pointing to the benefits of the observed plurality. It is a crucial claim of descriptive SP that the observed plurality is beneficial. Descriptive SP is often mischaracterized as a position that is not normative, for example, by Hasok Chang (Chang, 2020). The descriptive pluralist inevitably is always already more than purely descriptive. At least, it almost invariably contains an evaluative component. And it is a short step from this perhaps not fully articulated evaluative component to a normative position. The difference of “normative” and “descriptive” pluralism lies, therefore, in the respective agenda and not in the presence or absence of normativity. The terms “descriptive” and “normative” do not constitute a dichotomy in this particular context.

It is, however, also possible to introduce a more fine-grained distinction, that considers three different attitudes:

A) descriptive SP which incorporates statements of the form “S is p,”

B) evaluative SP which incorporates statements of the form “It is good that S is p,” and

C) normative SP which incorporates statements of the form "S should be p;"

Adopting this qualification, the transition from evaluative to normative SP differentiates the positions that were traditionally coined "descriptive" and "normative" positions. Following this classification, "descriptive" pluralism is descriptive and evaluative, but not normative.

In the remainder of this section, I will focus on the normative pluralist's agenda. This agenda can be "global" or "local." Global normative SP requires all areas of science to host various different theoretical accounts. Local normative SP is restricted to a specific area of interest, and in most cases, normative claims are supported by a case study. One aspect of the normative pluralists' position is, however, unclear. Should the field display plurality only for an infinite amount of time? Should researchers be pluralists as well? Preserving plurality in a field might be sufficient, however making scientists become pluralist would provide better chances for sustained plurality. Such questions are rarely asked and, if at all, they not excessively addressed in the scientific pluralism literature. Although the term "pluralist field" is often used in the literature, the meaning of this term is not well defined.

"Global" SP heralds another problem, namely the necessary support for a global pluralist claim. What would constitute a convincing argument that each and every scientific field should become pluralist? Naturally, it is impossible to provide a case study for all scientific research projects, past, present, and future, to support global pluralism. Global pluralism, thus, is often developed from a local case study. For example, Chang's account of the Chemical Revolution provides the necessary basis for developing local, normative pluralism concerning "phlogiston" and "oxygen." Guided by this case study, Chang distills the benefits of tolerant and interactive pluralism. Based on these benefits, he makes a case for global pluralism. Because pluralism would have been beneficial in the Chemical Revolution and because the benefits of SP apply to all research fields, every research field should become pluralist. Thus, most global normative pluralist arguments proceed in two steps: 1) Establishing pluralism for one case study. 2) Providing reasons why extrapolation of this case study is possible.

Another differentiation concerning global pluralism is necessary: whether SP is motivated epistemologically or metaphysically. Chang's global normative pluralism is epistemologically motivated since he claims that adopting pluralism in all fields of science would increase our knowledge. The burden of proof lies, thus, with Chang to show that the benefits of pluralism are possible to obtain in every possible scenario. Metaphysical normative pluralism usually lacks the transition from a "local" to a "global" argument. Most metaphysical pluralists are concerned with the nature of the world and not with one specific science in particular. Thus, they start with a "global" argument. Even if metaphysical pluralists provide case studies, these are pushed into the service of supporting their global claims but do not motivate the global claim in the first place. A global metaphysical pluralist argues for SP by referring to the complexity, or disunity, or "dappledness" of nature. In these cases, normative claims follow from a prior commitment or intuition. It is important to note, however, that metaphysical SP does not entail normative SP. It would, for example, be possible to argue that although the nature of the world is disunified, human inquiry should nevertheless be monist, as monism is the better way of gaining knowledge.

But can there be a convincing argument for global pluralism? This question is a difficult one, for pluralism is always connected with a value judgment. Card-carrying scientific pluralists value plurality. Chang's benefits of tolerance make it even more explicit: we should be delighted by pluralism. This leaves open the possibility that pluralism is a matter of taste.

Rather than asking how to justify global normative SP, one could also ask for counterarguments against global normative SP. What constitutes a case study that leads to the rejection of global SP? One way would be to offer a competing account of a specific case study that does not support a pluralist conclusion. In the second part of this dissertation, I will review such an instance. Another possibility for challenging global pluralist claims would be to produce a case study that shows that plurality was not beneficial at a certain point. One possibility would be to look for examples where plurality was hindering scientific progress. How would a pluralist respond to such claims?

On the one hand, she could claim that the plurality was erased too early so that no benefits could be seized. Or she could argue that there was not a sufficient number of proponents to support a specific theory, so it could not be developed in a way to add to the benefits of pluralism. Another way to counter such arguments would be to side-step them and point to one of Chang's benefits of "tolerant pluralism": "epistemic abundance." Chang argues that we should be delighted by epistemic abundance. Taking "epistemic abundance" seriously, all instances of plurality are beneficial because plurality is delightful. Of course, there is a certain circularity to that argument. It is thus not clear how a convincing case for global normative SP should be made, as case studies are not sufficient and other arguments are based on a prior commitment to the benefits of SP.

There is yet another set of problems that concerns both global and local normative SP. Most accounts on SP are presented from the perspective of philosophers and do not take into account whether SP is a position that presents itself appealing, or at least acceptable, to scientists. If SP is to be a prescription for science, this question would, however, be worthwhile answering. Let me give one intuitive reason why: scientific pluralism is not a scientist's default position.

I will treat this assumption as a working hypothesis. It is based on several reasons. Science textbooks and the way science is taught and presented in schools and universities provide us with one clue. In these contexts, the history of science is usually framed as a story about the search for one true theory. Episodes in which plurality was prevalent are not portrayed in a positive light, but plurality is described as a problem that had to be overcome. Episodes in which one theory superseded the other are described as critical moments in the search for truth. Usually it is not discussed whether there would have been benefits in preserving the old and the new theory. Most of the time, cutting edge examples of plurality are taught as problems and not as examples of a desirable state in science. Being confronted with a plurality of theories is instead interpreted as a sign of immaturity.

The reasons discussed in the last paragraph motivate my hypothesis that SP is not a very common position in science. Imagine telling a scientist that we should introduce another, radically different theory to her field of study. She will not agree that this is a good thing to do. And "epistemic abundance" might not delight her that much. This results in problems for both global and local SP, as also case studies that might appear convincing to scientific pluralists might not convince scientists. While I believe that global normative SP is not feasible, I believe that local, normative SP is possible. It is thus one primary goal of this dissertation to provide a sociological case study on plurality in a research field in order to develop a local, normative claim on plurality that considers researchers' aims and goals as well as their stances towards plurality.

3.2.2 Pluralism and plurality within science

The fact that SP is not a part of scientists' day-to-day "ideologies" invokes another problem. In the last section, I argued that practitioners might not agree with the claim that pluralism is beneficial or desirable. On the contrary, following the most outspoken advocates of science such as Steven Weinberg or Richard Dawkins, monism might instead be the prevalent ideology within science.

It is, however, necessary to add a qualification to my claim that "monism" might be the dominant position within the sciences. Although monism might appear to be the household or go-to position of a practicing scientist, the scientist's monism should not be equated with monism as a position in the philosophy of science. For differentiation, I claim that a "monist intuition" is prevalent among scientists. I characterize this monist intuition as a set of commitments like: "If the data I produce can be explained by more than one account, then at least one of them is wrong"; "It is the goal of science to find out which of them is true or whether both are false."

I insist on a "monist intuition," rather than on a monist position to deny the prevalence of firm metaphysical or epistemological commitments of the practicing scientist concerning monism or pluralism. Ideas like: "The world is such that it admits for only one theory to explain one specific phenomenon"; or "Knowledge can only be attained by singling out one theory from all the others" might well not be part of science shoptalk. Also, such ideas are not usually found in scientific publications but only in textbooks or monographs of scientists that reflect on their discipline. Thus, monism is often made to fit the scientific enterprise post hoc. This idea is inspired by Michael Lynch's "Art and Artifact in Laboratory Science" (1985). Most products of scientific experiments or theorizing are not planned or designed in a way to support a specific (philosophical) position. Instead, they are designed in a way that fit into narratives that are told long after experiments have ended. For monism this would mean that no experiment is conducted in order to verify (or falsify) monism. Instead, experiments are designed in a way so that the outcomes can later be used to support epistemological positions.

Accepting the claim of the last paragraph, it follows that if we describe scientists as monists or pluralists, we do this by imposing philosophical categories on their commitments. These categories are, however, not part of their theoretical or practical assumptions. It is, of course, an exciting exercise to reconstruct philosophical positions within science but it requires care and reflexivity. In his case study, Chang characterizes Priestley and other phlogistonists as "pluralists," whereas Lavoisier is criticized for being a ruthless "monist." The question is, however, what does it mean in this example if scientists are called "pluralists" and "monists"? If they are pluralists, do they accept some necessary and sufficient criteria for SP? And if they are monists, do they reject the criteria? Are there sufficient sources to conclude that Priestley was a pluralist or not? Philosophies that are prevalent within scientific communities often remain a black box and philosophers prefer to use the concepts they are familiar with in order to describe the positions of scientists. However, if pluralism should be a doctrine that governs science, we need to assess the resonance of positions in the philosophy of science and those held by scientists.

Such an analysis, however, is missing in the literature. There is hardly any scholarship that brings together the philosophical debate on SP and the question of which positions researchers (implicitly) take concerning these debates. It is one core goal of this dissertation to reconstruct the implicit philosophies of researchers working in the field and assess them with respect to the philosophical positions of pluralism and monism.

3.2.3 Pluralism, stability, and dynamics of a research field

One last problem associated with SP and the sciences is that of heterogeneity within a research field. In the last section, I asked what it means for a scientist to qualify as a pluralist or a monist. Usually, if two positions represent two components of a dichotomy, we expect a philosopher to accept either the one or the other but not both. We expect philosophers to be either monists or pluralists, either absolutists or relativists, either nominalists or essentialists. Given the fact that these terms are not "native" to the empirical sciences, should we expect the same for scientists?

In the current literature on SP, the answer to this question is not clear. It seems, though, that several philosophers describe scientists as if they were either pluralists or monists, as shows, for example, Chang's case study on phlogiston or Longino's case study on explanations of behavior. Neither of them defines what it means for a scientist to be a pluralist. What they reconstruct is how scientists respond to plurality. This, however, is not enough to classify them as pluralists. What their description shows is that these "pluralist" scientists are conscious of plurality and, in turn, "accept" plurality as the state of affairs. Whether scientists are conscious of plurality is not sufficient for pluralism. Also, monists need to accept a state of plurality and provide an interpretation if confronted with it. A monist cannot expect "monity" as the only possible conformation of scientific inquiry. The history of science teaches otherwise. Accepting and settling with a current state of plurality is not sufficient to qualify as a pluralist.

We might thus question whether pluralism and monism are the only positions scientists take towards plurality. Should we expect more heterogeneity in research fields? At least, we should not presuppose that the strict criteria for SP and monism apply to science. In addition, there is another aspect of SP in science which I already mentioned in my discussion of Mario Biagioli's work on the anthropology of incommensurability. Biagioli characterized pluralism and monism as rhetoric strategies. Even if researchers seem committed to pluralism or monism, this might not necessarily reflect their philosophical positions but rather their strategies to promote their interests. Besides rhetoric strategies, there might also be other layers of embracing a (partly) pluralist or monist position.

Thus, the positions of scientists might be mischaracterized when classified as either monist or pluralist. What is the spectrum of possible positions concerning plurality within a scientific field? Which positions are taken by relevant actors? What is considered beneficial? What is considered harmful? What are the aims and goals in respect of plurality? What are the reasons for treating theories as integratable or as non-integratable? It will thus be one key goal to emphasize heterogeneities within the research field. This should provide scientific pluralists with a refined framework and toolbox in order to assess what is at stake in their respective fields of interest. Also, the dynamics of a research field that displays plurality will be highlighted.

3.2.4 Setting out the goals of a sociological case study

In this dissertation, I will discuss issues that scientific pluralists so far have left unmentioned. In the next chapter, I will provide an analysis of the small RNA inheritance research field. I will present reasons why researchers favor, or do not favor, pluralism. Also, I will investigate which types of problems a field that displays plurality faces. I will argue that such issues are significant factors defining the trajectory of the field. In the next paragraphs, I will suggest a normative pluralist approach that pays attention to such considerations.

A normative pluralist approach should employ cutting-edge case studies. I believe that historical or hypothetical case studies are not enough to provide arguments on why a particular field should be changed. Of course, the philosopher's work might start in the armchair in order to identify a research field that appears to embrace plurality. After having gained the first intuition, the pluralist should engage with the actors in the field. A normative pluralist approach should pay attention to several levels of plurality. This makes it possible to distinguish the interior structure of a field from how it presents itself to the outside. Several questions need to be answered: how does a field present itself to the outside: pluralist, monist, exposing plurality? What is at stake at the inside: what is considered beneficial? What is considered problematic? How heterogeneous is the core-set of investigators concerning their takes on plurality? To conduct this analysis, it is pivotal to acknowledge that "pluralism" is not an actor's category and monism is neither. One, therefore, needs to diligently dissect the state of the field and interpretations of the state of the field (by practitioners and philosophers).

Such an analysis will enable the scientific pluralist to differentiate between three different phenomena/notions: SP as a philosophical position, the plurality of a research field, and the practitioners' interpretation of plurality. Only by accounting for these three factors, scientific pluralists can provide a normative judgment that integrates perspectives of researchers.

I submit that an approach that includes the perspectives of researchers and pays attention to processes and dynamics within the research field can enable historians and philosophers of science to influence research fields. It does no good to "claim" pluralism for a field without knowing how the field conceives of plurality. A field is not pluralist unless its practitioners are pluralists. There are many spaces a philosopher can occupy within scientific practice. She can fill these spaces by the acquisition of “interactional expertise.” I do not think that this demand is too high, as most branches of philosophy of science require philosophers to have a profound understanding of the science they are studying. The combination of armchair notions about pluralism, interactional expertise, and sociological case studies will enable the scientific pluralist to make normative claims which have the potential of changing the research field.

4 Testing Scientific Pluralism in Small RNA Inheritance

In this chapter, I will combine issues discussed in Chapter Two and Chapter Three. I will take the questions asked about the normativity of pluralism and scientists' interpretations of pluralism and apply them to the case of small RNA inheritance. This chapter is divided into four sections. In the first section, I will provide an outline of the goals of this chapter. Furthermore, I will motivate my approach by locating my work within the recent "practice turn" in philosophy of science. In the second section, I will survey methods and concepts employed in this case study. In the third section, I will present the results of my empirical case study. In the fourth section, I will present the interpretation of my findings.

4.1 Outline

In this section, I will formulate the goals of this chapter. Besides addressing several general and specific problems in the philosophy of science in the light of my case study, two primary aims, which are novel contributions, are: 1) To reconstruct positions held by scientists within the field of small RNA biology concerning their takes on plurality and pluralism. 2) To describe the dynamics of a research field that is confronted with plurality. In so doing, I aim to offer a perspective on what is at stake for pluralism in research fields. Having provided a discussion of the goals of the chapter I will discuss the promise of empirical philosophy of science in more general terms and locate the employed approach with the tradition of the "practice turn" in philosophy of science.

4.1.1 Goals of this chapter

Given the problems discussed in the previous chapter, several goals emerge for conducting a sociological case study on small RNA inheritance. Pluralism in the sciences needs to be assessed, that is, tested by a case study that highlights the attitudes of practicing scientists and pays attention to dynamics encountered in research fields. Besides establishing the benefits of such case studies for the philosophy of science in general, I intend to meet three specific goals.

1. The first aim is to reconstruct the positions of researchers towards SP. I will ask how scientists interpret plurality in their field and whether they even perceive the attested

plurality as plurality. I will discuss possible positions researchers could take and contrast them with positions that were found in the field. I will characterize their commitments trying to find attitudes that match positions found in the philosophy of science, for example, “epistemological” vs. “metaphysical” commitments. But I will also try to distill new positions that are not yet part of scientific pluralism literature.

2. Having reconstructed philosophical positions within a research field, I aim to develop a toolbox in order to describe these positions. In so doing, I aim at developing a conceptual toolbox to account for the heterogeneity within the research field. This toolbox will primarily make use of analysts’ categories and is intended to facilitate the assessment of case studies for philosophers. Categories used in the toolbox will be singularism vs. dualism, integrationism vs. non-integrationism, pluralism vs. monism, epistemic motivations vs. metaphysic motivations, etc.

3. The third aim is to describe a research field that displays plurality. For this aim, Biagioli’s anthropology of incommensurability will come in handy. For my case study, I selected small RNA inheritance. Small RNA inheritance researchers produce evidence which they present as challenging a ruling dogma. Proponents of the field claim that another theoretical account besides that of the ruling dogma is required. When challenging orthodoxy, struggles for power, funding, and acknowledgment are to be expected. Who is considered an enemy or critic of the field and which reasons for the perceived opposition are invoked? How do practitioners describe their field in terms of its stability and resilience to criticism? What is conceived as dangerous for the field? Which goals are considered important? Who is considered to be part of the field and who is not? How do accounts of membership vary amongst practitioners? How is trust divided across the field? Are there differences concerning the representation to the outside and actual structures and dynamics on the inside? The goal is to provide philosophers of science with an account of what is at stake in a field displaying plurality and what the trajectories of SP in this field are.

4.1.2 Empirical philosophy and the practice turn

The "practice turn" in philosophy of science is usually dated back to the 1980s and 1990s, heralded by hallmark publications such as Ian Hacking's *Representing and Intervening* (1983) and Andrew Pickering's *Science as Practice and Culture* (1992). The "practice turn" invoked a major conceptual shift in philosophy, sociology, and history of science by introducing an emphasis on the practices of scientists (Soler et al., 2014, 14). In her editorial introduction to the volume *Science after the Practice Turn in the Philosophy, History, and Social Studies of Science,* Lena Soler discussed five significant shifts in the philosophy of science caused by the practice turn.

The first change consists in turning away from a priori, idealized accounts of science and rigid notions of "the scientific method," "verificationism," "falsificationism," and "scientific progress." Proponents of the practice turn emphasize that we cannot take scientists' aims and motivations for granted. Instead, advocates of the practice turn encourage direct engagement with the scientific field in order to uncover these motivations and aims. In short, the program of the practice turn encouraged philosophers to focus on scientific practices themselves, rather than on the products of scientific practices.

The second change involves a shift away from traditional normative approaches. Proponents of the practice turn criticize normative approaches for failing to adequately describe scientific practice and plead for descriptive studies to capture more adequately what is at stake in the research field. Proponents of the practice turn, however, never dispended from normativity but introduced a different, ideally better, normative approach. Normative approaches after the "practice turn" ground recommendations and criticisms on empirical work on scientific practice and not on a priori commitments (Lynch, 2014, 103). The practice turn brought with it an oscillation between the normative to the descriptive.

The third change concerns historical studies of science. Proponents of the practice turn criticize traditional case studies in the history of science for being present-centered, rationalized reconstructions of past scientific endeavors. The practice turn advocates a contextualization of scientific situations in their historical setting.

This advocacy is also a component of the fourth change: “From Decontextualized, Intellectual, Explicit, Individual, and Purely Cognitive to Contextualized, Material, Tacit, Collective, and Psycho-Social Characterizations of Science” (Soler et al., 2014, 17) which can be observed in “finely contextualized micro-studies of science” (ibid.). Practice-turn approaches are characterized as interdisciplinary in that they combine philosophy, sociology, and history of science. One of the aims of the micro-studies carried out in this spirit is to reveal overlooked aspects of research and their epistemological relevance. Such overlooked aspects are, amongst others, physical materials, tacit knowledge, skills, economic and institutional settings, and scientists’ background values or beliefs.

The fifth change follows up on the emphasis on overlooked aspects of research by encouraging a shift from scientific *products* to scientific *processes* (2014, 21-22). This does not mean that scientific products, such as theories or data, are ignored. These objects are taken to be unstable and as constantly in the making. Soler describes this change as a shift of perspectives from science as contemplation and representation of the world, towards science as intervention, creation and transformation of the world (2014, 22-24). After Hacking influential *Representing and Intervening*, representations were no longer considered to be simply descriptions of the world; instead, they are understood as “means of doing things, tools for intervening, and material artifacts for transforming the world” (2014, 23).

Chang is also considered a proponent of the practice turn because of his focus on studying science “in terms of activities” (Chang, 2012, 18). Chang defines “systems of practice” as units of study. These comprise various “epistemic activities” which are “coherent set of mental or physical actions (or operations) that are intended to contribute to the production or improvement of knowledge in a particular way, in accordance with some discernible rules (though the rules may be unarticulated)” (2012, 209).

Epistemic activities are intentional and coordinated with other epistemic activities. Successful coordination of several epistemic activities constitutes a “system of practice” (Chang, 2014, 72). Chang employs three different levels of analysis in his framework: 1) the system of practice; 2) individual epistemic activities; 3) mental and physical operations. It is the philosopher-analyst who determines which aspect is in focus; this is not thought to be fixed by inherent qualities of the scientific endeavor under examination (Soler & Catinaud, 2014, 82).

The listed five changes brought with them a rethinking of methodology. Whereas the "products" of science were traditionally studied "from the armchair," attention for science as a process, for the unarticulated, tacit aspects, called for a different methodology. Empirical methods were introduced into the philosophy of science in order to meet the goal of studying science as a process (Wagenknecht et al., 2015, 15).

In the aftermath of the practice turn, empirical case studies have become common in history, philosophy, and sociology of science. There are primarily three different empirical approaches: the historical, the observational (ethnographic), and the experimental (or statistical) (Osbeck & Nersessian, 2015, 14). "Case studies are concerned with scientific work carried out during a limited time period and are usually restricted to a specified set of scientists, institutions, laboratories, disciplines, or traditions" (Burian, 2001, 384f). Topics of case studies are usually relatively narrow and are organized around a focal issue of interest. Case study-based research is open-ended as the boundary between the object of study and its context is variable throughout the process of study.

While case studies fit well with the aims of the practice turn, the case study method "cannot and should not be expected to yield universal methodologies or epistemologies" (2001, 399f). To explain both micro- and macro-scale aspects of science, the messy details unearthed by case studies are essential. Case studies make scientific practice visible. Case studies offer concrete, context-dependent knowledge. To proponents of the practice turn, context-dependence is not a disadvantage. While sweeping generalizations cannot be attained through case studies (Flyvbjerg, 2006, 8), they nevertheless can provide "exemplification" and falsify expectations and preconceived views. Rather than perpetuating the investigators' biases, case studies most often lead to questioning and reflecting on preconceived notions (2006, 23).

I can now examine the premises of my empirical case study concerning the characteristics of the practice turn. My research question is open-ended in the sense that I do not presuppose a pre-conceived, idealized version of scientific pluralism. Instead, I investigate and reconstruct whether and how scientific pluralism is part of the field and present in scientists' accounts. By comparing results with canonical texts on scientific pluralism, I aim to challenge certain pre-conceptions about pluralism and to examine the promise and trajectory of plurality in the sciences. Through conducting my case study, I also corrected preconceptions I had myself about pluralism in general and about pluralism in small RNA inheritance in particular.

The "normative–descriptive–normative" characteristic of the practice turn is in line with my case study. While I am against imposing a normative framework upon the research field I study, I use a descriptive approach in order to put forward qualified and restricted normative claims. In so doing, I provide criticism of other versions of scientific pluralism that fall back into formulating prescriptions for science that are not sufficiently informed by the new descriptive-normative approach. Along similar lines, I provide a contextualized account of a research field. An account of research traditions, model organisms, "repertoires," and the social organization of the field will be central. Thus, my case study is concerned with both products and processes. Specifically, I investigate the roles of these products and processes in different settings. On the one hand, I study the processes of negotiating these products within the research field. On the other hand, I examine how processes are turned into ready-made products in order to represent the field to the outside. I emphasize the situatedness of scientific products. In other words, I study how researchers make these products fit certain discussions.

4.2 Materials & methods

In this section, I will discuss the concepts and methods applied in my case study on scientific pluralism (SP) in small RNA inheritance. First, I will provide an overview of how interview partners and laboratories were selected, how observations were conducted, and how interviews were designed. I will also discuss the methods of analysis and coding. In the second part of this section, I will introduce two approaches for interpreting the dynamics of the studied research field: boundary work and grid/group analysis. Both approaches have their origin in sociology/cultural anthropology but have been applied by historians and philosophers of science. Thus, the second part of this section will provide a conceptual framing whose implementation will be a result of the analysis. These approaches will be “tested” for their applicability to the specific case study and at the same time the ways they apply to the case study will be one result.

One question that permeates the thoughts in this section is: how should we define a scientific research field? How are the boundaries of a discipline defined, both by analysists and by researchers within the discipline? Who is in and who is out? What is the socio-professional identity that defines one as a member, and what are the reasons for being expelled? Naturally, these questions are relevant when designing the case study as an account of who is part of the field decides who will be interviewed. How the interviewed actors define their field and how they reflect on it will be a result of the case study. Different definitions of the boundaries of the research field, provided by different interview partners, could, for example, be taken as a response to plurality within the field.

As mentioned above, two core concepts will structure my theoretical approach: “boundary work,” developed by Thomas Gieryn (Gieryn, 1983), and “grid/group analysis,” developed by Mary Douglas (Douglas, 1970/2013). Specifically, I will review the application of grid/group analysis of research communities by David Bloor (Bloor & Bloor, 1982/2013; Bloor, 1978). Also, Biagioli’s work on the anthropology of incommensurability (Biagioli, 1990) will be discussed in light of the other approaches introduced.

Before commencing with the methodological discussion, let me provide a fairly extensive section on methodology. I consider this case study on scientific pluralism in small RNA inheritance to be a contribution to and extension of “empirical philosophy of science” established by Paul Griffiths and Karola Stotz (Stotz & Griffiths, 2004). Griffiths and Stotz make use of quantitative methods exclusively, primarily questionnaires. I aim to show how qualitative methods can contribute to the endeavor of empirical philosophy.

My project is naturally seen as interdisciplinary, for it employs methods, concepts, aims, and goals from different disciplinary origins. The core aim is to test a philosophical concept (scientific pluralism) for its resonance within a research field that is considered to display plurality (small RNA inheritance research). If an “empirical” turn in the philosophy of science is to be successful, then methodological standards of working with empirical data need to be introduced. One of such standards is to provide a discussion of the process of analysis together with substantial reflexivity concerning changes during the analysis induced through the analyzed material. It is thus my aim to provide a detailed outline of how I approached the design of my study and which methodological choices I made. The goal is to contribute to a newly emerging way of approaching philosophical questions empirically that was recently provided with a manifesto by Thomas Pradeu and Lucie Laplane (Laplane et al., 2019).

The second motivation for providing a detailed methodological discussion is rather personal. Lacking formal training in sociology, books on social studies of science were decisive in providing me with insight into possible results of empirical case studies. Notwithstanding their multiple influences on my approach, methodological reflections are kept very short in these books. While these researchers use quotes from conducted interviews as anecdotal evidence for their hypotheses, how their theories and concepts emerged from the data are not accessible. I will subject my interviews to analysis tools that go beyond using them as anecdotal evidence. I aim to reflect on the steps that produced the theories and the accounts of the investigated research community.

4.2.1 Methodological reflections

The results of my case study are grounded based upon categories of analysis:

1) Close reading of scientific articles;

2) interviews with RNAi inheritance researchers;

3) participant observations in laboratories that research RNAi inheritance

The main focus of this empirical study will lie on 2); 1) and 3) will be used as complementary approaches. Naturally, these three approaches were interrelated through a dynamic, inductive process. For example, cues from the scientific literature influenced which scientists were interviewed and shaped questions that were asked. Cues from previously conducted interviews influenced observations as well as questions asked during observations. In what follows, I will discuss the methodological details of these approaches.

4.2.1.1 Literature

An assessment of the available literature on RNAi inheritance was the initial step that also shaped the research question for this project. An extensive survey of the literature was conducted in 2016 (about 300 papers), then newly published results were surveyed monthly until the end of the year 2018. Studying the RNAi inheritance literature had three main goals:

1) Acquire the necessary knowledge and detailed insight in order to determine plurality and propose a pluralist interpretation;

2) Acquire the "interactional expertise" (Collins, 2004) necessary to conduct interviews, listen to talks/participate at conferences, and observe in the laboratory. Interactional expertise is the expertise a social science researcher might acquire when engaging with a specific case study. She can talk like the experts and answer questions about the topic of the empirical research but cannot do experiments herself. This differentiates interactional expertise from contributory expertise, which is reserved to the practitioners. Collins and Evans summarize interactional expertise as: "enough expertise to interact interestingly with participants and carry out a sociological analysis" (Collins & Evans, 2002, 254).

3) Assess whether articles contain claims that suggest a pluralist position or reflect on plurality in a field. If, for example, a researcher interprets her results as providing evidence for a Lamarckian theory of inheritance/evolution, I took this as a cue that the respective researcher was suggesting a plurality of theories. Such notions were double-checked through interviews.

I read over 500 papers on RNA interference and specifically RNA inheritance. One important connecting link was the Fire and Mello paper of 1998. I studied papers of the 1980s and 1990s that reported anomalies, which would later be classified as RNAi based phenomena. In addition, I traced findings on the inheritance of RNAs starting in 1998 and followed these phenomena up until the end of the year 2018. I conducted the selection of papers by sampling references in one paper that referred to other findings in RNAi inheritance, reading the mentioned papers, and again sampling references. After repeating these cycles for several times, a point of saturation was reached, that is, no additional or little new references came up. In this process, I focused primarily on phenomena of RNAi inheritance in C. elegans, for which saturation was reached. Studies on other organisms such as plants, yeast, flies, and mice remained unsaturated, but papers that were regarded as "milestone papers" in these fields were read.

4.2.1.2 Selection of interview partners – core-set analysis

The review of the literature led to the first assessment of plurality/pluralism in small RNA inheritance. As a next step, I selected interviewees. Eligible interview partners were determined following a method called "core-set analysis," developed by H. M. Collins. By introducing the notion of the "core-set," Collins revisits the idea that replication of experiments by independent parties is always possible. Collins argues that replication and the design of "correct" experiments can only be ensured by a tiny group of scientists who have the relevant expertise but are all but "independent" (Collins, 1981b, 7). This small group is the core-set. The core-set is "made up of those scientists deeply involved in experimentation or theorization which is directly relevant to a scientific controversy or debate" (Collins & Evans, 2002, 242). A core-group, on the other hand, is a much larger group that comprises those solidaristic scientists after a debate has been settled (ibid.). The more esoteric a field is, the less researchers outside the boundaries of the core-set or core group can contribute to consensus or develop the science after consensus. The boundaries of a core-set might be hard to define, as disputes within a core-set often involve "boundary work" – defining who is in and who is out (ibid.). The term boundary work was introduced by Thomas Gieryn in the 1980s to describe an "ideological style" of scientists employed to solve the problem of demarcation. However, boundary work also traces demarcation processes within the sciences. I will provide a thorough discussion of boundary work in the second part of this section. For now, it is essential to note that the core-set is consciously not called a "group." Social markers that define a group, such as more interactions between the members than with the outside, are not fulfilled by core-set members. Proponents within the core-set might be enemies, or do not communicate with each other. Still, they are critical contributors to the respective debate (Collins, 1981b, 8).

Given these theoretical considerations on the constitution of a core set,

> "the best way to find out who are the members of a core-set is to locate some of them from the literature and citations, and then ask them who else they think has made a significant contribution to the debate. The named members can then be contacted and asked in turn." (1981b, 9)

Names that appear frequently can confidently be called members of the core-set. Core-set analysis is, thus, a form of snowball-sampling (Bryman, 2016; Guest et al., 2006). If specific individuals are less frequently named, the social scientist will decide about their membership. "Systemic discrepancies in scientists' perceptions of who is a member are likely to be more interesting than the exact membership. Exact membership is not a thing to be agonized over." (Collins, 1981b, 9).

It is important to note that Collins' definition of a core-set rests on the existence of one specific debate (such as those on the detection of gravitational waves or plant responses to remote stimuli). For the field that I am investigating, this is slightly different. The results of the small RNA inheritance field contribute to several different debates, on different levels, and in different disciplinary settings. Examples are the debates on

1) whether small RNAs or chromatin modifications have causal primacy in epigenetic inheritance;

2) whether acquired traits can be inherited, affecting phenotypes of later generations;

3) whether there are alternative accounts of evolution, not captured by the Modern Synthesis;

Although there is no "one" debate that defines the RNAi inheritance core-set of investigators, it nevertheless shows the characteristics Collins describes for a core-set. For example, the prevalence of boundary work and the absence of a well-defined group-structure according to sociological markers. That is, some members of the small RNA inheritance field have no interactions with certain other members. Therefore, I deem core-set analysis applicable for the researchers I am investigating.

Following Collins, core-set analysis has two steps: first, review articles and introductions to research articles were assessed for which researchers were cited. Textbooks were not considered because there is no textbook on small RNA inheritance in C. elegans. Surveying textbooks on a broader topic, such as "epigenetics," in general did not yield new names. Instead, it yielded the names of the most established members of the core-set, which were already part of the sample. The second step of determining the core-set consisted in asking researchers which other researchers are significantly contributing to the debate. Roughly speaking, the core-set was saturated after approximately half of the interviews were conducted, that is, no more new names were mentioned.

From this "saturated" core-set, it was possible to interview 80 to 90 % of its members. Some of the members did not respond to my emails or declined to be interviewed. In general, researchers that were characterized by other interview partners to be somewhat critical had a higher probability of not returning emails. In general, most interview partners readily agreed to be taped, while some asked who would listen to/read these interviews. Many researchers insisted that things they would say in the interviews about new data and other researchers were confidential. Some preferred to add to specific questions after the interview was finished in order to express personal opinions, which they decided were not eligible for being on tape. Such remarks were not considered for analysis.

4.2.1.3 Interviews

The primary cohort of interviewed small RNA inheritance researchers comprised principal investigators (PI). During observations in laboratories, these interviews were complemented through interviews with graduate students and postdoctoral researchers. The interviews were semi-open interviews based on fixed questions, however, additional questions were asked for clarification if required and the order of the questions was flexible (Bryman, 2016, 201). No "naïve" questions were asked and interview partners were conscious of the fact that their interviewer was a trained biologist, familiar with the details of their work. The interview consisted of three parts. The first concerned the scientific career of the interviewee, the second part concerned her perspective on the research field, and the third concerned her assessment of scientific pluralism. Interviews were always conducted in "native" working environments, most of the time offices, sometimes conference rooms or cafeterias (2016, 250).

Interview questions changed slightly over time. For example, after conducting the first six interviews, a question on whether interviewees regard the unification of theories as a desirable goal for science was added to facilitate an ensuing discussion on scientific pluralism. The interview manual can be found in the annexes. Part two of the interviews was substantially modified when interviewing graduate students and post-docs. Many of the questions in part two presupposed that interview partners were already active researchers by the year 1998 or soon after that. This condition was not met by graduate students and post-docs who all were between the ages of 25 and 35 at the time they were interviewed. Thus, the questionnaire was modified in the second part. Interviewees were asked about how they perceive their career opportunities in the field, how they describe the field in terms of cooperation and competition, and what motivates them most to stay in the field.

Part one and part two of the interview have the character of a narrative interview, in which the interviewee reflects upon episodes in her scientific career and is asked to interpret some of these episodes (2016, 589f). The third part contains questions that she is not "native" with. In the third part, interviewees are asked to reflect upon the problems of scientific pluralism in general and are asked whether they interpret their field as pluralistic or displaying plurality. Using philosophical concepts, such as "scientific pluralism," bears its challenges. One key challenge is to avoid imposing specific categories on scientists (Boyd & Westfall, 1970; Bryman, 2016; Fontana, A. & Frey, 2000). One way of avoiding this challenge was to not make use of such categories but instead describing the content of them by referring to examples the interviewee was familiar with. The necessity of this strategy was reinforced by the observation that concepts introduced by interviewees, such as "reductionism," showed idiosyncrasies specific to the researcher interviewed.

The only philosophical concept that was explicitly introduced in the interviews was "scientific pluralism." To avoid "imposing" the concept, the following strategy was employed. Interviewees were provided with a "rough-and-ready" definition of SP. Then they were provided with examples to familiarize them with the concept. A frequent example was that of the wave-particle dualism, assuming that they would recall this phenomenon from their school days[11]. Another example was Longino's account of theoretical pluralism in the study of behavior. I selected this example because one of the theoretical approaches Longino lists is the "genetic" approach, the home discipline of almost all interviewees. Some were even investigating the contribution of small RNA inheritance to behavior in subsequent generations. By using this example, they got an impression of what a state of plurality might look like. They could express a position concerning this plurality, for example whether they believed that in the long run, all explanations would have a molecular biological underpinning. After providing interview partners with a first idea of SP, they were asked whether this position is compatible with their scientific world view. I asked them whether they believed that science should aim for unification and how they would interpret a situation in which they were confronted with two possible theories. After the introduction to the topic, we discussed SP as an interpretation of small RNA inheritance in particular.

This "modus operandi" lead to two different approaches in the analysis of the interviews. The first approach was to assess whether responses to concrete questions were "yes" or "no" answers. Results were used for the quasi-quantification of qualitative research (Bryman, 2016, 630). The second approach was to assess the whole interview for possible pluralist or monist tendencies. Results were used for qualitative analysis. Discrepancies concerning outcomes of the first and second approach were not judged as refutations but as tensions that were more extensively discussed in the discussion section.

4.2.1.4 Participant observations

Participant observations were not a central part of the study but complemented other findings. During visits in research laboratories, one graduate student and one post-doc were observed for a time period of one and two months, respectively. The second aspect of observations included participation in (weekly or fortnightly) lab meetings of the research group. In these meetings, one or two members of the group presented progress in their research to the other members of the laboratory. Finally, negotiations of researchers while writing a review paper and while preparing the revision of a research paper were observed.

[11] Raffael Krismer has pointed out to me that many philosophers of physics and physicists would not agree that the wave-particle dualism is a case of SP. I nevertheless continued to use this example as it proved to give interviewees a good first idea. What was interesting to note is that some of the interviewees that were not provided with this example came up with it as an example for pluralism in their answers.

Observed researchers were conscious of the fact that I was acquainted with the details of the research. I, thus, also participated actively or as a consultant (for example, if researchers did not agree I was asked “what do you think?”). On a day-to-day basis, I kept a journal, in which I documented observations. Certain aspects of observations were taped if deemed particularly important. The main contribution of these observations was to deepen insight into trust relations and the social constitution of the research field in general (2016, 492). This was facilitated by the fact that researchers knew that I had interactional expertise but was not a member of one specific lab within the field.

4.2.1.5 Analysis

Interviews were transcribed and coded. As the aim of this study was to answer specific pre-selected questions, inductive approaches, such as grounded theory, were excluded. Instead, three pre-determined (deductive) research questions were selected: 1) How do researchers portray the dynamics of their research field? 2) What is the attitude of researchers towards SP? 3) How do researchers describe opponents of their discipline?

The method of analysis was “a priori coding” (Creswell, 2013). A priori coding entails selecting key themes – 1) to 3) – a priori and to search the text for passages that resonate with one or several of the themes. During the analysis process, subthemes emerged inductively, and these were incorporated into the analysis (Ryan & Bernard, 2003)[12]. In the analysis process, there was no major revision of key themes in response to the peculiarities of the material. Assigning one passage to more than one of the themes or subthemes was not avoided in order to allow for the emergence of cross-links. The code structure is depicted below.

4.2.2 The scientific field, boundary work, and small RNA inheritance

Having elaborated on the methodological workflow for conducting this case study, let me discuss the issue of defining a core-set or a research field in detail. Defining the studied scientific field is both a theoretical pre-condition as well as an output of the case study. In

[12] Validation of the selected codes was achieved in the following way. I am much indebted to Cosmas Grosser who coded two of the interviews inductively, using grounded theory. As a second step, I provided him with my codes and he re-coded the interviews. Two forms of validation were achieved. First, applying the same codestructure, my and his coding yielded very similar results. Second, codes emerged through inductive coding that were very similar to the codes developed a-priori.

this section, I will discuss approaches towards defining a scientific field, considering specifically the notion of boundary work and grid/group analysis. While boundary work describes a specific activity, grid/group analysis provides an account of resulting tensions.

For both concepts, scaling is of importance. “Boundary work” is a concept initially introduced to describe scientists’ strategies to demarcate “science” from what is not regarded as science. Grid/group analysis was initially developed by Mary Douglas to describe how different tribes across the world respond to perturbations in their cosmologies. Both concepts can and have been used to analyze sub-structures within societies as well. One could say that the processes both concepts describe have the properties of fractals, in that the structure of larger subunits resembles that of the next smaller structural part of the organizational unit. Boundary work can be observed when demarcating science from non-science, when emancipating biology as a discipline, and when negotiating the boundaries of biochemistry and genetics.

Knowing who is part of one’s field is also a part of an initiation process during university studies (Arnold, 2004, 19). In the first semesters of study, students are not only introduced to topics but also standards, norms, ethos, and memory of the discipline. In short, they are “disciplined” (meaning both reprimanding bad behaviors but also meaning to incorporate a person into a discipline) (ibid.). Communicating standards of how much and which kind of dispute is tolerated in a discipline is also part of this ritual (ibid.). Such taught disciplinary attachments are often stronger within a subdiscipline or an even smaller unit, like a cluster of labs, than for a discipline in total (Kastenhofer, 2004, 92). For example, Karen Kastenhofer observed that while identification with the discipline “biology” is rather weak, trained biologists identify as “zoologists,” “molecular biologists,” and the like, starting with their training as undergraduate students (2004, 92).

4.2.2.1 Boundary work

Gieryn defined the term “boundary work” as activities of scientists to solve the problem of demarcation. Boundary work includes acquisition of intellectual authority and career opportunities, denial of these resources to “pseudoscientists” and protection of the autonomy of scientific research from political interference. Boundary work is an “ideological style" (Gieryn, 1983, 781), an activity that is often conducted through elaborating on contrasts: "Scientists have a number of "cultural repertoires" available for constructing ideological self-descriptions, among them Merton's norms…" (1983, 783). In the course of boundary work, disciplines and subdisciplines are not defined through a set of necessary and sufficient criteria. Boundaries are drawn and redrawn, and characteristics to establish boundaries are used and disused as boundaries are established and re-negotiated.

While demarcation might be (or at least was) a challenging theoretical matter in the philosophy of science, it is accomplished on a day-to-day basis in practical settings. The questions over demarcation, or what constitutes a discipline is not only an analytical, academic matter. This question has tremendous practical bearing as successful boundary work leads to professional advantages. Boundary work is scientists'

> "attribution of selected characteristics to the institution of science (i.e., to its practitioners, methods, stock of knowledge, values and work organization) for purposes of constructing a social boundary that distinguishes some intellectual activities as 'non-science.'" (1983, 782).

The aims of boundary work are to acquire material and symbolic resources and to defend professional autonomy.

Boundary work is a near-universal rhetorical style, with the fractal character I mentioned above:

> "The utility of boundary-work is not limited to demarcations of science from non-science. The same rhetorical style is no doubt useful for ideological demarcations of disciplines, specialties or theoretical orientations within science" (1983, 792).

This results in the creation of a socio-professional identity: "Disciplines are political institutions that demarcate areas of academic territory, allocate the privileges and responsibilities of expertise, and structure claims on resources" (Kohler, 1982, 1).

Gieryn describes three configurations of boundary work:

> "a) when the goal is expansion of authority or expertise into domains claimed by other professions or occupations, boundary work heightens the contrast between rivals in ways flattering to the ideologists' side; b) when the goal is monopolization of professional authority and resources, boundary work excludes rivals from within by defining them as outsiders with labels such as "pseudo," "deviant," or "amateur"; c) when the goal is protection of autonomy over professional activities, boundary work exempts members from responsibility for consequences of their work by putting the blame on scapegoats from outside" (Gieryn, 1983, 792).

4.2.2.2 Grid/group analysis

While boundary work might appear to be a promising tool to define groups or sets of researchers, it is quite broad and might not capture peculiarities. In order to refine the theoretical toolbox applied in my case study, I will supplement the concept of "boundary work" with "grid/group analysis." Mary Douglas developed grid/group analysis as an account of cultural controls, informed by anthropological examples. This analysis soon turned out to apply to all sorts of "social gatherings" (Douglas, 1982/2013, 1). The core assumption of grid/group analysis is that "the infinite array of social interactions can be sorted and classified into a few grand classes" (ibid.).

Grid/group analysis has two dimensions: group commitment describes strong or weak ties between the members of the group studied. Grid control defines the hierarchical structure and the degree of regulation. This results in four different possible configurations of groups (1982/2013, 3-4). According to Douglas, grid/group dimensions are exhaustive of particular possibilities and "applicable to all kinds of social relations, wherever people apply penalties and rewards" (1982/2013, 5). In what follows, I will provide an overview of the characteristics of these four configurations. Then, I will review applications of grid/group analysis to "social gatherings" in science.

A) Low grid/low group (individualism): this configuration facilitates "negotiating contracts or choosing allies, and in consequence it also allows for individual mobility up and down whatever the current scale of prestige and influence" (1982/2013, 4). Low grid/low group configurations are described as individualistic, competitive, entrepreneurial. Individuals are only responsible for themselves. Inhabitants of low grid / low group structures perceive the world as not strictly ordered, and responsive to "special skills of the uniquely endowed individuals who study it." (Bloor & Bloor, 1982/2013, 89)

B) High grid/low group (atomized subordination): inhabitants of low grid / low group structures "do as they are told, without the protection and privileges of group membership (Douglas, 1982/2013, 4). Such configurations are isolated, subordinated, fatalist. Individuals are strongly regulated in terms of their socially assigned classifications. Bloor adds that individuals are often not there by choice but instead forced into this quadrant by pressures emanating from other quadrants (Bloor & Bloor, 1982/2013, 95).

C) High grid/high group (ascribed hierarchy): in this configuration, "loyalty is rewarded and hierarchy is respected, an individual knows her place in a world that is securely bounded and stratified" (Douglas, 1982/2013, 4). High grid/high group structures are bureaucratic, hierarchic and collectivist. This quadrant is structured by positional rules such as heredity, gender, age or other criteria (Caulkins, 2009, 65). Blame is often attributed to deviants. Inhabitants of this configuration believe that "the right methods and formulas, properly followed, will automatically produce the desired results." Nature is expected to be orderly (Bloor & Bloor, 1982/2013, 86).

D) Low grid/high group (factionalism): in low grid / high group structures, only the external group boundary is clear. All other statuses are "ambiguous and open to negotiation" (Douglas, 1982/2013, 4). This quadrant is often described sectarian or as an egalitarian enclave. "Organizations in this quadrant regard themselves as unique, as mavericks that are categorically different from other organizations with which they might be compared" (Caulkins, 2009, 65). No matter which quadrant of the grid/group system is perceived as the "majority," it is described as an enemy or threat. In an enclave, there is seldomly leadership, however charismatic leadership, without rules for succession might be accepted. Outsiders might perceive the system as deviant, while members feel it "is a matter of pride and indictment of the dominant organizations" (2009, 66). Inhabitants of such groups are mutually dependent but cannot trust another. There might be contradictory goals and conflicting duties, such as loyalty and competition (Bloor & Bloor, 1982/2013, 93).

Each of the quadrants has different modes of reward and punishment and thereby shapes the behaviors of its inhabitants. Individuals behave in sanctioned ways and use the framework of the respective configuration they inhabit to judge others and justify their actions. The four configurations of social groups lie in constant tension with each other. For example, entrepreneurial individualists might enter the reward system of the hierarchy by contributing innovations. Also, there are always groups of protestors, which are parts of enclaves and isolated workers who rarely have access to rewards (Douglas, 1982/2013, 8). A critical point about grid/group analysis is that the presence of only one of these categorized groups is impossible. None of these modes of justification and judgment is viable on its own, as other modes are needed in order to perform demarcation. Also, membership is not eternally fixed, and movement up and down the grid or group axes is possible.

One of the first researchers to link Douglas's work to studying scientific cultures was David Bloor. He appclies Douglas's model to Lakatos' "Proofs and Refutations," as both bodies of work "deal with the way men respond to things which do not fit into the boxes and boundaries of accepted ways of thinking; they are about anomalies to publicly accepted schemes and classification." Grid/group analysis classifies the range of possible responses to deviants, be it moral norms or counterexample to a proof (Bloor, 1978, 245). It is Bloor's hypothesis that also in the sciences, different responses are caused by different social structures: "The response to anomaly, and hence the drawing of intellectual boundaries will be negotiated into alignment with the pattern of social boundaries." (1978, 258). And: "By a process of selective reinforcement, characteristic forms of argument will emerge in a social setting, standing out by their frequency. This will give each social structure its dominant repertoire of explicit legitimations and its characteristic style of knowledge." (1978, 260)

Bloor specifically focuses on responses to counterexamples in mathematics: "Being a counterexample is a role which is conferred upon something, and this depends on how it is used" (1978, 249). "Counterexamplehood" is a social achievement (ibid.). A counterexample can become the basis of a revolution, "rivals can use it as a justification for a takeover." (1978, 254). Attitudes towards a counterexample will always polarize. If social institutions are stable, then there might not even be a resolution to a counterexample:

> "However loudly the advocates of the counterexample might proclaim that the theorem was refuted, and however vehemently the inventor of the theorem might declare the counterexample to be a monster, their two achievements would live side by side, insulated from one another by an institutional boundary" (1978, 256).

Bloor's elaborations on the different social groups within a scientific field shows several similarities to Biagioli's account on the anthropology of incommensurability. Socio-professional identity is a factor that determines who belongs to which part of the quadrant. For example, to have a specific set of expertise can be an essential way of identifying with one specific group (Bloor & Bloor, 1982/2013, 91). Bloor argues that utterances by researchers might not reflect which part of the quadrant they belong to. They often reflect which social milieus they would like to occupy. Utterances might often reflect goals and aspirations rather than present location. This is true for any grid/group position, as there will always be inhabitants of each quadrant who aim at increasing mobility and competition or decreasing it (Bloor, 1978, 261). Here one could add, referring to Biagioli's work, that such utterances might also reflect what he calls "strategic bilingualism." Researchers might force themselves into the reward system of another group while preserving one's socio-professional identity.

As my discussion of boundary work and grid/group analysis shows, both concepts are interrelated. Douglas argues that none of the grid/group configurations is viable on its own. Justification and judgment are based on an endeavor to perform demarcation from other quadrants of the grid/group axis. Besides, blame is often attributed to intruders from other parts of the grid/group sector. Membership to one of the quadrants is not eternally fixed. Continuous processes that can be partly characterized as "boundary work" are necessary in order to ensure the stability of the sectors. In addition, the three configurations of boundary work that Gieryn suggests can be regarded in resonance with different behaviors of trying to reduce or increase mobility up and down the grid/group axes.

In my analysis, I will follow primarily two aims when applying these theoretical concepts. On the one hand, I will characterize the group of researchers I interviewed for their position on the grid/group axis. I will analyze how they describe their opponents, and which position on the grid/group axis would be derivable from these descriptions. Also, I aim to investigate aims or pressures to move up/down grid or group, which might be prevalent in this research field. On the other hand, I will investigate the types of boundary work researchers are engaging in, and which types of socio-professional identities they describe for defining a member as in- or outside their group. I will also analyze their boundary work along the three configurations of boundary work following Gieryn. What will be key is to assess whether applying grid/group analysis to the small RNA inheritance community helps to predict their responses to anomalies.

4.3 Results

In this section, I will present the results of my case study on small RNA inheritance and scientific pluralism. The presentation of the results will be structured around the three key themes of my analysis: 1) What is the attitude of researchers towards SP? 2) How do researchers describe opponents of their discipline? 3) How do researchers describe the dynamics of a field that displays plurality?

4.3.1 RNA researchers and scientific pluralism

In this section, I will present two types of results. First, I will perform a quasi quantification of my qualitative results (Bryman, 2016, 630). I will assess in a quantitative manner how many of my interviewees are pluralists or monists. I will also stratify them along demographical markers. Second, I will present qualitative results on whether small RNA inheritance researchers are pluralists or not.

4.3.1.1 Quantitative results

In this section, I will discuss the quantitative results of my case study. This part is intended to complement the more extensive qualitative results that I will present in what follows. The goal of this section is to summarize the positions of small RNA inheritance researchers on whether they support SP as “yes” or “no” answers. This approach, of course, falls short of providing an in-depth reconstruction of scientists’ positions concerning SP or monism. While acknowledging the limits of this quantitative approach, it is my goal to point at some potentially remarkable findings that I will pick up again in the qualitative part of this section.

4.3.1.1.1 Essential tensions between global and local pluralism

In the course of the interview, I asked researchers about their attitudes concerning two different levels of pluralism. On the one hand, I asked them whether SP matches their scientific worldview in general. On the other hand, I asked them whether they regard a pluralistic interpretation of RNA and DNA inheritance as adequate. Hence, I differentiated between "global" vs. "local" SP. Answers were interpreted as "yes" or "no" answers and are shown in Fig. 18.

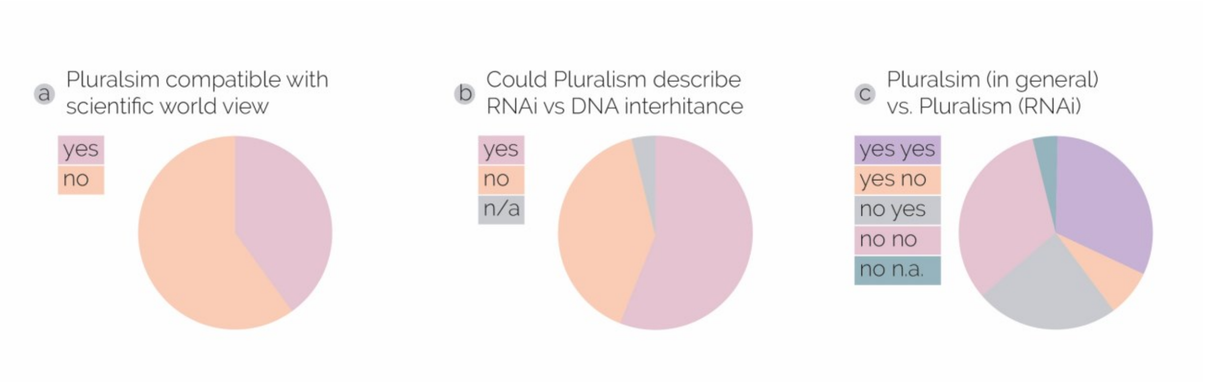

Figure 18 Visualization of interviewees' attitudes regarding scientific pluralism.

Fifteen of twenty-five interviewed researchers characterized SP as not compatible with their scientific worldview (Fig. 18a). This proportion changed when asked about SP as an interpretation of small RNA inheritance in particular. Fourteen of twenty-five interviewed researchers agreed that this interpretation might be adequate (Fig. 18b). It is important to note that not all interviewees who agreed with SP in general agreed that SP is an adequate description of RNAi inheritance. Vice versa, some researchers who argued that SP would not be compatible with their scientific world view agreed with SP as an interpretation of RNAi inheritance. This distribution is reproduced in Figure 18c. Eight of twenty-five researchers agreed with SP in general and as an interpretation of RNAi inheritance. Two researchers agreed with SP in general but rejected SP as an interpretation of small RNA inheritance. Six researchers rejected SP in general but found it to be an adequate interpretation of small RNA inheritance. Finally, eight researchers rejected SP in general and as an interpretation of small RNA inheritance. In the section on qualitative data, I will discuss in more detail what the acceptance or rejection of SP entailed for researchers.

4.3.1.1.2 Age and career stage influence positions towards SP

Amongst three selected demographic factors (age, career stage, sex), the sampling indicated differences concerning the acceptance or rejection of SP for two of the three factors. Statistical analysis was not possible because of the sampling (core-set analysis). For statistical analysis, it would be necessary to select interviewees in a stratified way or altogether random. This is not possible with core-set analysis, which is based on snowball sampling.

I divided researchers into three different career stages: graduate students, postdoctoral researchers, and PIs. Of these three groups, PIs were the largest group, comprising nineteen interviewees. Three graduate students and three postdoctoral researchers were interviewed. Comparisons between these three career stages revealed that early-career researchers accepted both global and local SP more frequently. Five of six considered SP as resonant with their scientific world view, and six of six agreed with SP as an interpretation of small RNA inheritance. A large proportion of advanced career scientists, in contrast, disagreed that SP is resonant with their scientific world view. Also, for small RNAs specifically, more than half of the interviewed PIs disagreed with SP as an interpretation of small RNA inheritance. It is nevertheless remarkable that almost all changes in opinion from rejecting global SP and accepting local SP took place in this group.

Comparing the attitudes towards SP by stratifying age groups (20-40, 40-60, 60-80) yielded similar results. The 20-40 years age group comprised all interviewed graduate students and postdoctoral researchers, along with some PIs. Acceptance of SP in general and as an interpretation of small RNA inheritance, in particular, decreased with age. The age group 40-60 displayed the highest number of changes in opinion regarding “global” and “local” SP. Thus, younger researchers in earlier career stages tend to be more open to SP (Fig. 19).

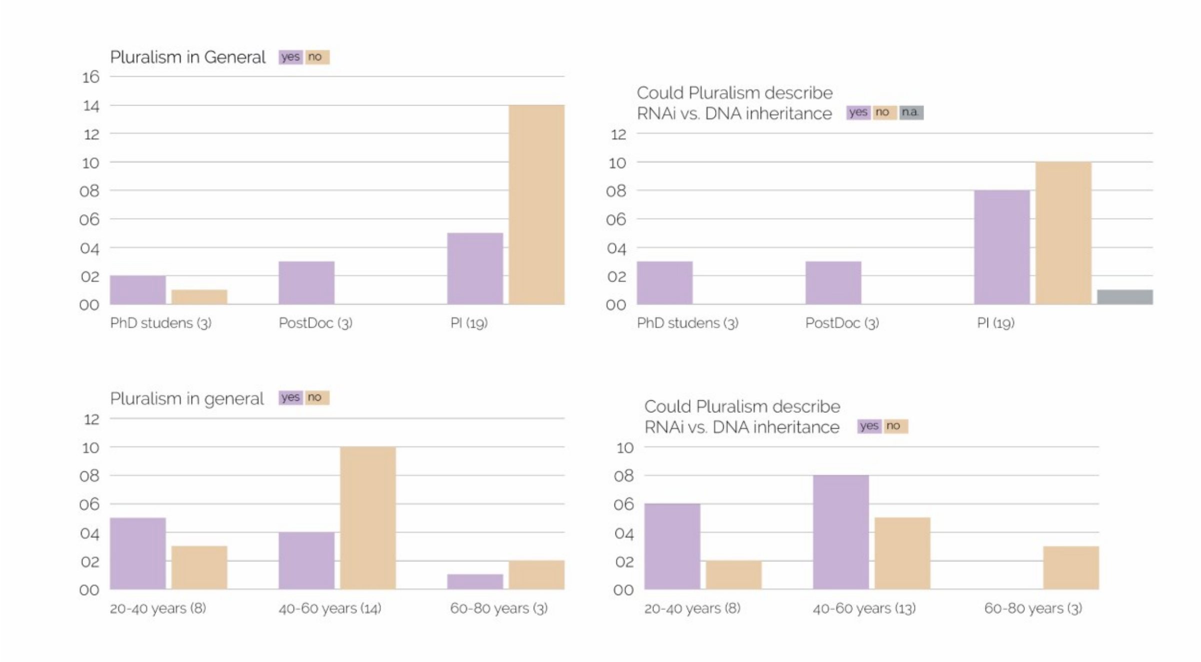

Figure 19 Visualization of researchers' attitudes regarding pluralism stratified by age and career stage.

4.3.1.2 Qualitative results

In this section, I will present results complementing quantitative results. Taken together, this section and the previous section assess whether small RNA researchers are pluralists. Before presenting the results, let me make one preliminary remark. No interviewee was familiar with the term "scientific pluralism." I take this as a critical contextual information about the interviews. As the following sections will show, interviewees expressed ideas that could be interpreted as pluralist. This section consists of three sub-sections. First, I will argue that the positions of most interviewees did not fit a strict pluralism/monism dichotomy. Second, I will list epistemological or metaphysical arguments advanced by interviewees to motivate their respective positions. Third, I will demonstrate that the question of integration vs. non-integration was a concern for interviewees.

4.3.1.2.1 Heterogeneity within expressed positions in tension with a pluralism/monism dichotomy

One of the key findings of this study lies in tension with the quantitative results of the previous section. The respective positions of many interviewed researchers did not unambiguously fit a strict pluralism/monism dichotomy. Some interviewees strictly adhered to a position close to or equivalent to monism. But the majority of interviewees displayed a vast heterogeneity concerning positions towards plurality and SP. In this section, I aim to discuss these findings in more detail and describe the encountered positions.

It is important to note that some issues discussed in this section will reappear in the sections to follow. This also reflects the coding strategy applied which allowed for multiple coding of text elements and mirrors crosslinks between the applied codes. First, I will show how interviewees motivated pluralism and monism. Second, I will show that researchers associate plurality and SP with aspects of scientific practice. Third, I will argue that interviewees provided unclear definitions of the “subject-matter” of SP. Finally, I will mention some miscellaneous observations.

“Non-absolutism” and not “non-monism” is the prevalent interpretation of plurality

Most interviewees acknowledged “plurality.” Most researchers agreed that the current state of research in biology/small RNA inheritance left room for a plurality of explanations, mechanisms, and also theories. Below, I will discuss heterogeneities when defining what the “subject” of plurality is in more detail.

For a philosopher of science, it is usually required to express a positive attitude in order to be considered a pluralist (Kellert, Longino, and Waters, 2006). Interviewees rarely stated an explicitly positive attitude concerning pluralism. Whereas some researchers felt that plurality was necessary and beneficial, or that they genuinely liked the idea of it, some found plurality quite worrisome.

Amongst researchers who had a monist attitude towards plurality, several emphasized that reconciliation and unification of theories, overcoming plurality, was the goal of science. Several argued that there should not be a clash between “whether something exists or does not exist.” They felt that contradictions or “aporia” were not possible in biology; two things could not be true at the same time and were rarely equally valid. These researchers agreed that plurality signaled deficits in knowledge or understanding. Let me provide an example of such a sentiment, expressed by a researcher after admitting plurality in the field:

> “But if you think that what is the goal, is the goal to come up with two final solutions that are both equally right… that is not true. The goal is to come up with one solution

> that is better than the other one and to keep going towards the better solution. Right? So that does assume that in the end a phenomenon can be explained by one."

Interviewees identified the goal of science as describing the natural world in a way that had utility and not to please each other by agreeing that both theories are right. Others expressed even stronger concerns about plurality by stating that it bothered them "at the very deepest of my scientific beliefs" and that they "don't like it."

One of the most exciting findings was how interviewees motivated pluralism. Interviewees frequently expressed the notion that absolute knowledge was unattainable. They referred to the absence of absolute knowledge to explain why several theories could account for the same phenomenon. This finding is quite striking, as it diverges from notions of card-carrying scientific pluralists. Philosophers arguing for SP seldom adhere to what I will call "non-absolutism." Interviewees asserted that we could never have absolute knowledge or full understanding. They pointed out that every theory was provisional and that our knowledge would always be limited. Some of the researchers expressing this position referred to the dependence of theories on exterior factors. For example, they explained the plurality of theories through the dependence on model organisms, the chosen method and disciplinary approach, and subjective perception in general.

At this point, it is important to note that such sentiments might also lie in the vicinity of another position, fallibilism. Fallibilism has several versions. One of the most famous is Charles Sanders Peirce's: Absolute certainty concerning questions of fact is unattainable (Peirce, 1955, 59). Other prominent defenders of fallibilism are Karl Popper and his student David Miller. They emphasize that we do not need to certify claims as true, but that it is sufficient to classify them as true (Miller, 1994, 27). It is tempting to suspect that fallibilism rather than non-absolutism is more familiar to researchers. The one or other version of "critical rationalism" is usually taught as part of science curricula at universities. Nevertheless, interviewees referred to these notions not to discuss the justification of theories but to explain plurality. In particular, for researchers who referred to the dependence of theories on subjectivity, experiments, and model organisms, I believe that a non-absolutist interpretation is warranted.

Scientific practice as a source of plurality

In this section, I will show that most interviewees associated SP with scientific practice. They pointed out that while planning or performing experiments and interpreting results, they were always confronted with a plurality of possible theories and explanations. They emphasized that they considered it beneficial to keep several possible theories in mind. Thus, interviewees identified plurality as a part of their research before reaching closure. Several researchers pointed to epistemological benefits of plurality and discouraged monist attitudes while producing and interpreting data. Interviewees emphasized the necessity of alternative views. Some pointed out that the plurality of possible interpretations or theories was sometimes overwhelming[13].

Elaborations on plurality in scientific practice were often crosslinked by coding to non-absolutist/fallibilist sentiments. Interviewees mentioned the plurality of explanatory strategies. They differentiated between a plurality of explanations, such as structure/function, how/why, or genetic/biochemical. They emphasized that one had to be open to plurality because "biology often comes up" with several solutions for a problem. In this context, they presented pluralism as the commitment to look at phenomena in different ways, employing different methodologies and develop new ways of thinking to increase the problem-solving ability.

It is important to note that the epistemic benefits of plurality in scientific practice were also emphasized by researchers who proposed a monist outlook on science. These interviewees also regarded it as productive to have several competing theories. Just like in the case of positive remarks about plurality in scientific practice, negative attitudes were also quite heterogeneous. For example, interviewees pointed out that while it was not a flaw to have several hypotheses, it nevertheless meant that one had not done enough work. Plurality in scientific practice should only be temporal. "Personal style" of researchers was emphasized, which determined how to accommodate the plurality of possible interpretation, that is,

[13] "So I would say not two, but too many. My view… personal view of data and the relationship to understanding is that understanding is something plastic which will keep changing based on the data. In other words the theory and experiment… relationship between theory and experiment… the theory is something that keeps changing and evolving."

whether to follow one or many[14]. Researchers who advocated for a monist attitude towards plurality in scientific practice argued that it was beneficial to "think in drawers" and that one had to focus on one problem. Some interviewees had an even more pessimistic outlook and were convinced that all possible theories or interpretations were usually wrong.

Several interviewees emphasized that on different levels of their research, either pluralism or monism prevailed. When pursuing one specific goal and "zooming in," it was necessary to be monistic. One had to be a pluralist, however, when trying to fit singular accounts into a bigger, complex picture. Thus, "resolution" was a factor when adopting a pluralist or monist attitude. Complex systems needed to be deconvolved in order to learn about them. At the lowest level if one was sufficiently specific, there should only be one answer. In this context, publishing was also mentioned. To publish a paper, one single question needed to be asked and answered and in order to ask such a question, "zooming in" was required. Thus, pluralism was associated with "bigger" questions.

The subject of plurality is ambiguous

One crucial recurring issue in several interviews were ambiguities concerning the "subject" of pluralism. Interviewees used the plurality of "theories," "explanations," or "mechanisms" interchangeably. While interviewees emphasized the plurality of theories in their research articles, , in interviews, they also considered mechanisms or explanations as subjected to plurality.

It is important to note that the terms used in interviews were not coextensive with standard definitions of these terms in the philosophy of science. While some used the terms "mechanism," "explanation," and "theory" interchangeably, others introduced important

[14] "I think it is a potentially dangerous but a very helpful way to do science, in my opinion, I tend to be somebody who likes to think in "Schubladen" – drawers. I… because I like to simplify things and maybe then realize that there are exceptions to this but without knowing the underlying major rules it is much more difficult to make sense out of the exceptions, in my opinion, so… I tend to be somebody who looks for general rules. In the end there are physical and chemical rules that we cannot change but of course it has a certain danger. I think you will not find certain things because your brain just doesn't look for these it just pushes that aside but, that's why we need different types of researchers."

differentiations or opined that pluralism would apply on all of these levels. Others mentioned that it was a biological fact that a phenomenon was often brought about by multiple mechanisms. This invited different explanations or theories. Specifically for those cases, their understanding of “mechanism” and “explanation” was relevant for their rejection of pluralism. Several interviewees advanced the notion that if one phenomenon required several explanations, the phenomenon had a “bigger” explanation. Through uncovering these different explanations, one discovered “the” mechanism. Interviewees brought forward similar arguments for cases in which there was more than one theory.

Before closing this section, I will highlight an observation concerning interviewees’ attitudes towards “mechanism” and “theory.” Several interviewees emphasized that they considered it their job to uncover “mechanisms” and that they were “mechanist kind of guys.” They argued that they were not interested or not trained in “paradigmatic thinking” or “high-level kind of thinking.” They usually did not bother whether mechanisms fit or did not fit theoretical accounts. By highlighting their interest in phenomena and their mechanisms only, they suggested that the “kind of thinking” SP requires was not part of their scientific life:

> “And thinking back of why I again… so also me, I didn’t come at this sort of phil… You know, I am kind of a very mechanistic kind of guy… I started life as a physicist so I didn’t think uh – modern synthesis – are there any problems, are there something else… I really came from this and then decide on the worm, and then uh let’s look at this and in fact it was really…”

Further issues raised

Similar to researchers’ takes on plurality and SP, their positions on whether science should aim for unification were heterogeneous. Researchers who expressed a positive attitude towards pluralism typically shared a negative attitude towards unification. Several emphasized that unification lead to the exclusion of theories and phenomena that did not fit the unified framework. Some reprehended “reductionist science” for losing complexity out of sight. They also complained of being “forced into monistic paradigms.” Others expressed an ambiguous position towards unification, arguing that unification was “nice but not necessary." Interviewees mentioned a "human bias": it was a human condition to think that phenomena were not well understood if not accounted for by a unified theory[15]. Interviewees displaying a negative attitude towards pluralism argued that they considered unification the goal of science.

The last issue to be raised in this section is a specific attitude towards "local" vs. "global" SP. Several researchers expressed the opinion that they felt uncomfortable when taking a "global" position towards SP. They aimed at basing their pluralism on empirical data and argued that they preferred the experiment to demonstrate whether a phenomenon requires a pluralist interpretation or not. Others emphasized that they were "pluralists about pluralism" leaving it open that some phenomena would not receive a pluralist interpretation.

4.3.1.2.2 Researchers motivate their positions referring to metaphysics or epistemology

In this section, I will review how researchers motivated their pluralist or monist positions. I will show that, in most cases, interviewees referred to epistemological or metaphysical reasons to make their case for or against pluralism.

Epistemological attitudes

One popular way of formulating an epistemological argument for SP was to refer to the provisional character of knowledge. Interviewees pointed out that as knowledge could never be finite or complete, different theories were always necessary. By focusing on one explanation, ways of thinking were narrowed down. Others referred to scientific practice, maintaining that close-mindedness and losing possible alternatives out of sight was detrimental to scientific practice. Others considered open-mindedness and awareness of alternatives to be an intrinsic scientific value:

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[15] "But also when you look at neuroscience it feels like people can't give a unifying explanation for the plain and this feels to us, maybe it is just the human bias, that we don't understand the brain because we can't explain it by one explanation or just several basic explanations. So I don't know if it is human bias, maybe it is. If you have some very fundamental laws then you feel that you actually understood something."

> "But then you know in many cases you know these discoveries, I think there's a lot of cases were the way that we talk about ideas is flawed to the extent that it sounds very monistic because of the way we just lapse into talking to, like treating transposons like they are these bad guys, that we have to control. I mean that is a really I think unfortunate way of talking about them because it may like turn a lot of people off how interesting the other possibilities are they are actually the oxygen in the genome, without them we suffocate as a species, so there is a lot of different ways of looking at things and we have to be careful as scientists not to become dogmatic or monastic or nonplural in our thinking, we really need to keep pluralism alive because it is the only way that science really works is to be pluralistic…"

In this context, religious beliefs were used as a counterexample, as dogma would not admit these values. Along these lines, researchers criticized research papers for being monistic in only focusing on one aspect of the subject matter.

Some interviewees argued that they would like to be sympathetic to admitting several theories but were concerned about measures how to keep out "crap." Others worried that pluralism would make the public distrust science. These opinions were in tension with statements of other interviewees who argued that they would be in favor of admitting creationism if epistemological benefits were possible. In this context, researchers came up with examples of co-optation of techniques developed in non-scientific intellectual enterprises.

Several interviewees supported an ambiguous position in which they approved pluralism for its (short term) epistemological value while disagreeing that plurality should be the long-term goal. They expressed that although several theories would always be possible, the goal should be to choose one. This position is ambiguous because it failed to define a point at which the epistemological benefits of SP would have to be given up to accommodate monism as a long-term goal. Interviewees who supported a negative attitude towards SP in general argued that the goal of science was to find one theory. If there were several alternatives, this meant that "true" knowledge of the phenomenon had not been attained.

Metaphysical attitudes

Several researchers argued that biology was "trivially" pluralist. Biology worked in a way ("whatever works exist"), so that plurality was to be the usual outcome. They claimed that the complexity of biological phenomena and the different levels of resolution required a pluralist underpinning. Others referred to the perceived "messiness" of biology. Researchers who received their academic training in disciplines such as physics or chemistry claimed that biology was trivially messy, and thus pluralism was the necessary consequence. Others emphasized that exceptions, redundancies, complementation, and ultimate variation fostered plurality in biology:

> "Physics and biology are very different in that respect… and in physics there are ultimate truths… and in biology there is ultimate variation"

Researchers advancing a monist view also mentioned "messiness" and "odd model organisms." Besides making the epistemological argument that the goal of science was to come up with one account, most researchers arguing against pluralism made use of metaphysical arguments. They referred to the "way nature is," to the presence of one singular molecular basis, and the deterministic structure of the world to argue against pluralism. Some argued that biology was quite "prosaic," and thus did not admit pluralism. Along these lines, several interviewees advanced the view that biological phenomena should, in the end, be explainable by referring to chemistry or physics.

4.3.1.2.3 "Integration" vs. "non-integration" is central to pluralist interpretations

In this section, a key theme elaborated in the previous chapter be critical. The question whether theories should be regarded as integratable or as non-integratable often determined whether interviewees accepted pluralism or not. Specifically, interviewees disagreed with "non-integratable" pluralism. This was pivotal for their interpretation of small RNA inheritance as pluralist or not.

Defining "inheritance" or "inherited phenotypes" as the subject of SP, several researchers argued that while non-DNA based modes of inheritance were indeed in place, this did not contradict DNA-based inheritance. It was clear that one needed different explanations and theories and these needed to be combined in order to account for the phenotype. The fact that non-DNA based inheritance required a different theory to account for it did not contradict Neo-Darwinism. They felt that DNA- and RNA-based inheritance did not contradict each other[16]. Researchers believed that it was not necessary to argue that RNA-based, epigenetic inheritance was necessarily operating on every single trait. Evidence for RNA- based epigenetic mechanisms operating on some traits was sufficient to require an additional theoretical account. In these regards, interviewees often mentioned the extended evolutionary synthesis as a way to incorporate different theoretical accounts.

The question of integration vs. non-integration involves another issue that is important both from a theoretical perspective and a practical one. When do two theories contradict each other and what does it mean that evidence contradicts a theory? This has a specific bearing on the Neo-Darwinian account of inheritance and evolution. If phenomena are observed that cannot be fit into a Neo-Darwinist framework, does this mean Neo-Darwinism is wrong? Several interviewees disagreed. Let me provide a simplified version of Neo-Darwinism: "Random DNA mutations are the only source for evolutionary innovation." Findings that directed RNA inheritance influences evolution is a contradiction, if "the only" in the provided definition is emphasized. Most interviewees, however, did not think that Neo-Darwinism was wrong. They believed that Neo-Darwinism did not explain all phenomena. Therefore, interviewees did not regard their findings as a counterexample to Neo-Darwinism. And they did not regard theories that might account for these findings as alternatives to Neo-Darwinism but as theories that together with Neo-Darwinism covered a broader range of phenomena. However, interviewees mentioned very real "clashes" with researchers who claimed that evolution could only happen gradually.

Despite emphasizing that both accounts did not contradict each other, interviewees argued that acknowledging RNA inheritance had led to a paradigmatic shift in thinking. They did not

[16] "I think they just extended… actually I would see it just as an extension, small RNA mediated inheritance of traits has a very firm molecular basis, no question, as I pointed out. It doesn't violate any molecular rule, it extends I think Darwinian inheritance because it still is always tied to a DNA locus, which has Mendelian inheritance. So I think it is an extension, I wouldn't call it as a challenge, I would call it an extension."

think that this paradigm shift necessitated believing that Neo-Darwinism was wrong. But this paradigm shift necessitated believing that Neo-Darwinism did not explain all phenomena. One interviewee specifically argued that this was not a paradigm shift of the "Kuhnian kind," because Neo-Darwinism was not rejected in the course of it. To support this claim, interviewees argued that both trajectories of inheritance interacted at a molecular level: DNA, the chromatin modification machinery, and RNA inheritance machinery were mechanistically interconnected. This argument was brought forward frequently when interviewees explained why small RNA inheritance was not a case of non-integrative pluralism. For providing their arguments, they referred to the properties of the molecular processes at work. Some specifically emphasized that biology "works in a way" that theories were not incompatible. Small RNA inheritance was another way of inheritance, but researchers were not wrong when believing in DNA inheritance. Interviewees portrayed RNA and DNA inheritance as different "modes" of inheritance that invited an integrative perspective. In addition, interviewees introduced the term "coexistence" and "complementarity."

For other researchers, the interconnectedness of small RNAs, epigenetic effects, and chromatin changes gave them reason to doubt that pluralism described small RNA inheritance. One interviewee pointed out: "It is not like we would say that information is passed on through telepathy.[17]" In that sense, some were confused about pluralism. They insisted that they did not know any people who said that there was only small RNA-based inheritance. Others emphasized that modes of inheritance that were neither mediated by DNA nor RNA should not be disregarded. Along these lines, interviewees urged that researchers adhering to the different "camps" of epigenetic inheritance should engage in more pluralist thinking. They urged that all known trajectories should be acknowledged.

[17] "Well I suppose it depends what you mean to some extend by pluralism. So I see the processes we are talking about, small RNAs and epigenetic effects and chromatin changes and so on and so forth… I mean these are all linked to more conventional paradigms of genetic inheritance. So mechanistic, you know, they involve RNA, they involve the same types of molecules and I think they fit within the same paradigm. So I don't see them as being, you know, we're saying there's something like telepathy running in parallel to lets say a more conventional explanation for information being transmitted from one generation to the next. So that would be truly if you held that view then you would be truly pluralistic. Well I suppose my question to you is do you see this as being fundamentally different from being genetic inheritance?"

Interviewees highlighted the differences between modes of inheritance. Some argued that they would feel awkward when putting DNA and RNA-based inheritance on the same footing, for “there is no life without DNA.” Several emphasized that DNA and RNA inheritance provided different kinds of solutions for different problems in different timespans. Although interviewees emphasized these different levels, they found that “reduction” of RNA-based mechanisms to DNA would be completely wrong. DNA and RNA inheritance were two distinct channels for transferring information.

Several interviewees changed the subject from thinking about inheritance to thinking about evolution. Some argued that there were reasons to rethink evolution and investigate how small RNAs could affect genetic mutation rates. Both DNA- and RNA-based inheritance affect each other. Natural selection, interviewees hypothesized, could operate on either randomness or directedness. Several felt that small RNA inheritance would deliver enough material to challenge traditional views on evolution. For example, one researcher argued that Lamarckian evolution could be engineered, and asked “what to do with that?” It is important to note that when talking about evolution, several interviewees mentioned that Darwin accepted Lamarckian inheritance. They introduced Darwin’s notion of the “gemmule” in order to justify their position. They preferred, thus, Darwin over Lamarck as their ideological forefather.

Other interviewees referred to the integrability of DNA- and RNA-based inheritance to argue against SP. Because these processes were intertwined, it was hard to distinguish them. By mentioning the interconnectedness of both pathways, interviewees rejected a pluralist interpretation of RNA inheritance. Proponents of this view claimed that RNA inheritance was an additional layer and just an extension. Because it did not violate any rule, it was no challenge. The plurality of RNA- and DNA-based modes of inheritance was regarded as resolvable. As both were governed by different molecules, their relation would be uncovered, and thus a “bigger” explanation of inheritance could be provided.

4.3.2 How are opponents of RNAi inheritance characterized?

In what follows, I will turn the question asked in the last section on its head. After having inquired whether small RNA researchers are pluralists, I will assess whether small RNA researchers characterize their opponents as monists. I will specifically focus on three points: 1) Which researchers are opponents of small RNA inheritance? 2) Which motivations do small RNA inheritance researchers attribute to their opponents? 3) Are opponents characterized as "monists"? Several issues discussed in this section will re-emerge in section 4.3.3, as several themes were strongly cross-linked with notions on the perceived stability of the field. In this section, I will demonstrate the heterogeneity of opponents and the corresponding variability of the contents of critique. This situation leads to expertise becoming a pressing issue, a problem to which I will come back in section 4.3.3.

4.3.2.1 Who are the opponents?

It is important to note that most interviewees were quite ambivalent when asked to characterize critics. Before being asked explicitly who their opponents were, several indicated that they were constantly confronted with opposition and were being "yelled at" from different sides. When asked to name their opponents, most were not able (or possibly did not want) to provide names. Some referred to this tension explicitly. When asked for a description of opponents, responses like: "I am sure there are, but if you ask me to name somebody I don't think I could," or "Nobody that would go on the record… but almost everybody I talked to…" were typical.

While not providing names, most interviewees identified their critics by other means. Most often, they characterized their opponents by affiliation with a specific research field. Along these lines, interviewees indicated that criticism was most often expressed by "a bunch of people guarding their careers." Five groups of researchers were frequently described as opponents: evolutionary biologists; researchers working on transcription factors (TFs); researchers working on "classical" epigenetic inheritance, that is, the transmission of DNA or histone modifications; neurobiologists; researchers who did not succeed with small RNA inheritance experiments. Before discussing the motivations interviewees attributed to these groups of researchers in the next section, I will survey general remarks on opponents.

Several interviewees expressed the feeling that resistance and opposition to their findings were stronger in the early days of small RNA inheritance, that is, in the 1990s and 2000s. They explained that it took researchers a while to accept that certain phenomena were occurring. One interviewee recalled that in the "early days" of small RNA inheritance in C. elegans, an "old generation of scientists," the leaders of molecular biology in the 1950s and 60s dismissed findings on RNA inheritance as a "mass psychosis in C. elegans." They opposed the idea that there could be "new biology, especially important biology still being discovered 50 years after all the important experiments were made." Several interviewees expressed the sentiment that an "older generation" was more likely to oppose their findings. Some jokingly remarked that because discoveries were happening so fast, the older generation of scientists did not have time to "die out."

Several interviewees mentioned that they experience a general hostility towards their research: "Some people just say 'I don't believe you.'" They felt that this general opposition led to their papers getting rejected because of disbelief[18]. Some described these critics as "really aggressive," and insisting that small RNA inheritance researchers should stop doing their research. Others mused that because their field was so competitive, it was hard to discern whether a reviewer wanted to slow down her opponents or rejected papers because of disbelief. While criticizing this hostility, interviewees acknowledged that experiencing criticism made them do better and more rigid experiments and made it easier to discern "poor craftsmanship."

Many interviewees insisted that one has to differentiate between the different MOs when criticizing small RNA inheritance. Most pointed out that epigenetic inheritance in humans was specifically contentious. In comparison, there were hardly any critics of small RNA inheritance in C. elegans because the findings "swept the field." But in other fields, this was different and the mammalian field was described as the "most extreme" and "radicalized." The majority of mouse researchers was skeptical and believed "it is bullshit.[19]" Small RNA

[18] "But the addition of this subtle phenotype make it and some people for example said: oh environment doesn't change so much, for example diet, or stress it's not enough for me… Some people say that… It makes me really angry because its resemble exactly what people said at the time of Galilei, that the earth doesn't turn enough, even though that it turns a little, that we have seasons, and gravity, and a number of the facts are dependent on that and its exactly the same thing."

[19] "In other fields it is very different and the most extreme other field are the mammalian field, including humans where I think the vast majority is extremely sceptical. And again it is quite interesting because if you wouldn't know anything about the field then you would say "whuu there are two science papers proving tRNA fragment mediated inheritance of information so that's a fact!" But then if you would ask people, the majority would say, even people working in the field and as you heard me say, I am not convinced that they have

inheritance researchers working with C. elegans explained this by claiming that the "believers" in the mouse field used "minimal data and claim outrageous things." These claims, in turn, made other members of the mammalian research community angry. Along the same lines, small RNA inheritance researchers who were members of the plant field claimed that in the plant community, "it wasn't that bad" as in other fields. In the context of criticism of the mammalian field, in particular, some interviewees identified themselves as critics or opponents.

4.3.2.2 What are the opponents' motivations?

In this section, I will analyze how interviewees described the motivations of their critics. One focus will lie on the opponents' disciplinary backgrounds, but more general remarks on motivations for criticism will also be surveyed.

Almost all interviewees described evolutionary biologists and specifically population geneticists as opponents of small RNA inheritance. Some explained this opposition by the fact that evolutionary biologists did not "like the hype." Others hypothesized that evolutionary biologists were angry that molecular biologists started to make claims about evolution, although molecular biologists were "novices" in the field of evolutionary biology[20]. In this context, interviewees mentioned that evolutionary biologists were angry at people who "come from mechanism" and tried to think about evolution. Some pointed out that evolutionary biologists had a general antipathy against epigenetics. They specifically disliked RNA inheritance because it was irritating to them and "ruins the equations of population geneticists." Evolutionary biologists were "…completely against it. And certainly anything which is not DNA mediated and inherited is completely crazy.[21]" Another

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proven thing. And I think that I am quite open minded. I think a lot of my colleagues would say that this is bullshit."

[20] „they probably don't like the hype, because they are… They epigeneticists like myself and others are very kind of… we are novices in the field and we are trying to say something without knowing the principles that they have established… So I can see that argument.. but they also probably cannot appreciate.. they are more close minded, appreciate the new science"

[21] "Uh – there's one other group, evolutionary biologists think that this is a criminal kind of thing so most evolutionary biologists… again they really don't like anything they hear about epigenetics, the most majority I think are completely against it. And certainly anything which is not DNA mediated and inherited is completely crazy. And this includes very prominent evolutionary biologists still today but often its just the lack of knowledge and understanding. Also they get pissed of by some outrageous claims, in particular if, lets say

motivation of evolutionary biologists mentioned was that they doubted the importance of epigenetic inheritance in general. It is important to note that several interviewees criticized evolutionary biologists for lacking the necessary knowledge and understanding required to voice appropriate criticism.

Researchers working on "classical" epigenetic inheritance were another group frequently described as opponents. Interviewees described the formation of two camps within the study of epigenetics: chromatin vs. small RNA people. They criticized the "chromatin people" for thinking that only modifications to DNA or histones could play a role in inheritance. Some interviewees argued that researchers working on chromatin factors opposed the small RNA field because they were frustrated as chromatin research did not produce fruitful results. The distribution of "chromatin people" and "small RNA people" investigating a specific model organism was also mentioned as a problem. For example, most "drosophila folks" were "chromatin people."

A different set of critics often mentioned were researchers working on TFs. Unlike evolutionary biologists and researchers working on chromatin epigenetics, interviewees expressed regret that researchers working on TFs were their critics. For example, one interviewee stated: "It's a shame that it all ended up that way." Small RNA researchers explained the opposition of researchers working on TFs by the fact that to the outside, small RNA inheritance and chromatin inheritance were often regarded as part of the same field. Thus, TF researchers who were very critical of chromatin inheritance were also critical of small RNA inheritance. The contempt of TF researchers against chromatin inheritance had historical reasons. Twenty years ago, there was a "huge rush where everything was chromatin epigenetics and TFs didn't matter anymore." Also, in the early days of RNAi research, interviewees pointed out that there was a rush on miRNAs because they were considered to have the same function and importance as TFs. Interviewees thought that small RNA researchers and TF researchers should join forces against "chromatin people."

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people like me who come from mechanism and then say uh – would be nice to look at this in evolutionary context."

Small RNAs and TFs shared specific properties that chromatin modifications lack. This, however, is yet a goal to be achieved.

Another group of critics that was explicitly mentioned by interviewees working in or interested in mammalian studies were neuroscientists[22]. Most articles on transgenerational inheritance in mice reported behavioral phenotypes. The inheritance of small RNAs in mice often lead to behavioral changes in consecutive generations. Thus, small RNA researchers became competitors to neuroscientists. Disputes on how trauma affects the brain and how specific behaviors are caused were traditionally negotiated in the neuroscience research community. Interviewees argued that findings on the transgenerational contribution of small RNAs to specific behaviors were "a step too far" for this community.

The last group of researchers opposing small RNA inheritance mentioned were those that had a bad experience with small RNA inheritance. Interviewees characterized these researchers as having invested much money in small RNA inheritance experiments but did not succeed or could not replicate previously reported findings. These opponents scrutinized findings of small RNA inheritance for methodological flaws.

Besides remarks on group membership, several interviewees pointed to the "history" of epigenetic inheritance as a reason for extensive criticism. Several blamed "the Soviets," specifically Lyssenko and Stalin, "the Germans," specifically eugenics, and the history of Lamarckian epigenetics in general for causing much suspicion. In this context, many

[22] "Here the people who are really super sceptical and I can understand why are neuroscientists. Because there are a number of papers in mice that claim behavioral effects and a lot of the neuroscience community is rejecting this altogether. First of all they are from a very different community and they even have a problem with epigenetics as such – this is quite funny – but then this was a step for them too far. And a lot of them without even thinking about it rejected it, also due to the fact that there are some very poor quality studies that they likely point to. And even for example there is a very famous study in rat, where I think you… if you get… it has to do with hippocampal development in rats that either get maternal care or not maternal care, where apparently touch to the face and maternal care in rats really helps the development of the hippocampus. It's a very famous study I can't remember the authors but this… Even the authors who published the paper later in their life they… couldn't repeat it when the tried to… this is apparently… Yeah something that is in the literature but even the authors themselves are saying we're not sure any more that it really was true."

interviewees mentioned that they started to avoid referring to “Lamarck” because they experienced pushback when formulating their findings with reference to Lamarck.

Others provided more general explanations for the pushback they experience, such as critics displaying “intellectual laziness,” rejecting “what they are uncomfortable with,” and downplaying small RNA inheritance researchers as “heretics.” Interviewees described their findings as so surprising that people did not “want to accept them.” Some explained this resistance as an “innate response” to findings that did not fit the critics’ world view. Such responses were “normal human behavior” because most people needed time to “accept a new paradigm.”

Interviewees characterized the resonance of small RNA inheritance with “the public” as both a problem and a reason for suspicion and criticism against the field. They rooted this problem in a popular misconception of what epigenetic inheritance is. It is easy to excite “the public” because the notion that inheritance could be changed through behavior was appealing to people. This circumstance caused anger in other research communities because this excitement could be transformed into funding at the expense of other research projects.

Most interviewees identified another line of criticism based on rather general arguments. For example, critics argued that small RNA inheritance was an artifact, a weird background effect, or criticized the significance of small RNA inheritance beyond a few examples. Other critics were concerned about inconsistencies between findings or replication issues. Interviewees mentioned this problem along with the claim that experiments, specifically in the mammalian field, were challenging to do, hard to control, and effects were often minimal. For these lines of criticism, interviewees referred to the expertise required as a significant problem. For example, they felt that one had to be working in the mammalian field in order to understand whether something was meaningful or not. In more general terms, interviewees explained that small RNA inheritance effects were inherently never as discrete as Mendelian phenotypes and thus required different standards for measurement.

It is, however, essential to note that interviewees found some lines of criticism appropriate. They mentioned that some small RNA researchers were “a little nutty,” and that the whole field received “backlash” because of them[23]. They criticized these researchers for being too evocative when using the term Lamarckianism. RNAi inheritance researchers made themselves unpopular because they did not recognize that what they were talking about was only a part of heritability. It thus appeared to critics that the role of genetics was downplayed. Many interviewees mentioned the problems of overinterpretation, presumptions about “meager experiments,” and “wishful thinking.” They criticized that researchers got “carried away” with their findings too easily. Most of them readily admitted that there were some poor-quality studies to be found in the literature. They believed that such articles gave those researchers who wanted to study RNA inheritance properly a very hard time. Often critics only read these “really bad” papers and concluded that the whole field is “horrendous.”

4.3.2.3 Monism is a common theme for describing opponents

Several researchers felt that they were subjected to a “prevailing culture of forcing people into coming up with monistic paradigms.” They criticized their opponents for their tendency to think only one way. This gave rise to dogmas which were not questioned anymore. Interviewees explained that they were perceived as “heretics” by defenders of orthodoxy and described opponents as afraid of the unknown and the dark matter. More specifically, interviewees reprehended their critics for eagerly holding on to theories that were inconsistent with data. One interviewee claimed to feel like Galileo having to face the Aristotelians.

Some interviewees pointed to the history of unorthodox forms of inheritance and referred to a “story of 100 years of suppression.” Opponents were criticized for rejecting findings that did not fit their scientific worldview and for being afraid of pushing the boundaries of accepted knowledge. One interviewee concluded: “The only monists I know are DNA people.” Again, the issue of different generations of researchers was mentioned: “As you

[23] “And anyone I think that is talking about… like sculpting evolution is… probably a little nutty… And some people do that and therefore you get a backlash. Thank you for pointing out the evolutionary biologists, I haven’t even talked to one about this but I can only imagine how irritating transgenerational epigenetic inheritance people probably are to them, yeah”

get older you get more used to the things you know and not perceptive [sic] to new ideas." In this context, one interviewee remarked that one could measure the real impact of a finding if one assessed how much it held the field back over time[24]. Those researchers who insisted on the absolute truth of established dogma were characterized as monists along these lines, and, as monists, they were judged to hold the field back.

4.3.3 Dynamics of the RNA inheritance research field

In this section, I will ask how small RNA inheritance researchers describe their research field. I will inquire whether interviewees experience their field as stable or unstable and how the plurality the field produces contributes to their impressions. In so doing, I will specifically focus on three points. First, I will investigate how interviewees assign group membership. Second, I will survey the reasons why interviewees regard their field as unstable. Third, I will demonstrate that factors beyond the plurality of theories contribute to the observed plurality. Specifically, I will enquire about the role of research traditions and model organisms.

4.3.3.1 Who is part of the field?

In this section, I will survey how interviewees assign group membership and specifically focus on which categories they employ to do so. Interviewees were very conscious about group membership and described only a fraction of other interviewees as group members. Some justified their choices by arguing that the small RNA inheritance field was tiny. Others argued that what used to be one field was not one field anymore but had bifurcated. In general, researchers assigned group membership along these lines: choice of model organism, type of small RNA worked with, for how many generations researchers could track RNA inheritance, and whether researchers did "natural" or "artificial" experiments. In what follows, I will survey interviewees' elaborations in detail.

[24] "Well, I would say jokingly that to my friends… and it's true that the strength of your discovery is … can be measured in how much you hold a field back, and that's what happened. So if something is so cleaving and so clean ahhh… Research tends to be held back because everyone works within that framework and changing frameworks requires a lot of work. Requires a lot of detailed thinking. So ahm often you'll find that the more ingrained the framework the harder it is to topple it and in that sense..."

Interviewees emphasized that working with a specific model organism (MO) required specialized technical knowledge, which cannot be extended to other MOs. As a result, they described themselves as mildly skeptical about "stuff published in other organisms." Also, the relative development and methodological standards which constituted the "repertoires" of research on a specific MO played a role. For example, C. elegans researchers argued that they found it hard to trust vertebrate studies. Compared to worms and plants, they found the experiments methodologically wanting. Others referred to specific properties of an MO to determine whether they believed that it inherits small RNAs are not. For example, interviewees pointed out that small RNA inheritance made no sense for MOs with long generation times. They did not understand why researchers would even study small RNA inheritance in them.

Group membership was rarely assigned to researchers working with mice. Interviewees offered several arguments about why they did not consider researchers studying small RNA inheritance in mice as part of their field. For instance, they referred to specific properties of the mouse MO: mice had no amplification system in the oocyte; their epigenetic marks were reset in each generation; it was unclear whether there are nuclear small RNAs in mammals; mammals did not need small RNA inheritance because they had an intricate immune system; small RNA inheritance in mice is accidental. Others provided more general reasons. For example, they found mammals "boring" and findings on small RNA inheritance in mice either unclear or controversial. Interviewees criticized findings in mice for being purely "phenomenological," that is, lacking a mechanistic basis. They pointed out that all "mouse papers" they knew were seriously questioned by the community. They added that they were themselves very critical about them or did not believe the findings at all. Others questioned the significance of small RNA inheritance in mammals and argued that small RNA inheritance might only happen accidentally. If small RNA inheritance were only an accident in mammals, it would be undesirable to work on it. Some found stronger words and called all findings in the mouse field "worrisome:"

> "I don't know what to make of the most epigenetic, transgenerational epigenetic inheritance stuff in mice, and I've seen enough labs report stuff that probably is transgenerational, that's about as strong as I would go. To say that yeah, it is

> probably happening, but then, over and over again I hear about people not being able to reproduce the result and so that's very worrysome, right? I wouldn't wanna be basing my lab on those experiments, that's for sure."

Interviewees used similar arguments when describing how they felt about experiments in humans. Some pointed to the biological properties of humans, who made directed small RNA inheritance unlikely. More generally, interviewees expressed skepticism, and that they would "struggle with it." Experimental standards were once again a problem. Interviewees found that experimental standards would never meet those of fields that studied "proper" small RNA inheritance.

Some interviewees were reluctant to consider small RNA inheritance research in fish and flies as part of their field. While many emphasized that results in fish were very uncertain, they were equivocal about small RNA inheritance in flies. Some interviewees agreed that research on small RNA inheritance in flies was part of their field, real, or "legit." Others were uncertain whether to assign group membership. These interviewees described themselves as critical or doubtful and pointed to the fact that there was much more evidence for chromatin-based inheritance in Drosophila.

Most interviewees agreed that findings in plants were legitimately part of small RNA inheritance. The same was true for findings in protozoans and fission yeast. Although no interviewee excluded plant findings from small RNA research, plant research was far less often mentioned as part of the field, compared with research on C. elegans. This was also true for research on protozoans and fission yeast.

Almost all interviewees described findings in C. elegans as clearly part of the small RNA inheritance field. Some advanced the view that findings in C. elegans were the only clear examples of small RNA inheritance in the animal kingdom. Interviewees, however, who were working with a mammalian MO raised their doubts whether findings in C. elegans were real[25].

[25] "I mean, think of C. elegans, as an hermaphrodite, it must be different, even if you force them into a paternal lineage, it is really hard even there to get pure germ cells to study them on a molecular level. And in plants again it is also different, because where you define the organism? where does it start and where does it

Besides MOs, interviewees often assigned group membership depending on which type of small RNA was studied. Considerations about types of small RNAs were related but not entirely dependent on considerations concerning MOs. Different MOs had different small RNA pathways and different types of small RNAs had different tasks, depending on the MO. Some small RNAs were, based on the history of their discovery, linked closer to one MO than to another. For example, research on tRNA fragments, which was most prominently studied in mammalian MOs was most often excluded from being part of the field. Some interviewees expressed strong feelings, for example, that all of these findings were "bullshit." Research on piRNAs was on the other end of the acceptance spectrum. Almost all interviewees acknowledged that piRNA inheritance and all inheritance that had to do with transposon control, was part of the small RNA field. Other types of small RNAs employed as a criterion for group membership were miRNAs and siRNAs. Whereas most interviewees agreed that research on siRNAs was part of the small RNA inheritance field, several did not consider research on miRNAs as part of it. Reasons for the exclusion were that miRNA research was "boring" or replication issues.

Besides these two major categories, two more intricate differentiations were introduced as well. Some interviewees divided the RNA inheritance research field into two different fields: one studying transgenerational inheritance, the other studying intergenerational inheritance. Those researchers who described themselves as investigating intergenerational small RNA inheritance often emphasized that they considered transgenerational small RNA inheritance not part of their field. Others assigned group membership via specific standards, namely the relative "artificiality" or "naturalness" of the experimental system. Interviewees working with "artificial" conditions argued that researchers who worked with less artificial and less controlled experimental setups were not part of the field and vice versa. The respective degree of "artificiality" was identified by referring to kinds of triggers used, use of GFP as a reporter protein, whether MOs are inbred/isogenic.

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end? And the generations... these definitions even they are somehow blurred, as in comparison to a mammalian system where you can very clearly tell one generation from the next…"

4.3.3.2 Perceived instability of the small RNA inheritance field

In this section, I will present data on how interviewees perceived the dynamics of the RNAi inheritance field. I will ask whether they experienced their field as stable or unstable and survey their intuitions about "claims that harm the field.[26]" Before I survey these notions, it is necessary to make one remark. When interviewees talked about the "field" in this context, they used a broader notion of the field than when having to assign group membership. For example, interviewees who did not consider researchers working on mammalian MOs as part of the field nevertheless considered their works or claims as examples of what "harms the field." Similar to questions about who were the opponents of the field, interviewees were reluctant to identify researchers that harmed the field by name. Nevertheless, they suggested that they had several people in mind. One quote is illustrative for this situation: "Oh, you are getting me in trouble here, what are you gonna do with these quotes? Who's gonna read 'em?" After being told that the interviews would be anonymized, the interviewee responded: "Oh, so I can go nuts here!" but did not provide any name in the end.

4.3.3.2.1 Methodological and conceptual problems

Most interviewees characterized methodological or conceptual challenges within the small RNA field as a primary source of instability. Several issues were related to methodological limitations. One frequently mentioned issue was the problem that it is not possible to track a particular small RNA throughout generations. Interviewees identified the sensitivity of instruments as a second source of instability. Several interviewees were conscious of the possibility that they measure "strange background effects" or artifacts. The properties of small RNA inheritance contributed to these methodological challenges: the amounts of RNA available for measurement were tiny; phenotypes were often subtle, ephemeral, transient, and metastable. Monitoring ephemeral effects made it impossible to ascertain whether "the whole project is garbage." Studying effects of environmental conditions added to the challenges: confounding factors were a problem; it was hard to determine what was the decisive stimulus; the experimental setup was very sensitive. Interviewees were conscious of the fact that any unidentified stimulus could affect the phenotype, besides the stimuli

[26] "But whether they are the true mediator I don't want to get too far ahead on the evidence on that. It's those claims that harm the field… Being presumptuous about mechanism."

they were controlling for. Experiments that measure behavior were considered very hard to do, also in MOs that were "ridiculously simple."

While some interviewees suggested that the sensitivity of experiments needed to be improved, others argued that the methods employed are too sensitive[27]. They criticized current sequencing techniques for sequencing so "deep" so that also "garbage," that is, decayed RNAs, got sequenced and thus appeared significant. Because target prediction was so sensitive, it was possible to find "anything," a circumstance which interviewees characterized as a "huge problem" for the field. This results in tension. There was a need to improve sensitivity to ensure that researchers are not looking at noise, but at the same time, "better" and "deeper" technology worsened the problem. At a certain point, it would be possible to measure "stochastic" differences.

The other main set of problems mentioned were conceptual hazards. Many interviewees emphasized that the mechanism of small RNA inheritance needed yet to be uncovered and that this would be key to achieving wider acceptance. Until this happened, presupposing a specific mode of action could impede experimental setups. Along these lines, several interviewees criticized that phenomena were studied in the wrong way. For example, interviewees criticized researchers for asking the wrong questions concerning their material: they looked for immediate effects, although the inheritance of small RNA based changes was to be viewed as gradual. Because of this graduality, setting a timepoint zero was regarded as problematic. Separating observed effects from phenotypes produced by other processes, such as developmental constraints, was characterized as a problem.

[27] "And at the same time deep sequencing is a danger, because it is inherent to the DS approach that you go to the machine, with a sort of library, so first you have to make the library and library production involves PCR amplification. So this is a huge problem as well, if you work cleanly you are fine, if you know what you do, your fine but the traps that emerge because of PCR amplification are scary. So we know for example that DNA that flies around in the air of a lab can be amplified, so if you are not really careful you will have cross-contamination between libraries, and you will sometimes sequence dust. But the dust map to again p53 and then "uhh – isn't that cool!" So there is a huge second problem, that we tend to underestimate the danger of deep sequencing and thereby overestimate results we see in front of our screen without doublechecking probably. I think also the two biggest dangers in this area. And when it comes to epigenetic inheritance, which I think is a very slippery field, where there is certainly evidence and beautiful examples that it happens and there is similarly a lot of garbage out there. Which again is seated in the combination of wishful thinking – "wouldn't it be cool?"; plus experimental dangers such as deep sequencing."

Another set of conceptual problems mentioned was that of investigators' preoccupations with specific aspects of the phenomenon. For example, interviewees criticized the focus on sperm inheritance, specifically in the mammalian field[28]. To them, it was unclear "if you do the math" how the sperm could faithfully transmit small RNAs. Also, interviewees regarded the prevalence of a "programming" metaphor as problematic. One interviewee mentioned having been pressured by reviewers and mentors to interpret findings as a result of directed programming.

4.3.3.2.2 Reproducibility

Interviewees identified the reproducibility of experiments as a significant problem. Most of the time, interviewees regarded the same papers as problematic. But if one surveys research papers authored by interviewees, papers with replication issues are still frequently cited. In what follows, I will discuss notions of reproducibility in more detail.

Several interviewees expressed strong opinions concerning replication, such as being very "worried," "concerned," "nervous," or "troubled." They identified conferences or private correspondences as the most convenient way to learn about problems with reproducibility. Others reported their own experiences of being unable to replicate published findings. Interviewees considered the existence of too much "bogus" literature as one major problem. Many interviewees raised this concern specifically about research on small RNA inheritance in mammals. They pointed to the most "famous" studies in mammals, which they also knew to be not replicable.

Replication issues were not confined to mammalian MOs. Specifically, one early paper which gave the small RNA inheritance field a considerable push forward was classified by most interviewees as not replicable. They defended the paper by arguing that the conclusions were right, despite the experimental data being not reproducible. One

[28] "The other thing is the shift to the female germline, at the moment we are all looking at the male germline because of some – in mammals or at least in mice – because there is these advantages of not having to disentangle maternal behaviour from paternal behaviour. Because only the moms are involved in the uprising of offspring, then only through a very short time window through the mating the males are exposed to the females. And there is no alterations in the… intra uterine environment potentially playing a role."

researcher even hinted that at the time this paper was published no one believed the findings because of methodological problems. No critique was raised at that time, however, because the community thought that the conclusions were correct nevertheless. This situation of tacit knowledge about replication problems was summarized by one interviewee as follows:

> "And again it is quite interesting because if you wouldn't know anything about the field then you would say 'woohoo there are two science papers proving tRNA fragment mediated inheritance of information, so that's a fact!'… But then if you would ask people, the majority would say, even people working in the field and as you heard me say, I am not convinced that they have proven a thing."

4.3.3.2.3 Research and researchers who harm the field

While interviewees characterized the problem of reproducibility as a general problem and not necessarily related to bad practice or problematic researchers, many also described a type of researcher who they considered to be bad for the field in general. Most interviewees were concerned that because these "bad" researchers produced poor-quality papers, the whole field suffered from criticism. They worried about researchers who wrote "complete bullshit" and thus made the whole field look bad to the outside, especially to practitioners within more orthodox professions.

Interviewees described the literature as "contaminated" with instances of poor-quality research. They perceived small RNA inheritance as a slippery field which produced a "lot of garbage," despite "very beautiful" examples. Interviewees were very conscious of the fact that their field attracted much interest and that many researchers wanted to participate. They often expressed the opinion that researchers joining the field did not do a good enough job, which made the whole field suffer. Along these lines, interviewees were worried about too many "free-riders" who were only "average" talented.

Interviewees complained about researchers who were "driving the standards of the field down." As a result, they had to fight against "crap" in the literature. One source of contamination identified was research in the mammalian field, in which there were only a few "believers" who claimed "outrageous things with minimal data." Some worried that low standards in some areas of small RNA inheritance research, such as the human and mammalian field, would infiltrate other areas and drive down the standards of research there. In this context, interviewees were often quite ambiguous. On the one hand, they claimed that they feel that the "bar is a bit too high," referring to their own experiences with publishing. On the other hand, they stated that some published papers should have been much more scrutinized ("even in plants!")[29].

4.3.3.2.4 Alliances that are bad for the field

Interviewees identified the appeal of their research field to third parties as another reason why they perceived their field as unstable. Besides "free-riders," two other groups, namely non-scientists and creationists, were mentioned as parties who took too much interest in small RNA inheritance. Issues about public interest were often discussed along with "troublesome" papers. Interviewees specifically pointed to a study on small RNA inheritance in holocaust survivors. This study was unanimously characterized as unacceptable by the standards of the small RNA inheritance research field. Nevertheless, this paper attracted more public attention than any other paper. Others argued more generally that the appeal to the public was dangerous because "the public" was intrinsically fascinated by the idea that inheritance could be changed through behavior. Another researcher emphasized that small RNA inheritance results were not ready for "the public" yet.

Interviewees characterized creationists as another party that takes too much interest in small RNA inheritance: "You gotta be really careful what you put out there because it's what creationists and intelligent design people grab." It was a common fear that by providing evidence that Neo-Darwinism did not describe all instances of inheritance, they opened a little "crack in the door" for creationists[30]. But interviewees considered it as pivotal to have

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[29] "Absolutely yeah, grants yeah, journal articles yeah… absolutely... So... yeah, I think… I think that's true to all scientific endeavours that there are always different points of view… I do think it is a shame that the lack of rigor in some of these studies that I was talking about, especially in humans, has sort of filtered over into the model organisms. And for that also human studies that were done rigorously, so some people have a really hard time with those grants and papers and absolutely… I mean, we all have a hard time publishing a paper but I sometimes do feel that the bar is a little too high… At the same time I think the field has got pretty big and quite mature at this point and I don't think it is as awful as it used to be."

[30] "Yeah I, so I don't use the word Lamarck deliberately in my papers. But we have a paper where we show the RNA going from neurons to the germline to cause multigenerational silencing and when you google the word Lamarck and our name a news report on our work will be the first thing that comes up...Just say Lamarck

a united front on how evolution works, even with people who did not believe in small RNA inheritance. They reported that after publishing findings they immediately received emails by creationists who try to get them to "say things." Although interviewees felt that this asymmetrical alliance was not their fault, they were worried about people who are part of their field getting "a little too nutty" by talking about evolution in "strange ways." These people became hard to discern from creationists.

Along with the fear of researchers getting "too nutty," interviewees pointed out that they were unhappy with the use of the term "Lamarckian." Such statements were in tension with the prevalence of the term "Lamarckian" in their writing. Most commonly, they criticized the use of this term as a marketing trick. Researchers used the term to sell their research, get the attention of people, be controversial, excite the audience, marketing, or make what they are doing more interesting. Some interviewees maintained that the use of the term "Lamarckian" was not helpful, as it was an overinterpretation. Some worried that this practice made the small RNA field unpopular because it gave the impression that researchers were downplaying genetics. Others, however, insisted that using the term "Lamarckian" as a rebellion is warranted. They argued that they reached a point at which they found such rebellion justified:

> „You know when I think about it… Lamarck's thesis has some emotional in it, it has all these suppressed scientists, that were, you know ignored. That's an understatement, so… the way I look at it when you say Lamarckism, it is almost like a rebellion. It is about the truth is out there, there is an alternative explanation and... so this is the non-rational… And you know what... sometimes when you are constantly pressed you get these frustrations "

RNA...okay?...That will be the first thing that will come up and that was written by the discovery institute which is a creationist website, okay? And the commentary itself is quite unscientific and is quite lacking in clarity and rigor but its about our work. It's not what we said, it's what they interpreted so I think as scientists we are trying to be careful as possible in spelling things out. But when it comes to relating to the media we do have to be very careful because people interpret the word Lamarckian or Lamarck in different ways."

4.3.3.2.5 Competition

Interviewees described their research field as very competitive in general. Some felt that the experienced density and competitiveness of the field makes it desirable to leave the field and look for small niches. Others enjoyed being in a field that challenges dogma, is risky, exciting, and competitive.

Several interviewees reported that the high degree of competition made collaboration harder. Interviewees acknowledged however that those who cooperate were, in the end, better off, because they demonstrated that they had the resources for cooperation and could form group ties. Group ties, in turn, facilitated publishing. All interviewees working in the mammalian field experienced competition as detrimental. They reported many bad encounters and hostilities. They explained that the public attention their field received made competition even stronger: "everyone just tries to survive." These interviewees, however, emphasized the importance of organizing into "sub-hubs," which locally reduced the competitiveness to a tolerable level:

> „So very bad anecdotes, which of course also leaves a very bad flavour of the field. And at the same time then triggered by my RNA publication in mouse this has kind of prompted follow up RNA publications and I have… I do have the feeling that it is… it's a bit hostile, the people might seem nice when you meet them, they even try to suggest collaborations and so forth… But in the end it's a hard business, and then everyone just tries to survive… But there is subgroups, there is certainly for instance what I can talk about from my first hand experience this Zurich sub-hub… So there is certainly a good hub there and people are not that hostile, because they also help each other out and… try to coexist."

4.3.3.2.6 Orthodoxy and gatekeepers

Many of the groups described as opponents were also described as "orthodoxy." Several interviewees reported sanctioning by orthodoxy in various elements of their scientific life, such as receiving grants and getting journal articles published. Some explained the high degree of sanctioning by the fact that some aspects of RNAi inheritance were hard to believe. Interviewees emphasized that editors and funding bodies were usually more conservative than the average investigator. This made it harder to publish a paper compared to other fields. They experienced reservations towards their publications and grant applications. Interviewees reported that it usually took a “big name” publication for others to publish similar papers. As a no-name, it was almost impossible to publish small RNA inheritance findings. They also complained that their reviewers usually did not understand the methods applied and the nature of the phenomena, which made them demand modifications that are methodologically impossible to deliver.

4.3.3.3 Research traditions and model organisms

In this section, I will survey several aspects that contribute to the plurality interviewees are experiencing. These aspects go beyond the plurality of Lamarckian vs. Neo-Darwinian theories of evolution and inheritance. I will discuss how different research traditions, the role of the respective MOs, and trends in the very recent history of the small RNA field determine plurality. Specifically, small RNA biology has a history of phases of merger and fission with different disciplines and subdisciplines. This history defines the professional identities of small RNA inheritance researchers and their outlook on their discipline. The history of the small RNA field and the contingencies of its establishment have a significant impact on the ways the field is constituted, and how alliances, problems, and methodological standards are negotiated.

4.3.3.3.1 The Golden Age

Interviewees developed a standard narrative of the first years of research on small RNAs, starting in 1998. Three “mergers” were considered essential for the small RNA field to become established:

1) the merger of plant findings with the phenomenology of RNA interference in C. elegans;

2) the merger of research on miRNAs with the RNAi field;

3) the influx of biochemists to the field.

As discussed in Chapter Two, the history of small RNA inheritance commences with plant science in the 1980s and 1990s and can be traced back to the works of Barbara McClintock in the 1950s. The standard history of the small RNA field starts however with Fire and Mello's discovery, which marks the point when "plant people were taken seriously." Interviewees working in plant science shared a narrative of being very conscious not only about the phenomenology of small RNA interference but also about its link to heritable responses before 1998. Interviewees pointed to papers published in the 1990s that reported such findings which were not accepted by the research community. With the realization that the "weird plant things" and the "mass psychosis in C. elegans" were based on the same underlying mechanism, RNAi, small RNA researchers in C. elegans could initially draw considerable amounts of their knowledge from knowledge established in plant science. One interviewee even emphasized that all RNAi research of the early days was inspired by research in plants.

Interviewees narrated the first years after 1998 as a tale about a golden age. Some called it the "most rewarding stage of my scientific career." A "bottleneck" led to the mergers of several research fields. This merger was initiated by the realization that these fields were interested in the same underlying phenomenon. As a result, a new "domain of biology unfolded in front of us." Interviewees specifically emphasized the role of meetings in this early stage. During this time, many people working on different model organisms and different problems entered the field. Although all aimed to uncover the same underlying phenomenon, the exchange of ideas was intense because there was no fear of getting "scooped." Interviewees described conferences at that early time as extremely rewarding. They reminisced that everyone gasped after every talk because all new findings were exciting and relevant for the entire emerging field[31].

The second key merger was that of the miRNA with the RNA interference field. This merger was enabled by the realization that miRNA and siRNA pathways were linked. At this time,

[31] "there were a lot of them… I am teaching a course next spring that focuses on that four-year period of time where we came from ignorance to enlightenment… So now we know what we don't know… and four years later we knew so much… Yeah, so many individual contributions... its hard to say that there was a singular leader… there was a lot of communication… first there was a meeting… I didn't get to go to, my daughter was just born... but my graduate students did… and they were quite excited… the meeting in Colorado... Keystone meeting in 2002 in march… and what they described… somebody gave a talk and everybody would just gasp… "so cool!"... and then somebody would give a talk on another topic and with the same: "oh really? Amazing!" At least with the talks: this enzyme is involved, and we found that RdRPs are required, and we found another RdRP that is not required but it seems to enhance it... and small RNAs are associated with this process, and we found them in this system, we found it in that system… There were just so many things to find…"

miRNAs were known to occur in several MOs, which lead to the conjecture that RNAi might be widely occurring as well. As a result, miRNA researchers working on other model organisms entered the field. Specifically, the realization that RNAi mediated silencing could be found in mammals gave the field much more visibility. The discovery of RNAi, in turn, gave the miRNA field a considerable boost, and "miRNAs became like transcription factors." Another interviewee pointed out: "you could publish anything if it was governed by a miRNA." The merger had mutual benefits. As soon as miRNAs were considered part of the RNAi phenomenon, it was easier to get people to believe that small RNAs were real. From a methodological standpoint, miRNAs were excellent targets to work with because they were essential for development and produced clear phenotypes. A smaller but similar merger was that with research on transposons when transposons were shown to be RNAi regulated. Also, in this scenario, researchers working in the RNAi field could profit from protocols and tools established by transposon researchers.

The perceived "rush" on miRNAs was, however, also referred to as a reason for why researchers did not study the role of small RNAs in inheritance. Researchers got "distracted" with the promises of studying miRNAs. miRNAs seemed to be involved in phenomena that attracted a lot of funding, such as diseases. One interviewee pointed out that while around 1998 people interested in inheritance did not jump onto the "small RNA wagon" and failed to merge, the people on the "small RNA wagon" jumped onto miRNAs.

"Once miRNAs became part of the field a lot of crucial people could get recruited into the field." Amongst these people were not only researchers interested in development but also biochemists. The recruitment of "hardcore chemist people [sic]" triggered a blast of activity. They established many protocols, that is, they unified methodology and some of these protocols are still the gold standard today. Through this merger, the field started "moving at break-neck speed." While many interviewed geneticists acknowledged the contributions of biochemists to the field, interviewed biochemists acknowledged that geneticists made the emergence of the field possible:

> "So RNAi was discovered by geneticists, geneticists are a really special breed of biologists, because they don't require physical models in order to conceptualize things. All the rest of us have to be able to draw, and are trained, makes it even

more rigid, that we have to be able to draw little cartoons, of individual molecules doing shit, right? Geneticists don't need this, they only need two things to make them happy, an arrow, at the end of a line and a repressive line, at the end of the line, that's it, that's all they need, right? As a result of which they can live with conceptions but they don't have to have detailed models in order to begin to build a pathway, report a phenomenon, right?"

As a result of these many mergers, the field grew bigger and bigger, which caused the field to bifurcate at a particular time. Researchers focused on different kinds of small RNAs and parted ways. Interviewees described a "miRNA wave," a "piRNA wave," and a "siRNA wave." As of now, miRNAs were often considered to have split into "cancer and medical" entirely. Interviewees however described miRNA researchers as unhappy because of a prevalent focus on piRNAs and siRNAs in the small RNA field.

It is essential to note that while the narrative of the "golden age" was widespread, the absence of another narrative, and in some cases, opposition against a particular narrative, was noteworthy. I am much indebted here to exchanges with members of the MCTS at TU Munich, amongst them Sophia Rossmann und Georgia Samras, who study the situatedness of epigenetics in different human-related contexts. They often encounter a narrative about curing diseases or improving human health (personal communication). This narrative was entirely absent in interviews. It was even actively rejected by some interviewees. My interviewees emphasized that it was not the goal of the field to cure every disease[32]. Another interviewee criticized the "medical profession" for their perspective on small RNA research.

4.3.3.3.2 Research communities

In the last section, I surveyed interviewees' accounts of different sets of expertise merging to become one field. As a result, until today, several disagreements about experimental and

[32] "And so, I mean rather than Mukarjee in his book, you know we don't want to say anything about how we gonna solve every disease… which I think is again a general problem in biomedical science that there tends to be too much excitement sometimes, in raising expectations for cures and all sorts of things…"

evidential standards are dominant. Some interviewees indicated that the group structure loosened considerably. For example, in the early days of the field, it was not feasible to publish experiments that “weren’t done well” because the community around small RNA research would not accept them. Other papers were not questioned by the community, although interviewees had their concerns about them when they were published.

In respect to findings on small RNA inheritance, interviewees working on plant MOs had an ambiguous position. On the one hand, they reprehended the small RNA field at large for underappreciating some critical findings on small RNA inheritance in plants. On the other hand, because these phenomena were quite established within the plant community, they did not have to face as much backlash as “animal people.” While small RNA inheritance was not followed up for a while in animals, there was constant interest in these phenomena by the plant community. For animal researchers…

> “there was a feeling, I think even for the investigators themselves, I think they felt that if they would push that too hard, people might… you know, the original results”…”And so that’s why I think why there was a lot of resistance or and even reluctance on the part of the discoverers to make much of a big deal out of it.”

For the plant community, the question of how “established” small RNA inheritance was also had another twist. That small RNAs were inherited in plants was accepted by the industry, who were sometimes even collaborators of plant small RNA researchers. Considerations how small RNAs might cause the sterility of certain plant pests were of high importance to crop companies.

Similar to the plant field, the structure of the C. elegans community was also decisive for the acceptance of small RNA inheritance. In this case, size mattered a lot. Compared to communities studying other MOs, the C. elegans community was considerably small. In the C. elegans field, most researchers were “children,” “grandchildren,” or “great-grandchildren” of Sidney Brenner, the founder of the field. A majority of the C. elegans community was either supervised by Brenner or their supervisors were. In the early days of research on small RNAs, this considerably facilitated the division of labor. Participating researchers could lay exclusive claim to which aspect of small RNAs they wanted to study.

This privilege was granted by the whole community. Even more so, overstepping one's assigned research focus was reprehended if not sanctioned by the community.

It is useful to contrast the structure of the C. elegans community with that of the mouse field. Owed to the Sidney Brenner family-tree, small RNA related phenomena, including inheritance, were known and accepted by most parts of the C. elegans community. In contrast, only a tiny part of the mammalian community was even conscious of small RNA inheritance. A typical response in the mouse community was cautious skepticism. Privileges for small RNA inheritance researchers, assigned by the mouse field, did not exist.

4.3.3.3.3 Methods and evidence

One interviewee described how looking at the effects of injected dsRNA in the next generation was, at first, a purely methodological facilitation. Small RNA injections considerably damaged the tissues of C. elegans. Because the 1998 paper had reported silencing effects in the subsequent generation, it appeared an excellent methodological choice to look at effects in the next generation to avoid the influence of tissue damage. Quite generally, small RNAs became the epigeneticists' favorite tools because they were easy to interfere with compared to other epigenetic marks. For example, modifying and directing chromatin modifications presented a significant methodological challenge. While several small RNA inheritance researchers agreed that other processes of epigenetic inheritance might be important, methodologically it was not feasible to study them. Small RNA inheritance researchers thus considered small RNA inheritance the prime candidate for mechanistic understanding. In this context, it is important to note that specific properties of MOs made them prime candidates for studying small RNA inheritance. For example, C. elegans has many well established, classic phenotypes that are easy to study.

Although small RNA inheritance researchers considered the occurrence of small RNA inheritance unquestionable, evidential standards, often imposed from the outside, were regarded as problematic. For example, the demand that all results had to be backed up with a genetic mutant, the "killer mutant[33]" was considered hard to fulfill for researchers

[33] "And so, I mean rather than Mukarjee in his book, you know we don't want to say anything about how we gonna solve every disease, you know by taking enough (?), or whatever it is… which I think is again a general

studying small RNA inheritance. Several RNAi inheritance researchers criticized that finding a single gene responsible for an effect was necessary for a high impact paper. This demand, however, might not be in correspondence with the goals of studying small RNA inheritance. For example, finding a clear, Mendelian distribution of traits was not to be expected in small RNA inheritance experiments. The demand for a "killer mutant" might, thus, not resonate with what was considered a positive result in small RNA inheritance. Interviewees criticized that the standards were unjustly high.

The limitations of small RNA research in mammals was a common theme amongst interviewees. One interviewee, working with plants, argued that mouse researchers were methodologically at the stage of plant breeders in the 1920s[34]. Also, several interviewees pointed out that they refrained from mouse research because of the costs and risks of doing experiments in mice. Expertise on how to build mouse lines was mentioned as a limiting factor. One interviewee admitted being very interested in working on mice but was not ready to spend all resources on this research. Whereas it was possible to do almost any experiment in C. elegans with reasonably little money, all mouse experiments were difficult and expensive.

The expertise required for working on mice was often regarded as a problem. Interviewees working on C. elegans pointed out that although they were critical of mouse findings, only very few people had the relevant expertise to judge the experiments. But also within the mouse field, there was much dispute over the right way of doing small RNA inheritance experiments. It is also important to note that several milestone papers were only possible because specific sets of expertise were combined. For example, the most influential paper on epigenetic inheritance in mice was a collaboration project between researchers trained

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problem in biomedical science that there tends to be too much excitement sometimes, in raising expectations for cures and all sorts of things…"

[34] "And so I think that's partly the explanation, that it is rare and hard to see, also people are still at their stage as plant breeders were in the 1920s, of looking at very broad phenotypic phenomena like, you know, maternal attention, prenatal care, you know, these very general things rather than genes. And I think it is much more rigorous, becomes much more believable if you actually have genes that are inherited."

in behavioral experiments in mice and researchers who had specialized in bioinformatical analysis of small RNAs.

Methodological challenges were not limited to mouse studies. For example, several interviewees problematized that the Drosophila field was traditionally a chromatin research community, except for the piRNA field. But piRNAs were hard to study because they were primarily expressed in the germline. For experiments in the germline, there were fewer tools compared to tools for studying small RNAs in somatic tissues. A similar asymmetry could be found in the study of male and female contributions, specifically in mammals. Whereas sperm contributions had been extensively studied because it was easy to separate the father from the upbringing of the pups, expertise on the female germline was lagging.

One last methodological issue mentioned was that of whether transgenerational effects of small RNAs should be studied in "artificial" or "natural" conditions. Interviewees working in the mouse and the C. elegans community were concerned about that issue. At the same time, such arguments were mobilized to criticize research in other MOs. Interviewees in favor of "natural" conditions argued that they used "real environmental triggers" and that "natural" phenomena were much more believable. They criticized researchers' use of GFP expression as a readout because artificial GFP insertions might produce artificial effects. Some argued that they found the use of transgenes as reporters not convincing in general. Several interviewees were concerned about the artificiality of their systems and the fact that they had to "bend over backward" in order to produce their inheritance phenotypes. Interviewees faulted others for using inbred mouse-lines because those did not mirror conditions in nature. Interviewees working on mice criticized other model organisms for having no "pure" germline; in contrast, they believed that doing transgenerational research on mammals was "nice and clear."

Other interviewees defended the "artificial" character of their experiments. For example, they argued that the use of isogenic C. elegans lines did not make the experiment artificial but helped to control the experimental conditions. They criticized researchers who did not use inbred mouse-lines because, in so doing, they admitted too many variabilities. By using the more "natural" mouse-lines they lost track of which effects were caused by small RNAs and which were caused by genetic variations within the mouse-line. They defended their systems by claiming that what they wanted to do is to establish a "proof of principle." Because naturally occurring phenomena were inherently plastic, it was too hard to study them. Thus, it made much more sense to control for particular conditions.

4.4 Discussion

In this section, I will provide an analysis of the results presented in section 4.3. I will organize this analysis around the three central goals of this chapter I listed in section 4.1. The first aim is to reconstruct scientists' positions concerning SP. Based on the findings of section 4.3, I will provide an analysis of interviewees' interpretations of plurality and pluralism. The second aim is to develop a "philosopher's toolbox" for describing the heterogeneity of the encountered positions concerning plurality and pluralism. The "philosophers' toolbox" will provide a collection of conceptual tools to describe the heterogenous attitudes of researchers within a field that displays plurality. Specifically, terminology will be developed to differentiate the various attitudes of interviewees towards plurality. Thus, while the first aim will approach researchers' positions "bottom-up," and purely descriptively the second aim will be "top-down," and normative. While reconstructions of interviewees' positions "emerged" organically during the process of coding, categories employed for the "philosopher's toolbox" were determined a priori. The third aim is to address the question of what is at stake in a field that displays plurality. Applying the conceptual tools introduced in section 4.2, the dynamics of the small RNA inheritance research field will be analyzed. Specifically, grid/group analysis will be employed to describe 1) the group dynamics within the small RNA inheritance field, and 2) the group dynamics of other groups that small RNA inheritance researchers interact with.

4.4.1 Reconstructing researchers' positions

In this section, I will provide a reconstruction of interviewees' positions towards plurality and pluralism. This approach will be bottom-up. Rather than starting with necessary and sufficient criteria, I will start by surveying the heterogeneity of the positions encountered. As seen in section 4.3.1, interviewees did not provide a unanimous interpretation of what defines the plurality in the field. Factors that shaped interviewees' positions concerning SP were attitudes about the "goals" of science, the status of their knowledge, and scientific practice as a source for SP.

In my attempts to find out what motivates the pluralist attitude of some researchers, I was struck by one aspect in particular. Researchers motivated their pluralist tendencies by referring to how they perceive the character of knowledge: as provisional, limited, and subjective. While they accepted that *human knowledge* naturally involves pluralism, they did not allow for pluralism about the "nature of the world." In conclusion, interviewees did not reject monism. Instead, interviewees motivate their pluralism through "non-absolutism" or "fallibilism."

In section 4.3, I pointed out that philosophers who support scientific pluralism usually reject monism. As the results show, small RNA researchers that sympathize with pluralism do not. This prompts a question. If small RNA inheritance researchers do not support a "tenet" of SP which philosophers seem to regard as essential, can we characterize them as scientific pluralists? Or do they fall outside of the category of SP altogether? Deciding to not call interviewees pluralists at all is not satisfying. Several interviewees expressed positions that included a quite favorable interpretation of plurality. What should we say about this group? Are they pluralists? However we decide the question, it is a striking observation that scientists' pluralism seems to diverge from philosophers' scientific pluralism.

There are two ways of addressing this problem. One way would be to modify the criteria for SP to include the interviewed researchers. Another way would be to admit that the dichotomy of pluralism vs. monism does not apply neatly to my researchers. In their context, non-monism does not motivate pluralism, but non-absolutism does. As it seems unintuitive to describe all interviewees as monists, we have to admit that at least for practicing researchers, positions in-between pluralism and monism are possible.

4.4.1.1 The role of "theory" in interviewees' pluralism

Another critical aspect that was brought to light by my case study is that even those researchers who sympathize with SP nevertheless experience plurality in their field as destabilizing. Some worried that plurality destabilizes science and raised "traditional" concerns, such as the problem of demarcation. This is connected with another issue, namely the shifting "subject" of SP. In their publications, many interviewees proposed a "Lamarckian" interpretation of their findings and claimed that "Neo-Darwinism" does not provide an account of the phenomena they are describing. Because of this emphasis on "theories" in their publications, I speculated that if small RNA inheritance researchers were pluralists, they would be pluralists about theories. If they were monists, I hypothesized, they would articulate monist interpretations about a plurality of theories within their field. This, however, was not the case[35].

Interviewees did not emphasize the role of "theory" and were also reluctant to agree with a Lamarckian interpretation of their findings in interviews. In addition, if they used "theory," the term did not have a fixed meaning but was used interchangeably with "explanation" and sometimes "mechanism." Even more so, and contrary to my initial interpretation, interviewees identified other sources of plurality, beyond that of theories. Issues about model organisms (MOs) and research traditions within small RNA biology were identified as possible sources.

We find a tension here. A tension between a) the plurality of theories encountered in publications of RNA inheritance researchers and b) the plurality of, e.g., repertoires that emerged through interviews. In the next sections, I will show how "theory" becomes a means to represent the field to the outside, while it is of minor importance on the inside. I will display an essential difference. "Theory" is central when confronting opponents, while it is of minor importance in intra-field communications.

There is also a further point worth mentioning concerning the implicit philosophies of interviewees. This point connects to what was discussed above. Alongside model organisms and research traditions, "scientific practice" was also identified as a source of plurality. Issues tied to the day-to-day practices in the lab, experimenting, hypothesizing, interpreting were most often associated with scientific pluralism. In this vein, interviewees focused less on pluralism concerning multiple researchers with different approaches and more on the single researcher as a pluralist who has different ways of accounting for the phenomena.

[35] Expecting actors' theoretical pluralism is also predicted by the ethnomethodological concept of "members' archaeology:" After schisms in a field, such as the disciplinary schisms in small RNA biology, members' often narrate their recent disciplinary history as a history of disagreement about theory (Lynch, Livingston and Garfinkel, 1983, 224).

Almost all interviewees mentioned that entertaining different perspectives, theories or interpretations at various stages of their research is pivotal. Another critical aspect of scientific practice was tied to the above-described asymmetric importance of "theory." While on the level of discourse directed to the outside tensions between a "DNA only" and an "RNA and DNA" perspective were considered problematic, this tension seemed to disappear on the level of scientific practice. The experimental systems small RNA inheritance researchers work with are a synthesis of knowledge from both perspectives. This becomes evident in experimental design and assumptions before the experiment. For example, when researchers perform "crossing experiments" in order to create the initial conditions of their experiments, they need to have small RNA-based and DNA-based aspects of their experimental design in mind.

4.4.1.2 Metaphysical and epistemological arguments

The above discussion highlights several discrepancies between philosophers' SP and scientists' SP. There are differences in focus and conceptions of the role of scientific practice. One aspect, however, was similar for both philosophers of science and interviewees. Both groups motivate their pluralism (or rejection of pluralism) mainly by two types of arguments: either epistemological or metaphysical. Epistemological arguments for pluralism were often tied to "non-absolutism." Interviewees pointed out that because knowledge is provisional, several theories should be in play. In this spirit, their epistemological arguments overlapped with Chang's proposed "benefits of tolerance" but not with "benefits of interaction." That is, they did not emphasize co-optation, integration or competition but instead emphasized the benefits of "division of the domain."

One of the interviewees' epistemological positions was a "pluralism now, monism in the end" outlook on science. I will call this form of pluralism "epistemic heuristic pluralism." Interviewees who qualify as epistemic heuristic pluralists acknowledge the epistemic benefits of plurality but believe that, in the long run, we will uncover, or should uncover, one true account. These interviewees did not specify or elaborate on the point in time when they would switch from pluralism to monism. In other words, they take it for granted that as long as pluralism is epistemologically useful, it should be maintained. I classify these researchers as pluralists: epistemic heuristic pluralists see plurality as something positive. And this makes it impossible to call them “monists.”

The next category of epistemic pluralists amongst interviewees are those concerned with the provisional character of knowledge. Their pluralism is mainly motivated by reference to the limits and limitations of knowledge. They believed that knowledge will always be provisional. These interviewees were more inclined towards non-absolutism than non-monism. Therefore, these interviewees can be rendered as non-absolutist (or fallibilist) epistemic pluralists.

The last category of epistemic pluralist interviewees is those that take scientific practice to motivate pluralism. They should not be confused with “heuristic” pluralists. While “heuristic” pluralists motivate their pluralism with the (short-term) pragmatic epistemic benefits of pluralism, epistemic practice-oriented pluralists are those who take their experiences with scientific practice, that is, their experience when performing various epistemic activities, as the source for their pluralist convictions. They do not believe that pluralism should, at one point, turn into monism.

It is interesting to note that those interviewees who provided metaphysical arguments for SP did not refer to what “the world” is like but restricted their arguments to what they thought “biology” is like. Vice versa, those interviewees arguing against pluralism based their arguments on what “nature” is like. It is worth asking what interviewees were referring to when discussing what biology “is like.” Do they make assumptions about the “essence” of biological phenomena or are they describing the contingent state of the research field?

In Chapter Two, I discussed Mitchell’s arguments for integrative pluralism in biology. Mitchell’s pluralism resonates to a degree with researchers’ accounts. That is, some refer to the abundance of “exceptions” in biology as one source of pluralism. As discussed in Chapter Two, the contingency brought about by “exceptions” is one fact on which Mitchell bases her metaphysical pluralism. Other interviewees putting forward metaphysical arguments referred to the “messiness” of biological phenomena. Because of this messiness, several approaches are required for the phenomena to be fully understood. This position resonates with the work of philosopher of science Nancy Cartwright (1999), whose work I will discuss in the following chapter.

In conclusion, metaphysical arguments can be divided into two groups: those referring to "exceptions" and those referring to "messiness." It remains to ask whether interviewees use these terms to describe the field they are working in or use these terms to refer to some "essential" properties of biological phenomena. I take claims about "exceptions" in biology as claims about the unique character of biological phenomena. That is, they argue that biological phenomena have certain "essential" properties that make them susceptible to plurality. In contrast, remarks on the "messiness" of biology were more likely to be used to describe the characteristics of biology as a discipline. Such remarks were primarily offered by interviewees who were trained in a different, "harder," "more fundamental" science. In speaking of the messiness of biology, they referred to how anomalies are handled and how exceptions to the rule are tolerated within biology. And these interviewees contrasted this to the discipline in which they were brought up in. In other words, these interviewees claim that biology allows for more plurality than other disciplines. This is however determined by contingencies in the trajectory of biology as a discipline. It is thus possible to divide the "metaphysical" pluralists into two groups: contingentist metaphysical pluralists and essentialist metaphysical pluralists.

4.4.1.3 "Integration" and "non-integration" in scientists' pluralism

Researchers were asked whether they considered DNA and RNA inheritance as either integratable or non-integratable. Due to my focus on "theories," interviewees were questioned explicitly about the integration of theoretical accounts covering DNA and RNA inheritance. The question of how to accommodate these different approaches was of great interest to interviewees. Concerning the case of "DNA" and "RNA" inheritance, interviewees opposed any form of "non-integrative" pluralism. Interviewees emphasized complementarity, non-contradiction, and that DNA and RNA inheritance represent different "modes" of inheritance. While claiming that small RNA inheritance cannot be explained by or be reduced to, a DNA-based explanation, interviewees did not agree that both modes can and should be studied separately.

In this context, it is important to mention a striking exception. Researchers who mentioned creationism or other religious endeavors, treated these as possible candidates for a scenario of “non-integrative” pluralism. These interviewees rejected the possibility of synthesizing, for example, creationism and evolutionary theory. Nevertheless, they found that phenomena uncovered or methods developed by one endeavor might be profitable for the other endeavor. Thus, the tendency to favor integrative or non-integrative pluralism depended on the peculiarities of specific approaches. For example, interviewees’ attributed certain attributes to the program of “creationism” that make it a candidate for non-integrative pluralism.

It is, however, important to note that those interviewees who denied that small RNA inheritance is a source of plurality within their research field employed arguments similar to those used to argue for pluralism by other interviewees. Those sympathizing with pluralism used “complementarity” as an argument for an integrative version of pluralism. Those disagreeing with a pluralist interpretation, however, also pointed to “complementarity” and non-exclusiveness of DNA and RNA inheritance in order to justify their position. This observation resonates with an issue that might be of concern for a scientific pluralist. Scientific pluralists who champion “integrative” SP, such as, for example, Mitchell, are often criticized by philosophers who favor non-integrative approaches. Such critics are concerned that the integration of the approaches in question might signal to some that unification is within reach. And if unification seems within reach, efforts will be concentrated on achieving unification and the benefits of plurality will be lost. Integrative pluralism, to such critics, is monism in disguise. Helen Longino, whose pluralism on the scientific study of behavior I discussed in the previous section, is one of such critics.

Thus, there are at least two different interpretations of approaches that seem candidates for integration. On one interpretation, they speak for unification, at least in principle. On another interpretation they signal complementarity and non-exclusiveness. This shows what I tried to explain in section 3.1. Whether two approaches are considered candidates for integration or not is never determined by an essential property of the subject-matter concerned. Rather, such interpretations are part of the decision tree I sketched in Chapter Three.

In Chapter Three, I differentiated between three decisions for researchers who are confronted with more than one approach:

a) the judgment and ensuing decision whether the presence of several approaches is recognized as an instance of plurality within "legitimate" science,

b) the judgment and ensuing decision whether the approaches at stake can be integrated or not, and

c) the judgment and ensuing decision whether the respective integration or lack thereof is taken to be beneficial or not.

Decision c) involves the question of whether we should be pluralists about these approaches. As discussed in Chapter One, these decisions will seldom be unanimous within a research field and heterogeneity within the field concerning these questions should be expected to be the rule. This is precisely what one finds concerning the interviewees' implicit philosophies. Whether the specific scenario of their field constitutes a case of plurality is answered by some with "yes" and by others with "no." It is quite remarkable, however, how little heterogeneity one finds concerning the question of whether the approaches of interest are integratable, or not. Still, this is different for the "opponents" of small RNA inheritance. Interviewees described the "opponents" of small RNA inheritance as supporting a "non-integrationist" interpretation of the concerned case. I started this section with an observation. The ways in which philosophers of science motivate scientific pluralism do not tally well with my scientific interviewees' ideas of pluralism. I tried to reconstruct elements that came up in interviewees' accounts of pluralism: practice-oriented reconstruction of pluralism, metaphysical or epistemological motivations, and an attitude on how to relate the plurality of approaches. Having presented this "bottom-up" approach, I will now develop a philosopher's toolbox to describe interviewees' "top-down" positions.

4.4.2 Towards a toolbox for scientists' positions

For the purpose of developing a toolbox for philosophers, I will primarily make use of analysts' categories. This marks a transition in the form of analysis. So far, I have proceeded purely descriptive. Starting with this section, a normative element will be added to my analysis. This normativity will come in in two ways. First, by employing a what I called "top-down" terminology, I will transition from discussing themes that came up "in vivo" in interviews, and thereby were part of the small RNA inheritance research field to analyzing the interview from the philosopher's perspective. That is, I analyze them respective to what philosophers deem interesting about scientific pluralism. Thus, there is not only the transition from "bottom-up" to "top-down," there also is a disciplinary transition, from issues raised within a field, to issues raised by commentators about the field.

If I am successful in creating this toolbox, then it should apply to other case studies of plurality within other research fields. My categories are neutral concerning the "subject" of pluralism. While "pluralism" and "monism" are my key initial categories, I will also introduce "singularism" and "dualism" as well as "non-integrationism" and "integrationism" as further categories to describe researchers' positions.

"Pluralism"/"monism" denote researchers' philosophical attitude about the state of their field.

"Singularism"/"dualism" denote how a researcher accommodates plurality in practice, that is: does she take her day-to-day activities to support plurality in practice[36]?

"Non-integrationism"/"integrationism" denotes how a researcher conceives of the relationship between different theories, explanations, repertoires, systems of practice, and the like.

In principle, these attitudes can criss-cross. Nevertheless, some go more frequently together or with less effort than others. It is important to note that this toolbox also has its limitations. For instance, it does not capture the rhetorical level. That is, it is agnostic concerning why one or more of these categories describes a researcher. For example, a researcher might qualify as a pluralist because she is a vehement defender of the epistemic values of scientific pluralism. Or she might qualify as a pluralist because she believes that pluralism is the best strategy to aid her institutional aspirations. Which of the two options is true is not discernible by using these categories.

[36] Scientific practice includes several activities such as, for example, experimenting, hypothesizing, theorizing, tinkering…

To exemplify the use of these categories and to create a scaffolding for the application of this framework to small RNA inheritance, I will discuss positions in the debate surrounding the Extended Evolutionary Synthesis (EES) as an example. This debate is, of course, connected to small RNA inheritance. Small RNA inheritance is one line of evidence presented by defenders of the EES in order to challenge the Modern Synthesis (MS). In general, defenders of the EES argue that the MS cannot account for certain phenomena of inheritance and evolution and that it, therefore, needs to be extended with more theoretical accounts. Defenders of the MS argue that it can explain all the phenomena that defenders of the EES present as exceptions or refuting instances.

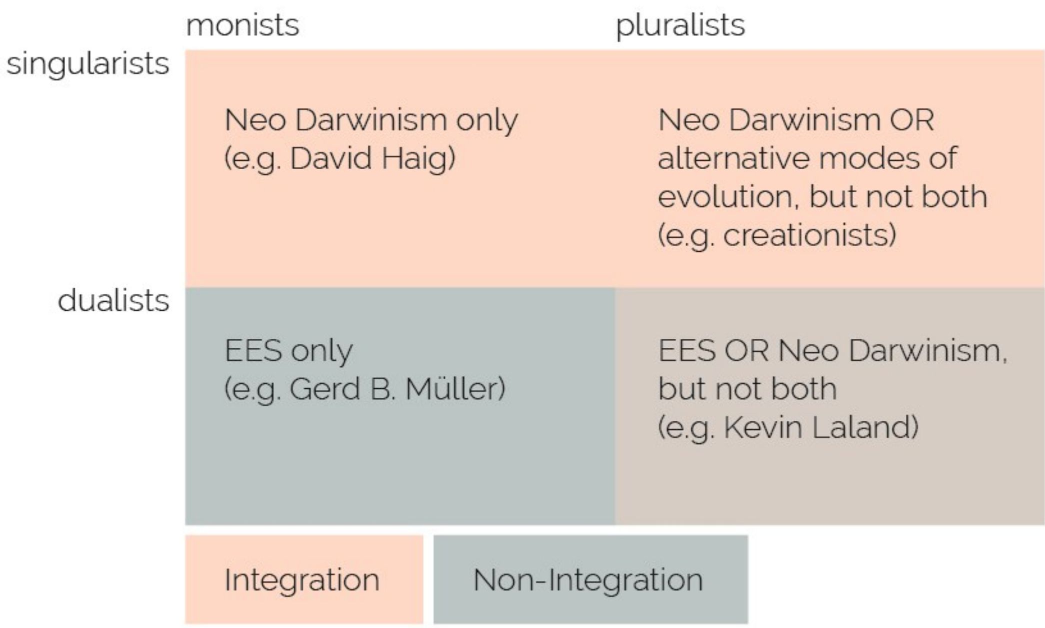

Figure 20 Proponents within the recent EES debate organized regarding their take on singularism/dualism, pluralism/monism and integration/non-integration.

In Fig. 20, I differentiate between four different types of actors within the debate surrounding the EES. I characterize Neo-Darwinists, that is defenders of MS, as non-integrationist singularist monists. They are monists because they argue that – in their case – only one theory can account for inheritance and evolution. They are non-integrationists because they accept that other approaches, such as small RNA inheritance, are not compatible with their framework. They are singularists because their research practices involve only one approach.

Some advocates of the EES are integrationist dualist monists. Proponents of this group claim that all researchers working on inheritance and evolution should work within an EES framework. They are monistic in the sense that they think it is bad if other researchers investigate a framework, which is not the EES. They would prefer a situation in which only one approach (i.e., EES) is around. They are integrationists because they believe that a synthesis of the MS and additional accounts is easily attained. In fact, in their view, the formulation of the EES proves that this integration is possible. They are dualists because they work with both approaches as explanatory resources.

There are, however, also other researchers who defend the EES but believe that it is worthwhile that other researchers work on the MS, and only the MS. These researchers are dualist pluralists. They are dualists because, similarly to the group of researchers discussed in the last paragraph, they work with both approaches as explanatory resources. They are pluralists because they believe it is good that, while some researchers focus on the EES, others use the MS. Their position concerning integration or non-integration is, however, more complicated. They are integrationist and non-integrationist but on different levels. In their research, they work with an integrative synthesis, the EES. But they appreciate that on a different level, EES and MS are non-integratable.

The last group are non-integrationist singularist pluralists. Creationists, for example, fall in this category. Proponents of this group present their approaches as alternatives to Neo-Darwinism. They are singularists because they consider their approach to be exclusive. This is the case because their approach is based on particular religious premises that are not shared with other positions within the debate. Creationists are, however, pluralists insofar as they emphasize that everyone should pursue the system of practice they prefer. Here one might ask, of course, are they *really* pluralists? Or is the pluralism just a rhetorical strategy? Such questions can, however, also be asked for the other groups which I discussed.

Having introduced the categories by applying them to the debate surrounding the EES, as a next step, I will characterize small RNA inheritance researchers with regards to these categories. For small inheritance researchers, it is essential to keep in mind that “theory” has different import in different contexts. “Theory” has a disproportionate bearing in disputes with non-small RNA inheritance researchers if compared with disputes within the field of small RNA inheritance. Small RNA inheritance researchers can be divided into two different groups. Some are dualist integrationist pluralists. They are dualists because they combine DNA-based and RNA-based approaches in their research. They are integrationist because they believe that an integrated account of both approaches that explains the inherited phenomena is possible. They are pluralists because they believe it is good that other approaches, e.g., the DNA only approach, are present.

Other small RNA inheritance researchers are monists. Two different subgroups are discernible. Some are dualist integrationist monists. They are dualists because they combine DNA-based and RNA-based approaches in their research. They are integrationist because they believe that an integrated account of both approaches to inheritance can be provided. They are monists because they believe that everyone should be a dualist integrationist. They think it is bad that other researchers pursue a different approach, such as the DNA only approach.

The second type of monists are singularist integrationist monists. They are integrationist because they believe that it is possible to integrate DNA- and RNA-based approaches. But they are singularists because they believe that the possibility of integration proves that the two approaches are not *really* different. To them, both approaches are, in fact, one approach. They are monists because they do not accept that there should be people who have an (integrationist or non-integrationist) pluralist position concerning DNA and RNA inheritance.

Another group of actors needs mentioning, too. Researchers who defend positions considered “too nutty” by others fall into the category of singularist non-integrationist monists or pluralists, respectively. Such researchers are singularist because they believe that only RNA inheritance and not DNA inheritance is causally relevant for all phenotypes. They are non-integrationists because they treat both the DNA-based and integrated approaches as alternatives to their view. Some of them are monists since they maintain that all researchers should follow their research program. Others, however, are pluralists. They take it to be good, or at least tolerable, that other researchers pursue other approaches. Thus, whereas integrative singularist positions are common amongst opponents of small RNA inheritance researchers, only very few small RNA inheritance researchers propose "RNA-only" singularist, non-integrationist positions.

Thus, the question concerning the possibility of integration plays a vital role in small RNA inheritance researchers. And, if directly asked, small RNA inheritance researchers have several intuitions about how to relate these approaches. For example, they observed that the case of small RNA inheritance is not like the wave-particle dualism and not like "telepathy." Most interviewees were also keen to emphasize that no-one (except "lunatics") would claim that there is no DNA. As discussed in section 4.3, one interviewee remarked that small RNA inheritance induces a "paradigm shift" but not of the "Kuhnian type." What this interviewee tried to say is that there is a shift in thinking about how to conceptualize inheritance, but not one in which one approach is rejected in favor of the other. Moreover, it is not the content of "Neo-Darwinism" that is challenged but one qualification of it, namely that Neo-Darwinism is the "only" right account.

Is this a paradigm shift? Certainly, as correctly observed, not of the Kuhnian type. Another way of framing this question would be to ask how important the "only" is for Neo-Darwinism. The literature shows that defenders of MS emphasize the exclusiveness of Neo-Darwinism (Hoekstra & Coyne, 2007). For them, the inclusion of small RNA inheritance-based phenomena would represent a paradigm change because one of the most critical components of their belief system is rejected. Small RNA inheritance researchers might, however, contend that by rejecting the "only," in fact, they reject the least essential component of Neo-Darwinism. Of course, no matter what kind of paradigm shift we are experiencing, we seem to be in the middle of it. It is impossible to tell whether, in the end, DNA-based inheritance or Neo-Darwinism will be rejected. But it is safe to say that none of the interviewees favors this outcome. It is probably best to differentiate between "paradigm shift" and "paradigm change." This differentiation is, however, not part of the Kuhnian terminology. "Shift" does not necessarily mean complete change but can also mean a shift of emphasis. Whether such a shift in emphasis is pathbreaking or heretical might be judged differently by different parties. Specifically, in a situation in which orthodoxy, newcomers, funding systems, hierarchies, and factions plays a role, the "shift" has different outcomes and different weight for the different parties.

After having described small RNA inheritance researchers and parties they are interacting with along philosophical qualifiers, I will provide a closer examination of small RNA inheritance researchers as a social group in the next section.

4.4.3 Descriptive account of the field

This section has two parts. First, I will present a grid/group analysis of small RNA inheritance researchers. I take this group of researchers as a discernible "social gathering" whose grid/group coordinates can be determined. The location of social groups small RNA inheritance researchers interact with will also be assessed. In so doing, boundary work in interviewees' accounts will play a significant role. Second, I will discuss how the plurality of "repertoires" shapes the constitution of the small RNA inheritance field. I will then elaborate on several issues that contribute to the perceived (in)stability of the small RNA research field, such as problems of expertise and the distribution of trust. In so doing, I will disentangle the trajectory of "theory" within discourses surrounding small RNA inheritance.

4.4.3.1 Grid/group Analysis

Boundary work (Gieryn, 1983) and grid/group (Douglas, 1973) analysis are related. No grid/group quadrant can exist on its own. Who qualifies as "the other," as different, is of significant importance for social groups. Boundary work is, therefore, a crucial exercise for inhabitants of grid/group quadrants. The determination of boundaries is pivotal for several reasons. Some individuals have the desire to move up or down the group or grid axes. They need to know what the criteria necessary for crossing a boundary are. To those who have no aspirations to move up or down, the integrity of their quadrant's frontier is pivotal. As a result, grid/group borders are continuously negotiated. My approach and Bloors' is similar in pursuing a particular way of doing grid/group analysis. Both Bloor and I perform grid/group analysis by assessment of speech acts only. The same is true for analyzing boundary work.

I will proceed as follows. First, I will assign quadrants to small RNA inheritance researchers (Fig. 21) and other social groups with which they interact. Second, I will contrast the interactions of small RNA inheritance researchers within their quadrant with how they present their field to the outside.

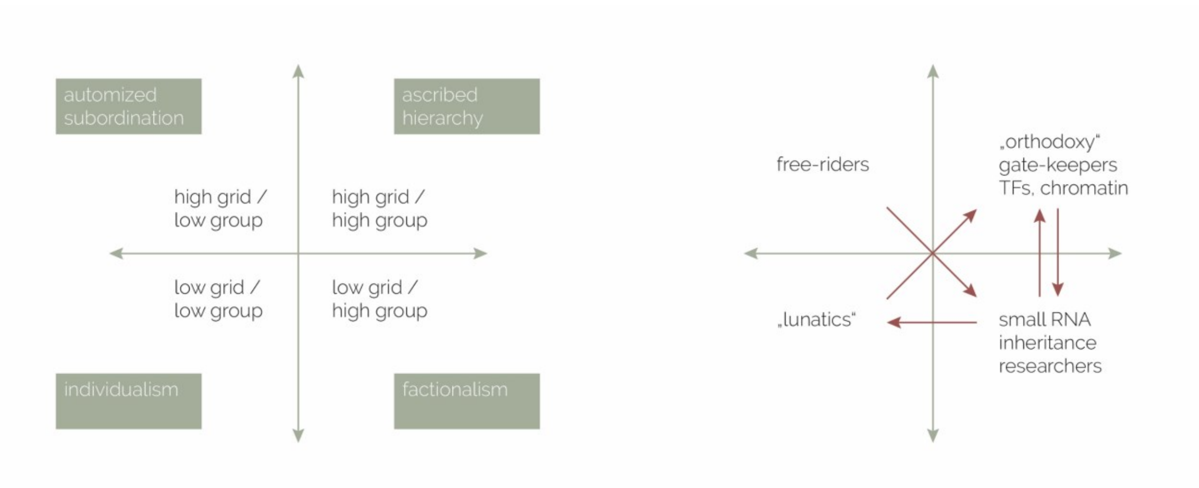

Figure 21 Scheme of grid/group analysis and grid/group analysis of small RNA inheritance researchers and social groups they interact with.

The small RNA inheritance research community inhabits the low grid/high group quadrant. Let me reiterate the characteristics of this sector to justify my choice. In low grid/high group structures, the external group boundary is clear. But all other statuses are "ambiguous and open to negotiation" (Douglas, 1982/2013, 4). This is also the case for the small RNA research field. Hierarchical structures or principles (such as age, sex, seniority) have no explicit role in organizing small RNA inheritance researchers. Interviewees characterized some of their peers as leading figures or researchers "who push the field forward." But they did not allocate specific powers with these peers, such as being in charge of decisions for the whole field. Although these researchers are considered leaders, they cannot take away resources from other group members. Criticizing works of these "leaders" is not prohibited or specifically sanctioned. They do not possess official ways of sanctioning members with fewer resources. These leading figures dominate small RNA inheritance nevertheless because they have allocated the most money and prestige. They have more resources to push the field in one or the other direction. Still, interviewees who are not considered "leaders" did not perceive pressure to coordinate their research focus with leading figures.

While the status and prestige of group members are open for negotiation and not rigorously assigned, the external boundary is explicit nonetheless. When assigning group membership, interviewees engaged in boundary work. They stressed the criteria of what makes one a member of the group. Failure to meet these criteria was considered a reason to take away group membership. Amongst others, the criteria were: working with the "right" MO, the "right" small RNA species, a specific experimental setup, and a particular generational timespan. There were also less precise reasons for exclusion, such as "being too nutty."

After having assigned a grid/group quadrant to small RNA inheritance researchers, I will proceed and assign quadrants to social groups with which interviewees interact. For the sake of this case study, all groups are described from the perspective of small RNA inheritance researchers. The social group most often mentioned was "orthodoxy." I place "orthodoxy" in the high grid/high group quadrant. In "ascribed hierarchy," "loyalty is rewarded, and hierarchy is respected, an individual knows her place in a world that is securely bounded and stratified" (1982/2013, 4). This quadrant is highly structured and who does not respect the rules (i.e., the right methods & formulas) is criticized.

Small RNA inheritance researchers characterized different types of researchers as "orthodox": evolutionary biologists, "chromatin-people," and "transcription-factor" people. They are "gate-keepers." Interviewees' relationship with "orthodoxy" is multi-dimensional. On the one hand, small RNA inheritance researchers aim at challenging orthodoxy and therefore try to set themselves apart from it. For example, they try to specify differences. On the other hand, small RNA inheritance researchers are dependent on the existence of institutions founded and governed by orthodoxy. High-impact journals, grants, and impactful conferences are part of the orthodox structures. There are thus two movements between the high group/high grid and the high group/low grid quadrants. One is up-grid: to access funding and publish papers, small RNA inheritance researchers try to move up-grid to access the reward-systems of orthodoxy. The other is down-grid: small RNA inheritance researchers experience pushback from orthodoxy. They know that the gatekeepers consider them "heretics." Some interviewees pointed out that they are conscious of efforts to keep small RNA inheritance research out of reward-systems.

To sum up, small RNA inheritance researchers' accounts of "orthodoxy" fit very well with the definition of "ascribed hierarchy." Within orthodoxy, there is a strong sense of following rules and strong consensus about the right formulas, methods, and theories. Researchers who do not follow these rules are viewed as deviants and blamed for instabilities within the group. Pretending to be part of, or move closer towards, orthodoxy is strategic for small RNA inheritance researchers. This recalls Mario Biagioli's concept of "bilingualism." "Bilingualism offers a way *around* incommensurability, it cannot resolve it" (Biagioli, 1990, 202). The use of bilingualism often makes the speaker "schizophrenic." By speaking the language of another program, one also speaks the language of a different socio-professional identity (1990, 205). But this is not necessarily the case. "Strategic" bilingualism means to make use of a "fossil," a language of one's scientific past (e.g., one's training), without changing one's socio-professional identity (ibid.).

There is a critical asymmetry concerning bilingualism: "while members of established socio-professional groups have much to lose from becoming bilingual, bilingualism is strategically important to the 'invaders'" (ibid.). Bilingualism might not produce dialogue. The "invaders'" bilingualism indicates a lack of power. It signals that they have too little power to legitimize their program employing their language (1990, 208). This description fits well with the characterization of small RNA inheritance researchers. They are not "like Galileo" (as one interviewee suggested). But their interactions with "ascribed hierarchy" and the necessity of employing bilingualism are similarities with Biagioli's case study.

Small RNA inheritance researchers are proficient in the language of "orthodoxy" because it is the language of their training, it is the language they were "disciplined" in (Arnold, 2004, 19). They need to make use of the language of orthodoxy – the "useful fossil" (1990, 205) – to access funding and prove their proficiency on issues that concern orthodoxy. In other contexts, they show their fluency in the language of their program, which breaks the rules of orthodoxy by proposing different formulas, methods, theories, and molecular entities.

In the remainder of this section, I will look at groups that inhabit other areas of the grid-group system. First, I will consider researchers in the low grid/low group quadrant, "individualism." In low grid/low group structures, individuals are responsible for themselves. Negotiating contracts and choosing allies is possible yet flexible and not permanent. Researchers described as "a little nutty" fit this quadrant. Interviewees most frequently engaged in boundary work to demarcate themselves from individuals who are "a little nutty." "A little nutty" can mean several things. Such individuals utter "claims that harm the field," overinterpret, and claim "outrageous things." Low grid/low group individuals have somewhat similar socio-professional identities as have low group/high grid researchers. Installing boundaries is thus necessary to ensure that "lunatics" cannot invade the small RNA inheritance field and thereby harm it.

There are two movements between small RNA inheritance researchers and "lunatics." Individuals who become "too nutty" move down-group. The small RNA inheritance research community is trying to push these researchers out, for they might be harmful to the field. Researchers described as "too nutty" aim at moving up-group to profit from the privileges of group ties. Within "individualism," they cannot profit from group cohesion and have to secure funding on their own. They are entrepreneurial and provide narratives about "special skills of the uniquely endowed" (Bloor & Bloor, 1982/2013, 89).

Although several researchers inhabit the low grid/low group quadrant, there are no strong group ties. While all of them were described as "lunatics," their beliefs, methods, model organisms, and theoretical commitments are heterogeneous, and it is difficult for them to form alliances. Inhabitants of the "individualism" quadrant might find other members within the same quadrant "too nutty" to collaborate with. They fear for their reputation and believe that such alliances might keep them from moving up-group.

While some interviewees locate themselves in the high group/low grid quadrant, they might be described by others as inhabitants of "individualism." There are several possible ways of explaining this tension in membership ascriptions. One starting point is Harry Collins's advice for determining the core set: exact membership is not as interesting as are systemic discrepancies in assigning membership (Collins, 1981, 9). In other words, interviewees do not commit a "mistake" if they disagree about the membership of one individual. Such disagreement highlights tensions within the research field. Systematic discrepancies in such membership ascriptions could be regarded as markers of instability.

Other interpretations are also possible. For example, Bloor argues that utterances of interviewees often do not reflect the quadrant they inhabit but the quadrant they aspire to inhabit (Bloor, 1978, 261). This insight is instructive for two of the described movements: up-group and up-grid. It is not clear whether those who were described as “a little nutty” but placed themselves within “factionalism” are conscious of the status of their group-membership. They might take their group membership (wrongly) for granted. But it is also plausible that they are conscious of their low group/low grid position. By speaking the language of the high group/low grid quadrant, they might try to reinforce their membership. What is important to note is that the “lunatics” do not only interact with “factionalism” but also with “ascribed hierarchy.” Orthodoxy is essential for all grid/group configurations in terms of funding, journals, and grants.

A similar phenomenon also occurs amongst relatively established small RNA inheritance researchers. These researchers tended to speak the language of orthodoxy, of the establishment. It is, of course, possible that these researchers (wrongly) take their membership in ascribed hierarchy for granted. This is, however, unlikely as they publish results that challenge the beliefs of orthodoxy. It is thus also conceivable that by speaking the language of the high group/high grid quadrant, they indicate their desired location.

I will interpret the discussed discrepancies as a marker of perceived instability. In section 4.3.1, I mentioned that interviewees who fault others for being “too nutty” still cite articles by “nutty” researchers. Similarly, interviewees cite papers that are known to have replication issues. It is thus possible to interpret these discrepancies as differences in representation to the outside rather than as indicative of the organization of the group as it looks on the inside. Biagioli describes these situations in *Anthropology of Incommensurability*. Copernican mathematicians shared a socio-professional identity but had significantly different theoretical commitments. They nevertheless presented themselves as Copernican mathematicians with one socio-professional identity to the outside. This strategy was necessary to challenge Aristotelianism. Presenting a picture of unity, consent, and homogeneity to the outside was pivotal to be taken seriously. Thus, how a field appears to the outside and the actual disputes and theoretical commitments within might be mostly disparate.[37]

Why is it so crucial for small RNA inheritance researchers to present themselves as a somewhat "united front" to the outside? To home in on this issue, I will return to the account of one interviewee in particular. They pointed out that in order to "keep creationists out," it is pivotal to unite with "orthodox" evolutionary biologists. For a specific purpose or because of a specific set of shared values, both "orthodox" and "heterodox" researchers aim to present themselves as one group to the outside.

The aforementioned ambiguous citing behavior of small RNA inheritance researchers can be explained with a similar approach. Small RNA inheritance researchers encounter substantial threats and criticisms. They feel the need to present small RNA inheritance research as unified to the outside to shield it from further criticism. To them, it is sufficient to handle problems like replication issues by spreading tacit knowledge within the field. Avoiding exposure to these problems to the outside is instrumental. Small RNA inheritance researchers fear that others might interpret heterogeneity and disagreement as signs of immaturity. Small RNA inheritance researchers prefer not to expose signs of immaturity to be taken more seriously as a research program.

The last grid/group quadrant that needs to be discussed is high grid/low group, "atomized subordination." This quadrant houses isolated individuals with no privileges of group membership. Researchers characterized as "free riders" inhabit this quadrant. By picking up on specific aspects of small RNA inheritance, they try to force their way into the small RNA inheritance reward system. These free riders do not have group ties but profit from pre-existing structures. They also benefit from the relative establishment of small RNA inheritance as a field because their work appears to be emanating from it. The movement of

[37] Another example, which is less a homologue and more an analogue, is cultural historian Serge Gruzinski's work on mestizo processes in colonial Mexico, for example in *The Mestizo Mind* (2002/2013). Gruzinski challenges an often transmitted homogenous picture of "colonial Mexico". He describes different ways to facilitate mechanisms of mestizo and hybridity, in which "mimicry became a source of inventiveness" (2002/2013, 63). One of his case studies are mestizaje mechanisms in Christian imagery. While Spanish colonialists presented colonial Mexico to the outside as a society fully converted to Christianity, and homogenous in belief, what was at stake on the inside was completely different. Hybridity of Christian and indigenous symbols, images, practices, and beliefs prevailed. Thus, how a society or group presents itself to the outside need not reflect its internal structure. It might well serve institutional or political aims and goals.

free riders is twofold. Their first move is out of “ascribed hierarchy.” “Free riders” lose the privileges of hierarchy by pursuing research on small RNA inheritance. As a result, they find themselves in the quadrant of “atomized subordination.” The second move is a difficult one because it is diagonal, towards the low grid/high group quadrant. This move is not welcomed by the inhabitants of the low grid/high group quadrant, however. While losing the privileges of ascribed hierarchy, free riders fail to attain group membership of factionalism.

4.4.3.2 Grid/group locations and responses to anomaly

Grid/group analysis of small RNA inheritance researchers proves to be a telling descriptive framework. The possibilities of grid/group analysis are, however, not exhausted with assigning quadrants. On the contrary, the most potent aspects of grid/group analysis begin after the successful assignment of quadrants. Specific grid/group location predicts responses to anomalies of those inhabiting a quadrant. In section 4.2, I discussed Bloor’s use of grid/group analysis when assigning quadrants for the proponents of Imre Lakatos’ *Proofs and Refutations*. Responses to anomaly, which Lakatos defined for different groups of mathematicians, fit the specific social organization defined by the grid/group location. In this section, I aim to test whether locating a group in a quadrant predicts how they respond to anomalies.

Small RNA inheritance is an anomaly within neo-Darwinism. In previous sections, I have shown that different social groups respond differently to small RNA inheritance. In what follows, I will assess whether the assigned grid/group locations match the responses to anomaly predicted by Bloor. Each quadrant has a specific “repertoire” of responding to anomalies. I will primarily focus on small RNA inheritance researchers (low grid/high group) and orthodoxy (high grid/high group). In passing, I will also shortly discuss whether “lunatics” (low grid/low group) and “free riders” (high grid/low group) match the predicted responses.

There is one striking difference between my case study and the one analyzed by Bloor. As discussed in section 4.2, Bloor urges us to regard “counterexamplehood” as a social achievement. The process of “recognizing” a counterexample is similar to that of "recognizing" someone as, e.g., a knight. If being recognized as a "counterexample" is a social achievement, this recognition might not happen evenly across all grid/group quadrants. Some groups might recognize a specific phenomenon or proposition as a counterexample, while other groups do not.

Counterexamplehood is not only a social achievement but also situated. Empirical data that could be regarded as a counterexample is usually produced in the context of one grid/group quadrant and then spread throughout the other quadrants. We should expect problems and counterexamples to emerge through negotiations of researchers within grid/group quadrants. Counterexamples do not "fly in" from *outside* the grid/group system. Counterexamples are produced and recognized *within* the grid/group system. Their respective status is *situated*.

Empirical data might be regarded as an anomaly in one quadrant but not in the other. If this is the case, we should expect some asymmetries concerning responses to this specific set of data. I am particularly interested in cases in which a set of researchers has produced findings that are asymmetrically recognized as anomalies. I will show that findings of small RNA inheritance are produced in one specific grid/group setting and subsequently recognized differently in different grid/group sectors.

Let me start with "orthodoxy," high grid/high group structures. High grid/high group structures are characterized by complex relations and complex procedures to make conceptual accommodations. Reactions towards anomalies are "exception-barring" and "monster-adjustment." Exception-barring means to accommodate an anomaly by introducing more subdivisions. The counterexample or monster, thus, only limits the scope of a theorem but is not a refutation. Monster-adjustment is the procedure that helps to see a counterexample in a way that it fits an established theorem. Because of these procedures, knowledge gets a rather "additive" or "segmented" character. High grid/high group structures have the resources for the compartmentalization of knowledge.

This characterization matches "orthodoxy" quite well. Exception-barring and monster-adjustment are, in fact, two strategies that are often encountered by defenders of orthodoxy or the MS in particular. For example, they engage in "exception-barring" when they argue that small RNA inheritance is only proven in “weird organisms” (Koonin & Wolf, 2009). In saying that, they suggest that their theorems need not cover “weird organisms.” They engage in monster-adjustment when they argue that phenomena described as small RNA inheritance were always covered by the propositions of the MS (Wray et al., 2014). In conclusion, for the case of “orthodoxy,” their grid/group location matches their responses to anomalies. Grid/group analysis is predictive.

When analyzing small RNA inheritance researchers, the low grid/high group inhabitants, the problem discussed above becomes visible. Small RNA inheritance researchers “produce” the anomaly. Orthodoxy responds to the anomaly. The activity characteristic of factionalism is “monster-barring.” “Monster-barring” seems, however, to be undesirable for small RNA inheritance researchers. As small RNA inheritance researchers are supporters of small RNA inheritance, it would be counterintuitive if they “monster-barred” small RNA inheritance.

Monster-barring is an activity that results in calling a counterexample “pathological” or a “monster.” Because of its “pathological” character, the anomaly needs not be considered anymore. Delta’s response to a counterexample in Lakatos’s Proofs and Refutations is one of the most striking illustrations of “monster-barring.”

> “I am gradually losing interest in your monsters. I turn in disgust from your lamentable "polyhedra", for which Euler's beautiful theorem doesn't hold. I look for order and harmony in mathematics, but you only propagate anarchy and chaos” (Lakatos, 1976, 19).

This sentiment resonates with the preoccupation of low grid/high group structures with “purity” and their elevated consciousness for “pollution.” Such responses represent the prevalence of a strong dichotomy between the “good” inside and the “evil” outside. It would, of course, be surprising if small RNA inheritance researchers “monster-barred” small RNA inheritance. My analysis has demonstrated that they also do not monster-barr traditional forms of inheritance, such as DNA inheritance. Within their quadrant, small RNA inheritance and DNA inheritance do not merely coexist. As argued in section 4.4.2, small RNA inheritance researchers are dualists; they work with both at the same time.

Do the predictions of grid/group theory fail here? Not necessarily. As I have highlighted above, within factionalism, small RNA inheritance is not recognized as an anomaly. If a phenomenon is not recognized as an anomaly within one grid/group quadrant, we cannot expect "monster-barring" responses. While it is, of course, essential to mind such asymmetries, I want to dig a little deeper. It is possible to detect "monster-barring" activities of small RNA inheritance researchers, but they occur on a different level. These monster-barring activities, I will demonstrate, ensure that the status of the anomaly will never be attained.

In section 4.3, I discussed responses of small RNA researchers, that I will now characterize as instances of monster-barring. One monster-barring activity is the sentiment of researchers that they "come from mechanism," thus they need not think about theories or provide their results with a Lamarckian framing. Thus, they maintain purity by barring theory. Without theory, there is no anomaly. Second, small RNA inheritance researchers expressed strong concern about researchers that are "too nutty." Researchers that are too nutty offer interpretations of small RNA inheritance that are "monsters." These interpretations are also recognized as monsters by the inhabitants of factionalism. Through excessive boundary work, they try to keep those researchers outside their quadrant. We can thus see that monster-barring is not absent but takes place on another level. It ensures that small RNA inheritance does not receive the status of an anomaly within the field.

In conclusion, the grid/group location successfully predicts responses of small RNA inheritance researchers, once asymmetries between grid/group quadrants are appreciated. This should not come as a surprise. If counterexamplehood is situated, we should expect that different aspects of the counterexample need to be barred/adjusted with respect to the individual quadrants. I shall now turn to the two quadrants which have not been discussed yet.

The low grid/low group quadrant hosts the "lunatics." "Dialectical methods of proofs and refutations" are characteristic of this quadrant. They have no problems to "abandon one's original problem in the course of solution and replace it by another" (1976, 68). Inhabitants desire discontinuity, tolerate mistakes, and there is no hope for permanent victory or stable compromise. The question of "where is the boundary?" has no meaning. This is true for those researchers described as "lunatics." They all have different theoretical accounts of their findings, which all are considered "too nutty." These affiliations can, however, be changed without much effort. They do not bar the monsters of "theory" or "Lamarckism" but incorporate them in their theorizing as readily as they are prepared to abandon them for another hypothesis.

Finally, the high grid/low group quadrant needs to be discussed. It is characterized by the simple coexistence of theorem and counterexample. This resonates with those characterized as "free riders." The counterexamplehood of small RNA inheritance does not present a problem for them. Their goal is to enter the low grid/high group quadrant which creates the "anomaly." The accommodation of the anomaly is a task for the participants of other quadrants.

4.4.3.3 What is at stake in small RNA inheritance?

To answer what is at stake in small RNA inheritance, I need to define which kind of plurality is prevalent in the field. In Chapter Two, I discussed three options: theoretical plurality, explanatory plurality, and mechanistic plurality. These three suggestions were made from the philosopher's perspective and draw upon the classic conceptual playgrounds of philosophy of science: theories, explanations, and mechanisms. Assessing the scientific literature on small RNA biology, the argument for theoretical plurality seems vindicated. Small RNA inheritance researchers write about "Lamarckian theory" and insist that Neo-Darwinism is unable to capture certain phenomena. Clearly, I hypothesized, they must be scientific pluralists about theories.

The analysis of my interviews proved me wrong. It proved me wrong in two respects. First, many of the researchers I considered scientific pluralists did not support pluralism. Second, while a focus on theories was explicit in their publications, theoretical plurality played little role in researchers' accounts of scientific practice. But that does not mean that scientific practice does not display plurality. Other sources of plurality were often more relevant than the pluralism of theories. Model organisms (MOs) and research traditions, for instance, may serve as sources of plurality.

In Chapter Two, I introduced the concepts of "model organisms," "repertoires," and "alignment." In this section, I will answer the question of how plurality influences the status and stability of the small RNA inheritance research field. In previous sections, I highlighted that "theory" is critical when presenting the field to the outside but that theory does not play this role in discussions within the field. In interviews, when not asked explicitly, interviewees did not mention theories. Neither did they describe their experiments as vindicating or disproving a theory. Most interviewees located plurality, and pluralism, in the arena of scientific practice. In addition to general remarks about scientific practice, MOs and the respective history of the research field were prevalent.

Interviewees argued that the different properties of MOs are sources of plurality. But they did not only refer to what one could call "interspecies mechanistic plurality" – the observation that in different MOs, different mechanisms operate small RNA inheritance. Beyond referring to this "biological fact," MOs bring about other sources of plurality, defined by the specific histories of the MO research communities and the specific repertoires which characterize these communities.

I will discuss one specific example of plurality created by MOs to highlight the intricacies of plurality in scientific practice. In section 4.3, I surveyed arguments of interviewees on why they perceive specific experimental designs, and MOs, as more or less apt for studying small RNA inheritance. This dispute involved a sub-debate on "natural" vs. "controlled" experimental designs. An experimental design can be the specific experimental set-up or a specific method or technology employed, but it starts with the choice of a model organism in general.

I surveyed two strands of argumentation. One group of researchers maintains that the right way of doing experiments on small RNA inheritance is to control experimental conditions as much as possible. Controlled conditions require inbred strains with as little genetic variation as possible and the use of transgenes to control for the readout. Such strategies profit from the well-aligned features of repertoires – standardization, established technologies, and comparability of results.

The other group of researchers opposes this way of doing experiments which they consider "unnatural." Studying small RNA inheritance under such conditions, "real" effects will be missed. Small RNA inheritance should be studied in conditions that are as "natural" as possible. For example, proponents of this view discourage using inbred strains and suggest using genetically heterogenous strains instead. Furthermore, they advise against the use of transgenes as readouts. Instead, they emphasize the importance of "endogenous" readouts, that is, small RNAs expressed under physiological conditions.

This dispute is not new. Specifically, it is not new in the history of genetics in general and studies on the inheritance of acquired traits in particular. For example, controversies surrounding Paul Kammerer's experiments on the inheritance of acquired traits foreshadowed these types of debates. Critics faulted Kammerer for not using uniform and inbred lines but lines with significant variations. For example, the famous geneticist and fly-room founder Thomas Hunt Morgan criticized Kammerer for using toads of mixed origin and genetic composition. Kammerer did not, however, share the geneticist's perspective on how to breed experimental organisms. The institution where he worked, the *Biologische Versuchsanstalt*, emphasized work in naturalistic settings (Müller, 2017). Naturalistic settings, in turn, require genetic variability. Neither did Kammerer share Morgan's socio-professional identity. Morgan represented a new breed of geneticists and Kammerer was a defender of experimental zoology. Their notions of appropriate methodology were disparate (Gliboff, 2006, 2010).

The continuity of the dispute about "controlled" vs. "natural" conditions is striking. Changes in repertoires, model organisms, socio-professional identities, institutional, and political factors require special attention nevertheless. This issue takes different shapes in different historical contexts, depending on participants, their socio-professional identities, and the repertoires they can mobilize. This controversy is a significant source of plurality, the plurality of repertoires. Different model organisms are better apt for "controlled" vs. "natural" conditions, respectively. Centralization and standardization also play a significant role. Using inbred, clearly defined strains is facilitated by researchers working on specific MOs, such as C. elegans. The C. elegans repertoire enables accessing strains from institutionalized breeding centers which are apt for highly controlled experiments. For mouse strains, this is much more difficult to achieve. As discussed in section 4.3.1.3, pleas for "natural" conditions are much more common amongst proponents of the mouse community. Moreover, the dispute between "natural" and "controlled" conditions introduces a plurality of various epistemic activities linked to specific experimental practices.

Concerns about theoretical plurality are absent from interviewees' accounts of the dispute about natural vs. controlled conditions. This runs in parallel to the example of Kammerer and Morgan. Of course, these researchers had different theoretical commitments. The start of the 20th century was one of the best times for disagreeing about theory: chromosome theory of inheritance, Mendelism, neo-Lamarckism, old-school Darwinism, neo-Darwinism, experimentalism… For example, Kammerer believed that acquired adaptive traits "mendelize" on the chromosome (Gliboff, 2006). His mentor Hans Prizibram rejected the chromosome theory of inheritance and believed in the "Weismann barrier." Despite major theoretical disagreements, Prizibram supported Kammerer. Morgan shared many of Kammerer's theoretical commitments but rejected Kammerer's work because of the experimental design. To conclude, although "theory" was not the primary concern in the scenario sketched above, Kammerer's story is often framed as a dispute about theory, that is, Lamarckian theory. We seem to experience a similar situation with small RNA inheritance.

Scientific practice also exposes another feature of the small RNA inheritance field, which results from the plurality displayed. Because small RNA inheritance is confronted with criticisms from different parties, expertise becomes a crucial problem. As discussed in section 4.3, different researchers with different types of disciplinary identities are "opponents." One can subdivide the "critics" into two groups, those "from within" and those "from outside." In what follows, I will demonstrate how this "duality" of opposition determines the stability of the field. In so doing, I will clarify several issues concerning the role of "theory" for small RNA inheritance researchers.

Researchers "from outside" are evolutionary biologists, neuroscientists, and "transcription-factor people." Researchers "from within" are "chromatin-people" and researchers who did not succeed with their small RNA inheritance experiments. Expertise concerning small RNA inheritance separates these two groups. “Chromatin-people” and researchers who did not succeed with small RNA inheritance experiments employ the same kinds of experiments and know pitfalls and problems very well. Those “from outside,” however, lack this expertise. Their criticisms target an “ideological” level and issues about theories. They offer criticism from “above.”

The presence of both criticism from “above” and from “within” results in a quite delicate situation: what counts as the right “expertise” becomes a non-trivial issue. Researchers who utter “ideological criticism” have little expertise about the actual content of small RNA inheritance research papers. Researchers “from within” utter “methodological criticism,” and target specific experiments for their validity. Who has the right set of expertise for launching criticism? Which kind of expertise is required to address criticism?

Together with the problems of expertise comes the problem of replication. Several interviewees mentioned that they are conscious of replication issues in milestone papers. These issues are not publicly debated. They remain a subject of “tacit knowledge.” “Trust” in other practitioners’ experiments is pivotal for the integrity of a research field. Trust is so essential because very few experiments are replicated. In times when there are no significant controversies, most of scientists’ knowledge is based on trust: “Scientists know so much about the natural world by knowing so much about whom they can trust” (Shapin, 1994, 417). Compared to testimonial knowledge, scientists gain relatively little first-hand knowledge. Even more so, testimony is always needed in order to acquire knowledge.

Trust in testimonial information is pivotal for small RNA inheritance researchers. Most of their knowledge is secondhand knowledge. The transmission of second-hand knowledge is particularly facilitated by the small group size and the “Sidney-Brenner-family-tree.” And they trust this information because the producers of this knowledge fulfill certain criteria: academic career, a specific mentor, experimenting with a specific MO or experimental design, track record, whether this person has uttered “outrageous claims” before, collaborations, but also the personal experience with similar experiments. At the same time, failing in all or some of these criteria results in distrust. Choice of MO, track record, tacit knowledge, institutional affiliation, collaborations… are reasons for distrust.

Problems concerning expertise, trust, and replication invite the question of whether small RNA inheritance faces an "Experimenters' Regress" (Collins, 1992). In what follows, I will argue that the current situation is not one of a regress, but it might well develop into one. Due to the two levels of critique, the "ideological" and the "methodological," I will treat these two issues separately. I will combine these thoughts with the results of grid/group analysis and ask whether different quadrants define whether a regress is enabled or not.

The novel detection of a phenomenon bears on several problems. Whether a so-called "crucial experiment" is successful or not determines whether a particular phenomenon was detected or not detected. Researchers need to agree whether the results of such an experiment imply the detection of a phenomenon. They also need to agree that the "crucial experiment" was performed by a competent person. As a result, scientists might disagree about two things. First, what the correct interpretation of a set of data is. Second, whether the set of data was competently produced. In times of an Experimenters' Regress, scientist cannot agree about the data and not about the interpretation of the data.

The interpretation of results by other scientists depends on their charity and their trust in the experimenter. As a general rule, lack of charity often implies a defense of the status quo and a license to expel anomalous findings from the body of scientific knowledge (Collins, 1981a, 53). The Experimenters' Regress describes such situations. In sophisticated scientific disputes, in which the outcome is not clear, only the outcome shows whether difficulties have been overcome. There is no other criterion. Thus, it is often impossible to determine whether a given empirical finding confirms or disconfirms evidence. In such situations, "scientists turn to a variety of criteria broadly similar to those used in making common-sense judgments in ordinary life – concerning reputation, institutional affiliation, track record, personal ties, conduct, and so forth" (Collins, 2005, 458).

I will return to the case of small RNA inheritance in particular. "Ideological" critics do not engage in a debate about the replicability of experiments. Instead, the ideological debate is a version of the underdetermination of theories by evidence. Controversies over theory are not central to small RNA inheritance researchers within their field. But theoretical considerations about inheritance and evolution are prominently featured in representations of their field to the outside. Reacting to these claims, evolutionary biologists argue that the data on small RNA inheritance is too sparse, or too restricted, or overinterpreted. The data does not support claims about the inheritance of acquired traits. In this scenario, the parties debate whether the accumulated evidence supports a specific interpretation or theory. Thus, the “ideological” criticism does not cause a “regress.”

The debate “from within” does not yield an Experimenters’ Regress either. Up to now, a dispute about replication issues is not visible. Awareness of these problems remains in the realm of tacit knowledge. But why is there no open dispute about replication issues? There are several possible explanations. One could argue that those who failed at replication are not members of the core group and thus cannot mobilize the right resources to challenge specific claims. Another explanation is that all concerned actors (small RNA inheritance researchers, “chromatin people” and researchers who failed to do small RNA inheritance experiments) still share a common goal, which is to establish the inheritance of acquired traits as a phenomenon. Therefore, they do not negotiate criticisms openly. Nevertheless, the emergence of a regress is not impossible in the future.

What remains to be done in this section is to bring the issues discussed above in contact with grid/group analysis. The question is: can grid/group position also predict the presence or absence of an Experimenters’ Regress? Above, by discussing the pressure from “above” and from “within,” I investigated whether there is an Experimenters’ Regress a) between the high grid/high group and the low grid/high group quadrant and b) within the low grid/high group quadrant. In what follows, I will investigate whether the results of my analysis are resonant with predictions of grid/group analysis.

I will start with a). In ascribed hierarchy, small RNA inheritance has the status of an anomaly. The importance of “theory” in general is very high. Therefore, it would be possible for a regress to emerge. However, it is prevented by several factors. The procedures of “exception-barring” and “monster-adjustment” play a role. Through these procedures, through making these conceptual accommodations, an Experimenters’ Regress is prevented. By compartmentalizing theories and sets of experimental data, chances of a regress get diminished.

For b), the problems within the low grid/high group quadrant, I argued that we do not see a regress *yet*. Grid/group location explains the absence of regress. Monster-barring activities of small RNA inheritance researchers are successful in keeping the "monsters," that is, "theory," e.g., "Lamarckism" out. Thus, there cannot be a regress at the moment because "theory" (interpretations of data) is not admitted. Nevertheless, the emergence of a regress seems very likely. Its starting point becomes already visible. There are problems of replication, excessive boundary work, and strong dichotomies of the "inside" and the "outside." Low grid/high group gatherings do not have elaborate ways or repertoires to respond to anomalies. Therefore, as soon as the "monster-barring" activities are rendered unsuccessful, we will most likely witness a regress within the field of small RNA inheritance. This is, of course, a prediction.

In summary, plurality, in general, makes interviewees perceive their field as less stable. Several observations can substantiate this claim. The first one is the grid/group quadrant small RNA inheritance researchers inhabit and the extensive boundary work they engage in. Small RNA inheritance researchers experience pressures from "above," challenges by, e.g., evolutionary biologists, mostly concerned with theory. Pressures from within the field are of a different kind. Model organisms, research traditions, replication issues, and technological limitations are sources of plurality within the small RNA inheritance field. Not only is it possible to divide these "pressures" into two different kinds – "ideological – from above" and "methodological – from within." This also influences the discrepancies in representation. To the outside, the status of one unified small RNA inheritance field is maintained through a theoretical program – the challenge of Neo-Darwinism. To the inside, the field is not unified, and group membership is heterogeneously assigned by group members. Theory, in this context, plays no role. Discussions about theory, Lamarckian theory, are even "barred" in order to avoid the emergence of the regress. In conclusion, "theory" has two contradicting roles. Theory is mobilized as a representation to the outside and is used as a unifier for small RNA inheritance researchers. Theory is, however, avoided in the inside, it is handled as a "monster." If theory is not kept out successfully, an Experimenters' Regress might ensue. In the case of small RNA inheritance, "theory" is Janus-faced; theory unifies and destabilizes at the same time.

5 Approaches Towards Scientific Pluralism

In previous chapters, I put "scientific pluralism" to the test. First, I reviewed the history of small RNA inheritance and proposed pluralist interpretations for DNA vs. RNA inheritance. Then I challenged my interpretation. I showed that scientific pluralism is primarily a philosophers' category that might not resonate with the positions of researchers. Subsequently, I raised the problem of "normative pluralism." If scientists do not embrace scientific pluralism, how can plurality be sustained within research fields? To address these questions, I put scientific pluralism to the test. Are small RNA inheritance researchers pluralists? And how does plurality affect the status and structure of the research field? Is it more stable or unstable? I presented several findings. First, there is a vast heterogeneity within researchers' positions regarding scientific pluralism. Even more so, a strict dichotomy between pluralism and monism was not maintained. Second, plurality destabilizes small RNA biology. Even those researchers sympathizing with plurality identified it as a destabilizer. Third, "theory" played a dual role for small RNA inheritance researchers. While theory is a means to represent the small RNA inheritance field to the outside, it has minor importance and is even avoided within the field. In this chapter, I will provide a philosophical discussion of scientific pluralism. While taking the time of explaining several issues about pluralism that were only considered *in passim* in the previous chapters, I will also raise some points about pluralism in general. Specifically, I will investigate classifications of pluralist positions.

5.1 Approaching scientific pluralism

Scientific pluralism has become an increasingly popular position in philosophy of science. A very general idea that motivates most pluralist programs is that some natural phenomena cannot be approached by just a single account. As a consequence, one needs multiple approaches in order to investigate such phenomena. I used this general account to assess researchers' positions. In what follows, I will elaborate on a more refined definition of scientific pluralism. I will provide necessary and sufficient conditions for scientific pluralism and will survey "optional" elements of SP which characterize specific pluralist positions.

5.1.1 Necessary and sufficient criteria

In Chapter Two, Three, and Four, I used a rather vague definition of SP. I identified the pluralist attitudes of researchers by assessing whether they sympathize with the idea that it takes more than one approach or theory to approach a target phenomenon. I also specified their positions, discussing whether their pluralism or monism was metaphysically or epistemologically motivated. I differentiated between whether they believed that pluralism is a position about integratable or non-integratable approaches. In this chapter, I will provide a philosophical discussion of SP and will thus elaborate a more refined definition. In this section, I will propose necessary and sufficient conditions. I will survey the specifications of SP in the section to follow.

To formulate a definition of SP, a certain tension needs to be overcome. On the one hand, the position must not be too thin. Pluralism is, in the end, opposed to monism. A monist should disagree with at least one (necessary and sufficient) criterium for SP. On the other hand, SP should also not be too broadly applicable. I will briefly illustrate the two extremes which should be avoided.

One example of "thin pluralism" is the position that scientific theories or explanations usually integrate a plurality of causal factors. Not even card-carrying monists would deny this. In Chapter Two, I discussed mechanistic plurality in small RNA inheritance and judged this type of plurality as "too thin." In Chapter Four, I showed that also interviewees regarded this kind of plurality as trivial. Do not the genomes of mother and father present two causal agents for producing a particular phenotype? Do not electrostatics and gravitational forces define how two objects interact? Pluralism about causal forces is, thus, "trivial pluralism."

One example of "too broad" pluralism would be SP that at the same time concerns two approaches that seem so different that it is hard to see how they approach one target phenomenon. One example would be comparing Azande witchcraft and investigations entertained by contemporary western scientists. Both projects structure and make sense of the exterior reality. It is, however, hard to define common target phenomena about which one could be an explanatory or theoretical pluralist. For example, if we define "weather" as the target phenomenon, can we even be sure that both approaches define the same phenomena as “weather?”

I aim to classify SP in a way that avoids both extremes. One necessary condition of SP, naturally, is

> “plurality” – several theories, explanations, or methods approach the same natural phenomenon.

Apart from plurality, it is hard to pinpoint an idea accepted by all scientific pluralists. In the remainder of this chapter, I will restrict my focus to pluralism in the natural sciences. It is important to note, nevertheless, that SP has also been formulated for other empirical science such as, for example, economics (Hoover, 2017; Sent, 2006). Despite my focus on the natural sciences, pluralism is a fruitful option in other disciplines as well. Pluralism, in addition, has also been proposed as a perspective for understanding interdisciplinary work (Kellert, 2006).

As discussed in previous paragraphs, scientific pluralists reject monism. I will ground the second necessary and sufficient criterion on this rejection by adopting Kellert, Longino, and Waters's definition of monism.

> “1. The ultimate aim of a science is to establish a single, complete, and comprehensive account of the natural world (or the part of the natural world investigated by the science) based on a single set of fundamental principles;
> 2. the nature of the world is such that it can, at least in principle, be completely described or explained by such an account;
> 3. there exist, at least in principle, methods of inquiry that if correctly pursued will yield such an account;
> 4. methods of inquiry are to be accepted on the basis of whether they can yield such an account; and
> 5. individual theories and models in science are to be evaluated in large part on the basis of whether they provide (or come close to providing) a comprehensive and complete account based on fundamental principles.” (Kellert et al., 2006, x)

It is important to note that monism can be an interpretation of plurality (2006, x). As Kellert, Longino, and Waters put it, plurality is a *description* of a state of affairs. Pluralism is a *view* about this state of affairs (2006, ix). The distinction between plurality and pluralism is essential. A negative attitude concerning plurality is not pluralism. For example, Thomas Kuhn describes pre-paradigmatic science as displaying plurality (Kuhn, 1962/2012, 162). "Mature" science, however, is governed by only one paradigm. It would be odd to call Kuhn a pluralist only because he diagnoses plurality as a trait of pre-paradigmatic science. We, thus, need additional criteria to complement "plurality."

Kellert, Longino, and Waters suggest that a pluralist should treat the second tenet of monism as an open question. There are no good reasons to presuppose the structure of the world. What the world is like is an empirical question. Agnosticism regarding tenant two undermines all the other tenants of monism (2006, xf). One other essential ingredient of SP, thus, is "non-monism:"

> Non-monism – denial of, or agnosticism regarding whether the nature of the world is such that it can, at least in principle, be wholly described or explained by one comprehensive account.

It is crucial to allow for two possibilities for non-monism. Rejecting or being agnostic about monism are both possible ways to motivate non-monism. Both versions reject the a-priori acceptance of monism. My definition of non-monism is, therefore, different from the one by Kellert, Longino, and Waters, as it includes the rejection of monism. Their version only covers agnosticism which is related to the particularities of their pluralist position, the "Pluralist Stance" (PS). By limiting the motivation of non-monism to being agnostic about metaphysics, many pluralist positions are left out. As will be shown in the following sections, several pluralist positions base their arguments on an in-principle rejection of monism (for metaphysical or epistemological reasons). Therefore, my definition of "non-monism" is more comprehensive. It accepts all commitments that are non-monist, no matter whether it is denial or agnosticism, no matter whether it is metaphysically or epistemologically motivated.

As Kellert, Longino, and Waters point out, plural*ism* is an attitude about something, an agenda, a system of beliefs. Pluralism is not neutral. This issue has also been elaborated on by the philosopher of economics Uskali Mäki. Plurality is a descriptive category that describes the multiplicity of an item (Gräbner & Strunk, 2019, 2). Pluralism is a normative principle "that justifies or legitimizes or prescribes a plurality of items of some sort" (Mäki, 1997, 38). Therefore, we need one more criterion for SP.

Approval – a positive attitude towards plurality.

Scientific pluralists do not regard plurality as a shortcoming of investigative strategies. Pluralism is, instead, an adequate way of approaching particular phenomena.

These three tenets are necessary and sufficient criteria that define scientific pluralist positions. As stated above, these criteria describe philosophers' pluralist attitudes. In Chapter Four, I presented data showing that scientists, if they supported a position that resembles pluralism, supported "plurality" and "approval." They did not, however, motivate their position through a rejection of monism. Instead, they motivated their pluralism with fallibilism or a rejection of absolutism. Differences between philosophers' scientific pluralism and scientists' pluralism become visible in this way. I will trace these differences further when surveying different options for scientific pluralists in the following section.

5.1.2 Additional options

Having provided three necessary and sufficient criteria for SP, I will explore additional options that differentiate individual pluralist positions. In this section, I will discuss: 1) the motivation of the position, 2) the use of case studies and their status, 3) normative vs. descriptive pluralism, 4) "levels" of pluralism, and 5) the relation between approaches.

5.1.2.1 Motivation (ontological, epistemological, values)

There are three options concerning the motivation of one's pluralist position. First, I will discuss ontological commitment as a motivation. There are several possible versions of ontologically motivated scientific pluralism. One centers around the question of whether pluralism is a position about one world or many worlds. A pluralist position can invoke one or many worlds. One way of arguing for many worlds is by proposing ontological abundance. Ontological abundance entails the existence of more than one empirical world. A pluralist could claim that several worlds are certainly imaginable. It is far more common, though, for scientific pluralists to defend a pluralist position about one empirical world[38]. A popular way of motivating one's ontological pluralist position is by referring to an infinite complexity of the empirical world. In order to know the world, it is necessary to have simplified perspectives on it. There need to be many different perspectives to sufficiently capture reality.

The second option is to motivate one's pluralist position on an epistemological basis. Epistemologically motivated scientific pluralists remain agnostic about what the world is like. The benefits of pluralism lie somewhere else. Pluralism has epistemic value because it is the superior way of gaining knowledge – no matter what the world is like.

In the previous paragraphs, I provided two motivations for pluralist positions – the epistemological and the ontological. Both views can be combined, but this need not be the case. If one proposes that the world is ontologically pluralist it is likely for one to believe that it should be investigated by a plurality of approaches. Vice versa, implementing a pluralist epistemology might lead to conclusions about the empirical world. If various accounts are required, this might resonate with a world that displays ontological plurality. But there are other possibilities as well.

One might hold that even though the world is infinitely complex (ontologically pluralist), it should nevertheless be studied by a single approach. Such a position might be influenced by the approaches that are currently present. One approach, for example, might be much more successful than the others. Also, it is possible to defend "the unity of scientific method." Each layer of a metaphysically pluralist world should be investigated by the same method.

Motivating one's pluralist position epistemologically does not require being an ontological pluralist. For example, one could argue that even though the world is monistic, a pluralist methodology is the best way to account for it. Hasok Chang's epistemological benefits

[38] Positions about many worlds, such as, for example, Goodman's and Rovane's "multimundialism" (Goodman, 1978; Rovane, 2013) are common in other areas of philosophy.

surveyed in chapter two, for example, could motivate the maintenance of several approaches.

The third and last option to motivate one's SP is motivating it by referring to a particular set of values. There are different sets of values. One set of values that are considered strictly epistemological are the – canonical – cognitive values (simplicity, precision, explanatory power, unification, and the like). Other sets of values go beyond what is traditionally conceived as epistemological. Feminist sets of values, for example, provide an epistemological perspective on how and what knowledge should be attained. Human beings have heterogeneous needs. Science, therefore, should address these needs. Science should serve to establish equal rights for everyone. Such values, therefore, have a political agenda. They are thus epistemological with a "political" component. They both determine how knowledge should be attained but also specify what kind of knowledge should be produced. In order to achieve equality, these values need to govern scientific inquiry.

It is possible to combine different elements of different sets of values. Together, they designate a specific set of "heuristics" (Longino, 2008, 69). In this context, "heuristics" means that a specific combination of values (cognitive and political) guides solving a problem or choosing a theory. Adding a value such as "clarity" or "explanatory power" to a primarily political agenda does not render this agenda less political. Of course, pluralism on its own can also be a value. I will provide a more extensive discussion of values and scientific pluralism in Chapter Six. Particularly, I will examine whether value-based epistemologies shield SP from epistemic relativism.

Interviewees subscribed to two of the three possible ways of motivating one's pluralist positions. They motivated (or rejected) SP based on either metaphysical or epistemological considerations. Some supported their epistemological commitment with a metaphysical presupposition and vice versa. They did not support a value-based motivation of pluralism, except for quite general pleas for "tolerance," against "dogmatism," and for sensitivity to empirical facts.

5.1.2.2 Case studies and their status

Another way of distinguishing scientific pluralist positions is to ask whether they employ case studies. And, if yes, how the case studies inform SP. Case studies might cover cutting-edge research or historical material. Case studies based on sociological, ethnomethodological, or experimental philosophical approaches are also possible. For example, I regard my account of the small RNA inheritance field as an example of an empirical case study on scientific pluralism. So far, however, there are not many other empirical approaches that explicitly target scientific pluralism.

There is much literature on periods of plurality authored by philosophers, historians, or sociologists of science. But these case studies were not initially aimed at studying scientific pluralism. For example, in Chapter Three, I reviewed Collins's case study on plant responses to remote stimuli. Collins discusses this case to reconstruct an instance of the Experimenters' Regress. It, nevertheless, also presents a case in which a research field was confronted with a plurality of theories. Such case studies might, therefore, be of interest to card-carrying scientific pluralists. Scientific pluralists could, therefore, conduct meta-studies of HPSS literature that studies periods of plurality.

Case studies might have several consequences. On the one hand, they might lead to ontological conclusions of two kinds. It is possible to use a case study to claim that the studied field is (fundamentally) pluralist. Or the respective case study might serve an even bigger claim –to take it as evidence that all domains of science are pluralist. In chapter three, I introduced the distinction between "local" and "global" SP. Local pluralists stay within their fields of study. Global pluralists infer from the conclusions of a case study in one field about other fields as well. Conclusions, however, need not be ontological. They can also be epistemological. If a case study shows that plurality benefits a discipline epistemologically, it is possible to expand this claim to science in its entirety.

5.1.2.3 Normative or descriptive pluralism

In Chapter Three, I introduced the differentiation between "normative" and "descriptive" SP. I will briefly summarize my elaboration. Descriptive SP employs case studies to claim that one subdiscipline, one science, or "the sciences" display plurality. Descriptive pluralism involves not only the analysis (plurality) but also the approval (pluralism). Even though it obviously sounds odd, descriptive SP is, thus, also normative, or, at least, evaluative. Vice versa, normative SP need not be descriptive. Normative SP is prescriptive. The state of a particular discipline or the sciences in general is of little relevance to the normative scientific pluralist. She claims that one (local) or all (global) scientific endeavor(s) should be pluralist. This normative stance can either be grounded in metaphysical or epistemological intuitions.

5.1.2.4 "Levels" of pluralism

Another distinction of pluralist positions concerns the levels on which SP operates. In what follows, I will work through several options. One option is to propose pluralism concerning the "strata" within one scientific discipline. This option emphasizes the independence of theory, experiment, and instrument. Such a version of SP might not seem very provocative. When formulated, "strata-pluralism" criticized the Popperian/Lakatosian position that the experiment only serves as the testing ground of theory. Proponents of strata-pluralism claimed that experiments and instruments have lives of their own. This position was most popular within the philosophy of physics and is, in principle, rather uncontroversial nowadays. Philosophers of science Peter Galison (Galison, 1997) and Ian Hacking (Hacking, 1983) are advocates of strata-pluralism. Although strata-pluralism is somewhat accepted, it might prove to be an exciting exercise nevertheless to enquire about the roles of theory, experiment, and instrument in different research traditions. I demonstrated in Chapter Four that the "strata" within one field are negotiated continuously. Theory, I showed, might also have different roles for different purposes within a field. Purposes are not always "purely" epistemological but might also be rhetorical. In conclusion, given current trends within the philosophy of science, "strata" pluralism might need to be revisited.

A second level of putting forward a pluralist position is to argue for pluralism within a discipline. For instance, one could state that in order to explain one target phenomenon – for the case reviewed in Chapters Two and Four, the target phenomenon was inheritance – several non-reducible accounts need to be employed.

An even broader level for advancing a pluralist position is to advocate pluralism of scientific disciplines. Such a position entails that there are no fixed relations between the different scientific disciplines and no reduction basis. Such a position frequently counters foundationalism. Foundationalism is the position that all scientific disciplines have a shared reduction basis. Pluralism about scientific disciplines can also be practically motivated. Knowledge derived from several disciplines needs to be employed to solve a particular problem. Pluralism, as a perspective on interdisciplinarity, also plays a role in these cases.

It is important to note that formulating a pluralist position might involve pleas for the inclusion of enterprises that are not commonly understood as scientific. Some scientific pluralists limit their discussions to what they call science. Of course, science is a normative concept. When we use the term "science," we usually commit to beliefs regarding what is "scientific" and what is not. For this section, however, I will presuppose a consensus on what it means to qualify as "scientific."

Some scientific pluralists expand their pluralisms to realms that are not generally considered scientific. Chang, whose position I introduced in Chapter Two, admits, for example, the inclusion of creationism. But philosophers usually draw a line between the plurality of quantum and classical mechanics and reintroducing the light-bearing ether into physics, for example. Those pluralist approaches that do not draw this line are usually quite controversial. Versions of SP that admit approaches that are not considered scientific can operate on two distinct levels. Either, one rather "maverick" approach might be admitted within a discipline. Or a whole intellectual enterprise, say, voodoo is admitted as a new discipline. Both intra and interdisciplinary pluralism might, thus, be concerned.

There is one more level at which pluralism might apply, Ian Hacking's "styles of reasoning." I briefly referred to Hacking's concept of "styles of reasoning" in Chapter Three. I will expand on them in what follows. Hacking draws his conceptualization from historian Alistair Crombie who distinguishes six distinct styles of thinking that developed since the European middle ages:

> "a) the simple postulation established in the mathematical sciences; b) the experimental exploration and measurement of more complex observable relations; c) the hypothetical construction of analogical models; d) the ordering of variety by comparison and taxonomy; e) the statistical analysis of regularities of populations

and the calculus of probabilities; and f) the historical derivation of genetic development;" (Crombie, 1981, 284, quoted in Hacking 1982/2002, 161)

Hacking adds that these styles can "die out" and become inaccessible to later generations. Styles of one historical episode might seem entirely inaccessible to assess from our perspective. Ways of thinking can be disparate. They decide on what counts as evidence and determine the knowledge that is produced (Hacking, 1983, 128). Hacking claims that cutting-edge science recently witnessed the emergence of a new style of reasoning. The "laboratory style" is a mixture of Crombie's b) and c).

Styles of reasoning can be incommensurable. Some claims can be made in any language, whereas others are specific to a particular style of reasoning. In the latter case, the respective claims are dissociated (1983, 71). Hacking considers different styles of reasoning: the historical, the legal, the mystical, and the magical… Taking the concept of styles of reasoning seriously, one could even argue that objectivity or at least our conceptualization of it could be a particular style of reasoning.

So far, I have focused on the theoretical side of the "levels" of SP. From this elaboration, it follows that a whole range of positions concerning pluralism of scientific methods is possible. As for theories, I will work my way up from the very "low-level" to the "high-level" examples. A very low-level version would be to propose pluralism concerning the different methods employed in the wet lab. In Chapter Two, I have pointed to a disciplinary and methodological divide within molecular biology: biochemistry and genetics. There is no particular similarity between those two approaches, yet the same result can be produced. At best, both are employed to double-check. This can be seen as an instance of very low-level methodological pluralism.

A slightly higher level is also imaginable. Methodological choices are not only determined by disciplinary commitments (e.g., biochemistry vs. genetics) but also by other beliefs as well. Take the example of small RNA inheritance. Roughly speaking, there are two groups of researchers. One believes in RNA inheritance and the other does not. Both want to produce the same phenotype. In this scenario, one way of producing the desired phenotype would be to rely entirely on Mendelian genetics. Those who also believe in small RNA inheritance would probably use a different approach. They would take into account non-Mendelian inheritance and probably employ a different methodology. For example, they might consider that inherited material is not equally distributed between daughter cells. In this case, not only the method is different. But theoretical commitments determine the plurality of methods.

SP of methods can also expand to different styles of reasoning. I will come back to Hacking's example concerning cures for syphilis. Syphilis might either be treated by applying antibiotics or a salve of mercury. In this case, adopting Hacking's language, there is no way of expressing the rationale of the one approach in the language of the other. In the previous example, in contrast, the Mendelians might understand what the non-Mendelians mean but think that they are wrong. The most extreme case of SP concerning methods would be cases in which methods are plural in the sense that they are built on other conceptions of rationality or objectivity or causality, or even styles that exclude one or all of these concepts.

5.1.2.5 Relation of theories – integratable or not integratable

I elaborated on the issue of integration vs. non-integration extensively in Chapter Three. This differentiation concerns the question of how different approaches relate. Mäki calls this difference "substitutive" vs. "complementary" approaches (1997, 45). "Toleration of rival claims to truth is more demanding than toleration of complementary claims" (Gräbner and Strunk, 2018, 3). In Chapter Three, I considered whether scientists regard theories as integratable or non-integratable (as substitutive or complementary). I will proceed to assess whether this divide also differentiates philosophers' positions. In Chapter Three, I criticized pluralist positions that believe that essential properties of phenomena determine whether they lend themselves to substitutive or complementary accounts. While adhering to this criticism, it is nevertheless fruitful to classify pluralist positions regarding these takes because the differentiation between integration and non-integration is ubiquitous in the scientific pluralism literature.

There are several possibilities for the scientific pluralist. She could define approaches as "truly" incommensurable. They are incommensurable in that they are not making sufficiently similar contacts with exterior reality. They might not even share a set of problems they approach. Another way is to grant that there is no way of integrating approaches. But for solving a specific set of problems, approaches need to be combined. If combined, they have more problem-solving potential than each approach individually. We have, thus, a contingent and purpose-dependent admission of combining different approaches, at least. Approaches are not integratable but they also do not pass each other like ships in the night.

A third way of relating approaches within one's pluralist account is through "ad hoc" integration. Theories are idealizations. On the abstract level, different theories that approach the same target phenomenon are not combinable. But to describe a specific target phenomenon (the phenotype of a particular organism, the ecosystem of a particular lake, the way one particular pen falls on the floor) or to build a particular tool (GPS), they can be integrated ad hoc to provide one causal account. An even stronger position regarding integration would be to claim that not only in the particular but also on the theoretical level, integrated accounts are possible. For such cases, the scientific pluralist would have to provide sound arguments for how this version of "integration" is different from "unification."

Having introduced necessary and sufficient criteria for scientific pluralism along with some specifications, in the next section I will survey pluralist positions of philosophers of science. This survey will serve as an exemplification of what I outlined theoretically in this section.

5.2 Scientific pluralism, a survey

In the last section, I provided necessary and sufficient as well as optional criteria for scientific pluralists. In this section, I will illustrate these elaborations with examples. I will review the positions of card-carrying scientific pluralists. These examples will comprise influential "forefathers" of pluralism, such as Otto Neurath and Paul Feyerabend, the proponents of the so-called "Stanford School" in the 1980s and 90s; and recently formulated scientific pluralist positions. In so doing, without claiming or aiming for completeness, I will provide a short list of the most influential programs past and present.

The positions surveyed were selected to produce a survey of positions as diverse as possible.

I will not provide an exhaustive account of the intellectual history of each scientific pluralist. I will summarize, instead, the most influential and distinct positions of each philosopher in three (or four) theses. Each of these theses will be complemented with comments and citations, where necessary.

This section aims to illustrate abstract claims of the previous section with examples. I will demonstrate how card-carrying scientific pluralists realize the different optional ingredients. This survey, in turn, will inform discussions in the section to follow. In the next and final section of this chapter, I will criticize conventional ways of classifying pluralist positions and will explore new classificatory approaches. I will push the positions surveyed here in the service of two agendas. First, referring to these authors, I will show that these current classifications fail to capture pluralist positions. Second, I will propose new classificatory approaches and examine whether they characterize the positions surveyed here well.

I will now commence with the survey. The "history" of scientific pluralism is usually narrated by starting with the rise of the "Stanford School." In the early 1980s, a group of philosophers based at Stanford University provided influential arguments for the disunity of science. Their work was foreshadowed by Patrick Suppes's address to the Philosophy of Science Association in 1978. In this address, Suppes made a plea for acknowledging the plurality of science. Together with Suppes, Nancy Cartwright, John Dupré, Ian Hacking, and Peter Galison count as the canonical members of the "Stanford School." Another possible "origin" of scientific pluralism is the notion of "theoretical pluralism" developed by Paul Feyerabend in the early 1960s. Feyerabend, although influential, is the *enfant terrible* of scientific pluralism. Most or even all scientific pluralists go to great lengths to distinguish their position from Feyerabend's.

I will start neither with Feyerabend nor Suppes but with a pluralist that lived much earlier – Otto Neurath. Before surveying Neurath's takes regarding SP, I will spill some ink on the "Unity of Science Movement." In collaboration with Rudolf Carnap, Neurath tried to realize this unity through the "Encyclopedia of Unified Science." But strikingly, Neurath's notion of unity is compatible with scientific pluralism and the disunity of science. Various scholars have contributed to explaining this tension. I will, therefore, keep this elucidation as concise as possible.

In the interwar period, the unity of science had a different agenda than the name suggests. It is our postmodern ears – Hacking suggests – in which “unity now rings…hegemonic, patriarchal, imperial” (1996, 41). Unity of science was a rallying cry of the “rational forces” against the threat of upcoming “irrationalities” – such as fanatism, fascism, and intolerance. The disunity of European societies and scientists was to be prevented. Unity and disunity had thus different connotations. The seeming anti-themata of (interwar) unity and (postwar) disunity share similar agendas – providing a safe space for knowledge. Of course, the agenda of the post-war version of scientific pluralism sounds different compared to its interwar pendant. It emerged together with strong political movements such as feminism, new age, the 68 counterculture, and the civil rights movement. These movements have in common that they cherish diversity (ibid.).

With that in mind, we might try to look into the future of unity/disunity. Will the 21st century cherish diversity and plurality in the same way as the second half of the 20th century did? Or will scientific unity again be required as a stronghold against the aftermath of post-truth and alternative-facts movements? Having hinted at historical and political embeddings for unities or disunities of science, I will review versions of SP. Having surveyed these positions, I will evaluate and compare them in the remainder of this chapter.

5.2.1 Otto Neurath

> (ON1): There are no secure foundations of knowledge and no fixed relations between areas of knowledge.

Neurath acknowledges the substantial complexity of the natural and social world. This complexity renders it impossible to account for all natural phenomena with only one theory. Similarly, Neurath deems it highly unlikely that there is only one single foundation of knowledge or a single true method for approaching the foundations of knowledge. The world, as a consequence, is intrinsically disunified. Neurath’s commentators Nancy Cartwright, Jordi Cat, and Lola Fleck characterize his position as empirically motivated. But they also opine that Neurath draws metaphysical conclusions. This characterization might shock the connoisseurs of Neurath's philosophy. Neurath would never have used the term "metaphysical pluralism," he even added the term "metaphysics" on his *index verborum prohibitorum*. Following the spirit of the *Wiener Kreis*, he believed metaphysics should have no bearing on science. But Neurath's emphasis on complexity led Cartwright, Cat, and Fleck to characterize him as a metaphysical pluralist (Cartwright et al., 1996, 209).

Neurath denies that the reduction of individual branches of sciences to more "fundamental" ones is possible. Instead of a "pyramidal" model of knowledge with one foundation, he proposes that knowledge is organized like the different entries in an encyclopedia. An encyclopedia comprises radically different subjects that share no common ground. These subjects can, nevertheless, be accommodated by the same encyclopedia. The relation of these different entries is never fixed, and boundaries are flexible and locally defined in a purpose dependent manner (Cartwright et al., 1996, 169). The same is true for the diverse areas of the disunified sciences. One attempt to provide such an account was the "Encyclopedia of Unified Science."

> (ON2): Different theories can be built on the same foundations. No science can be tested against the "real world."

To Neurath, "reality consists of an infinitely manifold stream of events" (1996, 169). Different theories can be built on the same foundations, dependent on auxiliary hypotheses. There is no way in which a theory can be "tested" against the real world. Thus, Neurath supports a version of anti-realism (1996, 169).

Neurath's ideas draw heavily on the underdetermination of theory by evidence. "Everything flows" concerning the relation of theories to foundations (1996, 81). The properties of "Wirklichkeit," "reality," are not of interest to Neurath. His anti-realism has no specific bearing on his pluralist agenda. Neurath's emphasis lies on the epistemological consequences of metaphysical disunity.

> (ON3): Unity of Science is only possible when solving specific, practical problems.

Although the sciences are disunified, disciplines can join forces to solve specific problems. The metaphor of the encyclopedia is essential: individual languages of scientific disciplines are highly specified. But in an encyclopedia, scientific facts need to be expressed in a way to become accessible for a broader audience. A "universal slang" needs to be created. (1996, 96). In order to solve a specific social or scientific problem, disciplines maintain communication by speaking such a slang.

Neurath imagined the relations of different theories and disciplines in a particular way (ON1). The relations are flexible and context-specific. A solution to a problem is a local merger of several disunified fields. The particularities of this solution are contingent and historically conditioned. The truth of such solutions does not matter but their effectiveness does (1996, 244). Every concrete solution is historically contingent and only justified in its specific context. Disunity has to be overcome when action is required.

5.2.2 Paul Feyerabend

> (PF1): Theories that are kept from scrutiny become "myths" – they lose their empirical content.

Feyerabend is suspicious of "well-established theories" because they quickly become dogmata. These dogmata are kept away from scrutiny and criticism. Because criticism is disabled, dogmatic theories lose their empirical content – they become myths. In his early writings, Feyerabend argues that a combination of non-integratable theories provides richer access to external reality (Feyerabend, 1975/2010, 21).

> (PF2): Counterinduction – theories need to be tested against alternatives; there is no constraint on their source.

> "You can be a good empiricist only if you are prepared to work with many alternative theories rather than with a single point of view and "experience." This plurality of theories must not be regarded as a preliminary stage of knowledge which will at some time in the future be replaced by the One True Theory. Theoretical pluralism is assumed to be an essential feature of all knowledge that claims to be objective" (Feyerabend, 1963, 80).

The need for consistency and meaning invariance should be abandoned. The only reason to abandon a theory is if it disagrees with facts. Alternative theories might be the only source to make new, decisive facts available to test other theories (1963, 90). Sources for alternative theories can be manifold: the history of science, ancient myths, and modern lunatics. By accepting these sources as valid options for theories, the distinction between the history of science, philosophy of science, and science dissolves. So does the demarcation between science and non-science. By creating a plurality of theories, the contact with reality can be maximized (1975/2010, 27).

> (PF3): Standards are always needed – the history of science shows that no standard is universal.

Standards are necessary but not universal: “the one man’s “reason” is the other man’s insanity” (1975/2010, 165). Standards must never “bend minds.” Crucial experiments cannot decide between two theories. Theories define the framework of an experiment. Alternative theories might not share enough common ground and not the same observation language. If alternatives satisfyingly approach the same facts and show internal consistency, there is no reason to abandon all but one (1975/2010, 220f). The world can be accounted for in many different ways; standards that grant freedom for one way will severely constrain the other. The danger of absolute standards is that people disobeying those standards get robbed of their reason.

> (PF4): Different conceptions of reality all have empirical grounding.

In his later writings, Feyerabend supports a metaphysical version of SP. Even among scientists, there are different conceptions of reality, which all have empirical grounding. “Non-scientific” sources can be equally successful in capturing a reality of the world. He calls such realities “manifest worlds.” Switching between those worlds is not possible by comparison – big junks of one world need to be abandoned to convert to another (Feyerabend, 1999/2005, 218). What lies outside these worlds – the “outer reality” is not attainable (ibid.). There is no independent basis from which to judge different manifest worlds.

5.2.3 The Stanford School

Having reviewed Neurath, I will turn to the "wave" of pluralist thought in the 1980s– the "Stanford School." I will start with Suppes's presidential address to the Philosophy of Science Association of 1978. In what follows, I will elaborate on Cartwright, Hacking, Galison, and Dupré.

5.2.3.1 Patrick Suppes

> (PS1): The sciences are disunified, reduction is not possible.

Suppes distinguishes three types of reduction: language, subject matter, and method. All types of reduction are bound to fail (Suppes, 1978, 5ff). The impossibility of the reduction of scientific language is grounded on an empirical observation: it has become impossible, even for scientists within nearby disciplines, to understand the scientific writing of their peers. To Suppes, this is more a practical than a theoretical issue. Reduction is not impossible in principle but not feasible to solve practical problems. Researchers in different branches of one scientific field do not need to know adjacent fields. And researchers do not have enough time to do so. The "cognitive facts of life" cause the plurality of scientific language (1978, 6). Plurality in science is as desirable as the "irreducible plurality of political views in a democracy" (ibid.).

The impossibility of the reduction of the scientific method is informed by Suppes's view on the plurality of scientific language. It has become impossible for experts to understand the handbooks and manuals of fields closeby (1978, 8). Methods are distinct in each scientific field and there is no one "true" scientific method. The reduction of subject matter, ultimately, is impossible, because although there might be ultimate entities, we do not know what these entities are. Current knowledge about the universe suggests disorder instead of unity. Higher-level problems and theories are much better understood that their "foundations" (ibid.).

> (PS2): The phenomena which science investigates are complex. Certainty and completeness are not attainable.

Suppes challenges the possibility of certainty and completeness of scientific theories. Suppes points to random fluctuations in physics as an example. Real quantities have no clear meaning and no precise value (1978, 10). As absolute certainty fails for physics, it fails for other branches of science in the same manner. Suppes denies the possibility of completeness because the universe is not "fully knowable in character" (1978, 11). The only area where completeness can be realized is first-order logic, but in practical mathematics, it becomes useless (ibid.). Along these lines, Suppes also points to incompleteness in geometric constructions, in set theory, and to the failure to formulate a unified field theory (1978, 11ff). Suppes concludes that any theory in any field can only account for a restricted part of a phenomenon.

> (PS3): Science is a human activity – it focuses on specific problems and not specific theories.

Suppes' claims about the certainty and completeness of theories are not metaphysically motivated. Instead, he points to the limitations of human knowledge. Suppes claims, drawing from Peirce and Dewey, that science is a continuous problem-solving activity. Science is not primarily interested in formulating one complete and certain theory but rather in developing solutions to problems. Without exploring one theory to its limits, scientists might move on to the next. Our best theories are "local and are designed to meet a given set of problems" (1978, 14f). Even though our theories might improve, there is no finite value up to which they can improve. Science will always be pluralistic (1978, 15).

5.2.3.2 Nancy Cartwright

> (NC1): The world consists of patches about which we can have knowledge; laws only hold within their patches.

Neurath's philosophy has profoundly influenced Cartwright's pluralist position. But in contrast to Neurath, metaphysics plays a substantial role in her version of SP. Cartwright grounds her pluralism in case studies in the fields of physics and economics. Her conclusion, however, is global. The world is "dappled." It consists of a "patchwork of laws" with no fixed relations (Cartwright, 1999, 31). "Nature is governed in different domains by different systems of laws not necessarily related to each other in any systematic or uniform way; by a patchwork of laws." (ibid.).

Cartwright's view on laws is also based on her argument that universal laws are only true *ceteris paribus*. Laws only hold in conditions where every parameter is controlled. For "real" phenomena, universal laws do not hold (1999, 39). While rejecting the truth of universal laws, Cartwright believes in the truth of phenomenological, local laws: anything general can only exist in particular (ibid.). Cartwright is a realist about theoretical entities. She believes that our notions of theoretical entities are resonant with entities that are "out there." Explaining phenomena by referring to a theoretical entity provides a causal explanation. Existence is an internal characteristic of causal claims. Therefore, theoretical entities explain because they exist (1983, 93).

> (NC2): To solve specific problems, we need to patch up knowledge from different areas.

In most cases, scientific practice is not deduced from one universal law but is the product of patching up knowledge of different areas. Cartwright aims at developing "methodologies, not for life in the laboratory where conditions can be set as one likes, but methodologies for life in the messy world that we inevitably inhabit" (1999, 18). Pluralist approaches are most successful in doing so.

Although constraints might exist on the abstract level, integration in practice proves successful. Theories might seem unreconcilable in theory but they can be combined for practical purposes. Connections of different patches of knowledge, which become necessary in practice, are always local and contingent. If too many areas get patched up at once, patches will get loose again – the unification of all patches is never feasible.

> (NC3): A wrong conception of the world (monism) leads to wrong tools to approach the set of phenomena the world consists of.

Cartwright holds that unity cannot be a goal because unity cannot explain what the world is like. Wherever we see order, we need to keep in mind that order is an artificial product of human activity, trying to force nature into ceteris paribus conditions. It is essential to bear in mind that the formulation of *ceteris paribus* laws and the production of *ceteris paribus* conditions have a positive feedback loop. Both processes are human activities. *Ceteris paribus* conditions lead to the formulation of *ceteris paribus* laws. *Ceteris paribus* laws reinforce and verify *ceteris paribus* conditions.

Cartwright does not support "prescriptive pluralism." She warns, however, of the consequences of a purely monist approach. "The worry is not so much that we will adopt wrong images with which to represent the world, but rather that we will choose wrong tools with which to change it" (1999, 12).

5.2.3.3 Ian Hacking

> (IH1): There is no hierarchy between theory and experiment, "representing" and "intervening" are two strata of each scientific discipline.

Hacking argues for the pluralism of two distinct strata of science: experiment and instrument. Hacking tries to establish the experiments' "own life". "Reality has more to do with what we do in the world than with what we think about it" (1983, 17). Experiments prove the existence of theoretical entities.

Similar to Cartwright, Hacking rejects the realism of theories but is a realist concerning theoretical entities: "So far as I'm concerned, if you can spray them then they are real" (1983, 23). If certain entities can be successfully manipulated and used in various experimental setups, they exist.

> (IH2): The sciences are disunified and should be conceptualized as family resemblances.

Hacking "cannot understand the idea of a complete theory of our interesting universe" (1983, 93). Disunity is an empirically well-established fact. Disunity manifests in the diversity of scientific languages, overspecialization in science, constant redistribution of power, and the emergence of new research clusters and foci (1996, 57).

To Hacking, it seems both unlikely and also uninteresting to find a unified theory or to express everything in the same language (1983, 94). Hacking doubts that all areas of science can have a common "unifier." The different scientific disciplines are related to one another in the form of family resemblances (1996, 68). Although all areas of research are perceived as scientific, there is nothing that all have in common that could be associated with their scientific character. Hacking imagines science as a "Borghesian library":

> "each book of which is as brief as possible, yet is inconsistent with every other. No book is redundant. For every book, there is some humanly accessible bit of Nature such that that book, and no other, makes possible the comprehension, prediction and influencing of what is going on" (1983, 219).

(IH3): There are many different styles of scientific reasoning.

Styles of reasoning are another source of pluralism. Styles of reasoning are ways of making sense of the world. They decide which propositions can have a true/false value, specify what counts as evidence and determine the knowledge that is produced (1983, 128). Such styles of reasoning can be incommensurable. Styles of reasoning can exist synchronously and diachronously.

The different classifiers that are used are not entirely mind-dependent but have their truthmakers in the empirical world (1983, 95). For example, both Inuit and skiers classify snow: employed categories are not translatable and the categories do not match each other or can be seen in a higher-to-lower-level-relation. Hacking allows for an infinite number of styles of reasoning. Yet, the world decides what is in which category and whether these categories are useful. The incommensurability of styles of reasoning is also limited. Some claims might be made in any language, whereas others are particular to an individual style of reasoning; in that case, they are dissociated (1983, 71).

5.2.3.4 Peter Galison

(PG1): "Postmodern model[39]": each scientific discipline consists of independent strata: theory, observation, instrument.

[39] Galison calls the modern "postmodern," referring both to "modernism" – the positivist movement and "postmodernism" – the antipositivist movement

Galison mainly locates his pluralism within the disunity of theory, observation, and instrument in physics. The postmodern model negates hierarchies between these strata. This model offers an alternative to both positivist and anti-positivist views concerning the relation of theory and observation. It incorporates positivist and antipositivist notions. It is based on the view that observations do not give unmediated access to universals (antipositivism). But it allows that theories change while observations remain constant (positivism) (Galison, 1997, 793).

Different strata are relatively independent but intercalated. This secures stability. If there is a fundamental change in one stratum, this need not lead to a break in others. Changes in theory, observation and instrument never happen synchronously. The postmodern model allows for the existence of different substrata. Within the stratum of theory, several theories with independent relations to experiment and instrumentation can be accommodated. This adds another level of strength to the postmodern model – if one substratum fails, others can secure bonds with the remaining strata.

> (PG2): "Trading zones (TZs)": to solve specific problems, TZs are formed, a pidgin language[40] is used.

In addition to disunity within one field, science as a whole is also disunified. Science displays a variety of languages, practices, purposes, and arguments (1997, 13). How and under which conditions can proponents of different agendas communicate with each other? Galison suggests that communication can be secured through TZs. TZs function by coordinating actions and beliefs of different disciplines. Within TZs, the development of a pidgin language secures communication. All sites set specific constraints on the ways of exchange (1997, 806).

Galison characterizes himself as an "empirical realist." He aims at providing an account of what is there and how it works together. There are real strata and real interactions in real TZs (1997, 840). TZs are the best argument against framework relativism. Although

[40] A pidgin language is a hybrid language developed by two or more communities that do not have a language in common. Vocabulary and grammar are often quite limited. A pidgin language is never a first language (Bickerton, 1976).

disciplines are disunified and sometimes independent, they do not pass each other like "ships in the night." There is, thus, no incommensurability of approaches.

> (PG3): Communication between different strata and within TZs is secured, because of the constraints of scientific practice.

Discontinuities between strata make science robust – if one stratum fails, the whole discipline need not perish (1997, 829). Local trading zones form the clay between the disunified subject areas of science. Constraints secure that the different branches of science keep their ability to communicate. These constraints confine scientific practice (theorizing, experimenting, building instruments). In each stratum, it is not the case that anything goes. Strata, also, do not relate in any possible way. "The theorist is not free to admit any particle or effect in order to come into harmony with the experimenter" (1997, 801).

5.2.3.5 John Dupré

> (JD1): The disorder of things leads to the disorder of science.

Dupré argues for metaphysical pluralism. The world displays diversity. Science, as a result, can never constitute a single, unified project. The "disorder of things" is an intrinsic feature of the phenomenal world. The structure of the world cannot be deterministic, exhaustively law-governed, and potentially intelligible. The disorder (or the disunity) of science is a direct consequence of the disorder of things (Dupré, 1993, 1).

Dupré makes use of examples to support his claims. Dupré, however, claims that both in science and in philosophy, it is natural to start with strong metaphysical convictions. The disorder of things rejects essentialism, reductionism, and determinism.[41]

> (JD2): Promiscuous realism – there are "real" scientific objects in the world, yet no constraints on how to classify them.

Dupré characterizes his pluralism

[41]Dupré's metaphysics changed drastically over time. In his latest publications on process ontology, Dupré argues for ontological monism. Processes are the unified ontological basis of all biological phenomena (Nicholson & Dupré, 2018).

> "…as the claim that there are many equally legitimate ways of dividing the world into kinds, … and second, in opposition to reductionism, pluralism as the insistence on the equal reality and causal efficacy of objects both large and small" (1993, 7).

The metaphysical disorder of the world presents no "obstruction to reality." There are real objects in the world but their respective classification is purpose-relative (1993, 262). Depending on the particular purpose of a classification, things will belong to one class or the other. Dupré claims that his position is relativist in the sense that classifications and approaches to make sense of the world are always purpose-relative (1993, 261f).

There is no unity amongst the sciences. Instead, the different disciplines and approaches are family resemblances (1993, 224). There is only one kind of scientific unity, which is sociological. Dupré defines this sociological unity as scientism. Scientism is detrimental because it enables individual institutions to keep a stronghold against alternative approaches. Scientism attributes too much power to disciplines that are regarded as "more fundamental" than others. Lack of criticism leads "science" to become a non-contested epistemic standard. This epistemic authority can be used against outsiders.

> (JD3): No distinction between science and non-science, yet epistemic virtues help to decide between "good" and "bad" theories.

Demarcation of science and non-science is not possible. Meaningful classification is not only a scientific activity. Although a criterion of demarcation is not required, it is still possible to decide between good and bad theories. A set of epistemic virtues determines these decisions (1993, 243). These virtues include "sensitivity to empirical fact, plausible background assumptions, coherence with other things we know, exposure to criticism from the widest variety of sources" (ibid.).

Dupré's position is descriptive and not prescriptive. His pluralism does not require to "let a thousand flours bloom." The disorder of things and the disorder of science are empirical facts. There is no reason why more theories or classifications should be introduced. In other words, Dupré does not require more disorder/plurality. He also tolerates exceptions to his position: “truly tolerant pluralism should surely allow the occasional appearance of a plausible candidate for a real essence” (1993, 55).

5.2.4 The “millennial” pluralists

After having discussed a so-called “first wave” of scientific pluralist positions, I shall now introduce the “second wave” of SP. These positions have all been formulated after the year 2000 and are thus quite recent. What characterizes pluralist positions formulated after 2000 is their focus on scientific practice. Because of this focus, “theory pluralism” usually does not play a significant role.

5.2.4.1 The Pluralist Stance / Helen Longino

> (PS1): Every single phenomenon should be empirically tested whether it requires pluralism or not.

The Pluralist Stance is empirically motivated. Each phenomenon should be tested regarding whether it is pluralist or not (2006, xiii). Kellert, Longino, and Waters exemplify their methodology by surveying case studies from different fields that suggest plurality within those fields. “Black-boxed” knowledge should also be investigated for potential (overlooked) plurality (Longino, 2002, 189). If two or more theories produce knowledge but are not integratable, it would be harmful to science to decide between them (2006, xxivff).

> (PS2): Tempered realism – PS1 will not lead to metaphysical conclusions.

Some scientific phenomena might require a monist interpretation and others a pluralist interpretation. Neither monist nor pluralist accounts will bear on metaphysical questions. Scientific data might be in touch with nature or not. That should not change our ways in which we exercise the Pluralist Stance (2006, xxiv).

> (PS3/HL1): To keep specific theories out, a set of standards needs to be applied.

Kellert, Longino, and Waters emphasize that their position is distinct from accepting “any” theory. They believe in constraints “that limit the variety of acceptable classificatory or explanatory schemes.” The Pluralist Stance needs conceptual tools for deciding between “good” and “bad” research. One example of “bad” research is research that tries to eradicate pluralism. Proponents of the Pluralist Stance, thus, need to form consensus concerning a set of scientific virtues or standards. One clear example is Helen Longino's value heuristics. Value heuristics are a combination of values that might be cognitive, political, or both (Longino, 2008, 81). "Different sets of heuristics…will satisfy the different cognitive goals" (ibid.).

5.2.4.2 Hasok Chang

(HC1): It is always beneficial to have various systems of practice around.

Systems of practice are

> "coherent and interacting sets of epistemic activities performed with a view to achieve certain aims. Each system embodies an account of the aspect of reality that is its subject area, and methods for creating and using such accounts." (2012, 16).

The cultivation of different systems of practice leads to various "benefits of toleration" and "benefits of interaction," which I reviewed extensively in Chapter Two.

(HC2): We should actively introduce systems of practice

Chang's pluralism is normative. Every scientific field should adopt pluralism. Even if a system seems "inferior" to others, it should be kept to reap the benefits of pluralism (2012, 224 and 282ff). Chang's pluralism is epistemic in that it claims that pluralism is the best way of gaining knowledge. Chang refrains from metaphysical conclusions because he considers them uninteresting and too weakly supported[42].

(HC3): "Many things (systems of practice) go" – depending on their Bayesian prior probabilities.

Chang believes that pluralism is a more significant threat to absolutism than relativism. Instead of accepting the possibility of various systems of practice, pluralism introduces them

[42] In his recent writing, Chang turns towards a more metaphysical version of SP, in which he admits "degrees of reality" of theoretical entities, such as phlogiston and oxygen (Chang, 2018, 33).

(2012, 261f). Chang introduces quite loose constraints for theory choice. f theories compete and one seems "weaker," it should not be abandoned.

5.2.4.3 Sandra Mitchell

(SM1): The complexity of biological phenomena leads to theoretical pluralism.

Mitchell acknowledges the complexity of biological sciences. Her pluralism aims at integrating this complexity. "Complexity in nature, particularly in biological nature, has direct implications for our scientific theories, models, and explanations. In other words, nature is complex and so, too, should be our representations of it".

(SM2): In biology, integration of "abstract models" explains phenomena.

Complexity in biological systems results in different ways of representing them. This leads to idealized models. Theoretical plurality on the abstract level entails explanatory pluralism on the concrete level. Mitchell claims that SP is a better explanation for the relation of different theories, models, and explanations than reductionism (2003, 182). Mitchell opposes an "anarchist view" of theory choice. Instead,

> "the challenge is to define clearly the middle ground: How can a diverse, well confirmed, but irreducible set of theories be used collectively to achieve a more complete understanding than any of the theories taken in isolation?" (2003, 186).

She opposes an isolationist account of theories. When differentiating levels of analysis, the possible interactions between those levels get limited (2003, 188). Mitchell does not applaud the independence of sets of questions and the resulting independence of the answers to those questions.

(SM3): For every single phenomenon, there exists one single account.

One of the most important questions is when "are two preferred explanations incompatible alternatives and when are they complementary components of a complete account of a biological phenomenon?" (2003, 1999). To understand and locate disagreement and its domain, the sphere of the idealized models needs to be distinguished from the sphere of the particular application of such models (2003, 203). To provide the causal story of one phenomenon, the theoretical plurality on the abstract level needs to be integrated. Thereby it is possible to provide one explanation on the concrete level. Explanatory, concrete integration is locally possible. Given a specific problem, e.g., the ecosystem of one specific lake, various models can be integrated in order to produce accurate renderings and predictions. This local unification will not apply to a slightly different lake. This results from the non-independence of some factors and the singularity of a particular combination of factors (2003, 207).

In this section, I surveyed various prominent pluralist positions. By choosing three main tenants, I focused on the most characteristic features of each position. In the remainder of this section, I will provide a table that summarizes how the surveyed positions fit the "additional options" for scientific pluralism provided in section 5.2.3.

5.3 Revisiting classifications

In this section, I will explore different ways of classifying pluralist positions. The plurality of pluralist positions is quite evident. Even though some positions do not explicitly disagree with each other, they are quite heterogeneous in emphasis and focus. Some versions of SP do certainly not go together. Classifications can be useful epistemic tools to organize and understand the relation between different forms of SP.

Several scientific pluralist positions rely on a case study in one scientific discipline. Specific problems and aspects of pluralism might become apparent through case studies in one discipline but not in the other. For instance, Sandra Mitchell grounds her pluralism in the specific character of biological phenomena. Different kinds of problems might challenge different research traditions. Different research traditions might, therefore, require pluralist positions with different emphases. The emphasis of the respective pluralist position will differ regarding specific problems it tries to solve. The particularities of a case study add another layer of diversity to pluralist positions. Classifications might also shed light on this issue.

In this section, I will first criticize the current proposal for classifying pluralist positions. This classification relies on a distinction between "radical" and "modest" pluralism. It was introduced by Kellert, Longino, and Waters. Both ends of their scale ("radical" vs. "modest") are undesirable, according to the authors. I will bring forward two arguments against this classification. First, I will argue that both classifiers (radical and modest) do not capture most scientific pluralist positions. Second, I will argue that a certain "radicalness" is characteristic of any scientific pluralist position. In so doing, I will demonstrate that these classifiers cannot distinguish pluralist positions. After presenting arguments against Kellert, Longino, and Waters' model, I will propose four alternative classifications.

5.3.1 "Radical" and "modest" do not differentiate pluralist positions

First, I will reconstruct Kellert, Longino, and Waters's classification of pluralist positions. Second, I will criticize their classification along several lines. My critique will be grounded in my rejection of the classifiers "radical" and "modest."

I will start with "modest" pluralism. There are, at least, two options for the modest pluralist. Either she believes in an in-principle translatability of theories, methods, or explanations. Or she regards a plurality within science as a result of technical or intellectual limitations. This plurality, yet, should dissolve in the future or is at least dissolvable in principle. These modest pluralist positions are monism in disguise (2006, xiff). The modest pluralist is not ready to accept that plurality might be not only the current state but also the goal of science.

"Radical" scientific pluralism does not fare much better, according to the authors. Radical pluralists deny that there is a rational basis for deciding between theories. Some state that an infinite number of theories should be introduced. Such positions lead inevitably to relativism (2006, xiii). Accepting relativism entails, according to Kellert, Longino, and Waters, abandoning judgment.

In conclusion, Kellert, Longino, and Waters's approach provides a way of classifying pluralist positions according to their monist or relativist tendencies. The authors locate their position, the Pluralist Stance in the middle of the scale. The Pluralist Stance shows neither the traits of radical nor the traits of modest pluralism. The classification Kellert, Longino, and Waters propose is, thus, not neutral. Both ends of it are undesirable, at least in the eyes of the authors. Both "radical" and "modest" pluralism are not only descriptive but normative concepts.

I will argue that "radical" and "modest" do not characterize pluralist positions well. They do not even hint at contents, problems, or peculiarities of certain positions. Most versions of scientific pluralism, I will argue, show relativist tendencies. It is both a feature of and a challenge to every pluralist position to accommodate this tendency while remaining a distinct program. Before continuing to argue for this assertion, I will examine candidates for "radical" and "modest" pluralism to demonstrate that these categories are not apt for classifying these positions.

5.3.1.1 Radical vs. modest, a misconception

In this section, I will provide a critique of the radical/modest classification along two lines. First, I will show that the classification is incapable of representing scientific pluralist positions adequately. Second, I will demonstrate that, by following its credentials strictly, it groups positions almost randomly.

I will group the pluralist positions which I introduced in the previous section with respect to the criteria of the classification. I will start with radical pluralism. According to Kellert, Longino, and Waters, there are anti-realist and realist radical pluralists. Antirealist radical pluralists believe that there is no limit to possible accounts of the world. Such positions are constructivist. Realist radical pluralists believe that exterior reality offers unlimited ways of classifying it, of grouping phenomena together. There is, again, no limit for possible accounts (2006, xiii). According to the authors, both versions are not distinguishable from relativism. They entail relativism because they do not maintain the demarcation of science from non-science and set no limits for theory choice.

But one might ask, which positions realize these categories? I will start with anti-realist radical pluralism. Kellert, Longino, and Waters do not provide examples in their discussion. I suspect that they consider proponents of the Sociology of Scientific Knowledge (SSK) anti-realist pluralists. SSK might seem radical to the tastes of some philosophers. SSK is, there can be no doubt, compatible with anti-realism. The symmetry principle of SSK states that truth or falsity of scientific theories is not an explanatory tool within SSK. SSK "would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs” (Bloor, 1976/1984, 7). But the focus of scholars like Collins, Woolgar, or Bloor does not lie on pluralism in particular. Pluralism can be a consequence of the SSK program or a particular case study. But pluralism is not necessarily a consequence.

In Chapter Three, I briefly reviewed Collins’ case study on plant responses to remote stimuli. Collins did not provide this case study to argue for pluralism but to demonstrate an instance of the Experimenters’ Regress. While I made use of the case study to make a point about pluralism, Collins did not argue for a specific attitude concerning a state of plurality. He is, instead, interested in how it is possible to reach closure in times of a scientific dispute. In conclusion, SSK does not fit the radical spectrum of Kellert, Longino, and Waters’s classification.

I will briefly consider my case study. Am I a radical pluralist? In Chapter Two, I examined small RNA inheritance and proposed different pluralist interpretations. In Chapter Four, I employed SSK-proximate methodology in order to investigate plurality within research fields. I examined the role of theory in different contexts. I focused on how research traditions, repertoires, and narratives shape plurality and theoretical commitments. I did not refer to the truth and falsity of approaches to explain why specific theoretical commitments are successful or not. Would I thus qualify as a radical, anti-realist pluralist? My case study is a case in point to highlight the inaptness of the classification. “Radical” anti-realist pluralism, as Kellert, Longino, and Waters define it, describes my methodology. Yet it has no bearing on my pluralist conclusions or prescriptions whatsoever.

Positions that are at least partly anti-realist would be those of Nancy Cartwright or Ian Hacking. Both accept the reality of theoretical entities but reject the realism of universal laws. Could Hacking and Cartwright be the anti-realists Kellert, Longino, and Waters have in mind? Nancy Cartwright holds that the world is divided into patches about which we can have knowledge. One law might hold true for one patch but will fail for another (NC1). But Cartwright does not admit infinitely many different accounts of the world. The laws she considers in her case studies lie in the realm of well-established science. Cartwright is, thus, not the targeted radical pluralist. Neither is Hacking. The styles of reasoning he proposes are styles of *scientific* reasoning that were accepted in specific historical settings. Hacking’s styles of reasoning set constraints for which theories have a true/false value. Styles of reasoning, thus, limit the number of possible theories. In conclusion, neither Cartwright nor Hacking buy into “anti-realist pluralism” as defined by Kellert, Longino, and Waters.

I was unsuccessful in identifying the “anti-realist” pluralists. Moving on, I will turn to realist radical pluralism. Kellert, Longino, and Waters provide Dupré’s *Disorder of Things* as an example. Dupré claims that there are unlimited ways of classifying nature and thus accepts “promiscuous realism” (JD2). But Dupré provides a set of standards to choose between classifications, theories, and explanations (JD3). Dupré’s pluralism is not anarchic. He subscribes to relativism only in the sense that all scientific endeavors are purpose-relative. This commitment will hardly differentiate him from other scientific pluralists. Dupré is a realist pluralist but not particularly “radical.”

Chang and Feyerabend might qualify as radical realist pluralists. Both scholars embrace realism in a similar way. They maintain that several theories make more contact with exterior reality than one theory (PF1, HC1). They, thus, are realists. Chang and Feyerabend differ regarding their takes on theory choice. To Chang, “many things go” (HC3). To Feyerabend, any standard goes (PF3). But do both abandon judgment?

Chang does not propose specific epistemic virtues or standards. A criterion for theory choice is the capacity of a theory to reap the benefits of toleration and interaction (HC1). In Feyerabend’s pluralism, the only limit for theories is human ingenuity. Methodological anarchism suggests that theories need not only come from the realm of well-established science. But Feyerabend acknowledges that standards influence theory choice in every historical setting. At best, standards allow for many alternatives (PF3). In conclusion, both Chang and Feyerabend propose ways for theory choice. Chang refers to Bayesian prior probabilities and the capacity of a theory to reap the benefits of tolerance and interaction. Feyerabend accepts historically contingent standards as guides of theory choice. Both philosophers might not be the radicals we are looking for.

In the last paragraphs, I considered some candidates for radical pluralism. There are two problems with classifying them as “radical.” On the one hand, none of these positions renounces judgment about scientific theories in a way Kellert, Longino, and Waters prescribe. On the other hand, if these positions fall into the range of radical classification, “radical” pluralism would comprise a range of positions that are not in agreement with each other. Some of these positions stress their demarcation from other positions which I located in the same group. The purpose and focus of each of these positions are not captured by classifying them as “radical.” Calling pluralist positions “radical” does not satisfactorily characterize their content.

I will proceed and examine the other end of the scale: modest pluralism. What does modest pluralism mean? Either it requires a monist outlook on plurality. Or it is the claim that there exists an *in principle* translatability or possibility of integration for different theories. The flaw of such positions, as Kellert, Longino, and Waters state, is that they reduce to monism.

One version of modest pluralism, the “plurality now – monism then” version, is not SP. This position involves the claim that plurality is and should only be a temporal condition. The development of new technologies and the augmentation of intellectual abilities will resolve plurality. No card-carrying pluralist would support this claim. The necessary and sufficient criteria which I proposed for SP exclude this commitment as a pluralist position.

Kellert, Longino, and Waters consider Philip Kitcher an example of this version of modest pluralism. To Kitcher, cultivating a plurality of theories is beneficial if a scientific dispute is not settled. A plurality of theories enlarges the probability of finding the right theory (Kitcher, 2011, 194). In such a scenario, all theories but one are wrong. I would consider it quite common to regard “wrong” theories as instrumental for the higher purpose of finding the right one. Drawing from historical examples, nobody would deny that there were such instances. But this is not SP. Such positions fail the criterium of non-monism which I discussed in section 5.1.

The second version of moderate pluralism involves the claim that translatability and integration of pluralist positions are locally possible. The authors of the Pluralist Stance criticize this version because it presupposes that for every unique situation, one – true – integrated pluralist account can be provided. To Kellert, Longino, and Waters, this is monism because such positions favor singular, true accounts of natural phenomena.

The authors associate this position with Mitchell's work on integrative pluralism. Mitchell holds that "However complex and however many contributing causes participated, there is only one causal history that, in fact, has generated the phenomenon to be explained" (2003, 216). Chang also refers to ad hoc integration as one of the benefits of integrative pluralism.

The reviewed positions do not necessarily imply monism. On the one hand, they are not globally monist. Mitchell grants theoretical pluralism on the abstract level (SM2). Chang's integration is only ad hoc. On the other hand, both authors are open to the possibility that integration is not always possible. Chang considers such outcomes from the perspective of the benefits of toleration; Mitchell admits cases in biology where integration fails.

I will consider two other scientific pluralists as candidates for modest pluralism: Galison and Neurath. Galison's key metaphors are "the trading zone" (PG2) and "the postmodern metaphor" (PG1). His point is motivated by the possibility of cooperation, notwithstanding the "fact" of plurality. In TZs, translation of theories is not possible, but integration through a pidgin language is. Galison's TZs do not propose monism. TZs produce local and specific solutions to accommodate plurality. Neurath's position has similar characteristics. Solutions that overcome theoretical plurality need to be possible (ON3). Neurath emphasizes the historical conditioning of such decisions, yet, there is one (historical, local) account to each (historical, local) problem. For the case of Neurath and Galison, integration does not constitute a slippery slope into monism. Integration, instead, maintains theoretical plurality while providing methodological suggestions to address concrete problems.

In conclusion, modest pluralism fails as a classifier in the same ways as radical pluralism does. It is not clear how accounts that aim for integration would entail monism. The positions that would be classified through the label "modest" pluralism seem rather heterogeneous. The authors discussed as possible candidates aim at understanding how collaborations in a pluralist enterprise are possible. Kellert, Longino, and Waters' classification misses the focus of these accounts.

I conclude that "modest" as well as "radical" pluralism fail as classificatory markers: positions these markers classify do not necessarily show the tendencies they suggest. They mask, in addition, the diversity of the positions described. In the next section, I will elaborate on a second problem for Kellert, Longino, and Waters' classification.

5.3.1.2 Relativism and pluralism, a primer

In this section, I will bring forward arguments that relativism is a tendency that characterizes all pluralist positions. Kellert, Longino, and Waters criticize positions for displaying a certain relativist tendency. I will argue that Kellert, Longino, and Waters are mistaken in doing so. This tendency is an inevitable feature rather than a drawback of pluralist positions. Integrating a relativist tendency while remaining a distinct program might be one of the most exciting features of scientific pluralism. In what follows, I will exemplify this claim by demonstrating that Kellert, Longino, and Waters's Pluralist Stance also displays this duality.

I will not argue that Kellert, Longino, and Waters' position entails or equals relativism. But I will show that some relativist tendencies might not be easily overcome. I locate these tendencies with the problem of theory choice. Kellert, Longino, and Waters emphasize that it is essential to decide which theories are allowed within their pluralist framework. Kellert, Longino, and Waters claim that the Pluralist Stance is not relativist because they do not renounce judgment about such decisions.

From what was discussed above, it follows that the Pluralist Stance requires clear-cut guidelines for choosing between theories. "There are instances of poor research" (2006, xxvii) in every branch of science, which should not be accepted in a pluralist framework. One aspect of poor research is the endeavor to demolish plurality. There are, of course, other examples, which Kellert, Longino, and Waters do not explicitly mention.

A proponent of the Pluralist Stance thus needs to engage in several levels of decision making. The first level is that of whether a phenomenon requires a plurality of approaches or not. The second level is that of whether a specific approach is scientific or not. The third level is to decide whether a specific approach is "good" or "bad" science. Kellert, Longino, and Waters do not introduce guidelines for making these decisions. Elsewhere, Longino introduces a set of values to guide decision-making on these levels (PS3/HL1).

Values, therefore, are an integral aspect of the agenda of the Pluralist Stance. But there is a caveat. If epistemological anarchism does not govern theory choice, then there is at least a certain anarchism on the level of value choice – as elucidated by Feyerabend (PF3). Longino seems to agree: “different sets of heuristics (i.e., sets of values) will satisfy the different cognitive goals.” The particular choice of accepted theories can thus only be justified from within a set of standards. There is no neutral point from which to judge which set of theories is the best one. The dependence of judgments on particular standards is, however, an integral ingredient of most relativist positions, as we shall see in the following chapter. The Pluralist Stance cannot, thus, avoid relativism. But this should not be a problem for pluralist positions. We need to accept the relativist tendency within pluralist positions. This does not mean, however, that pluralism is relativism because pluralism has a different agenda. In Chapter Six, I will analyze the relationship between pluralism and relativism in more detail.

In conclusion, I have shown that Kellert, Longino, and Waters' classification fails for a specific reason. They consider “relativism” and “monism” two undesirable “extremes” of pluralist positions. I demonstrated that this classification fails because a “relativist tendency” is inevitable for pluralist positions. “Monism” as a classifier on the other end of the spectrum is, of course, also problematic. Pluralism is the rejection of monism. Kellert, Longino, and Waters as well list the rejection of monism as an essential criterium of pluralist positions. Both ends of the scale are therefore highly problematic. A certain “relativist touch” is common to all pluralist positions. “Monism” is a tendency no pluralist position can be accused of, based on the definition of scientific pluralism provided. The classification, thus, fails. In the section to follow, I will suggest alternative approaches towards classification.

5.3.2 Classification revisited

Having criticized Kellert, Longino, and Waters' classification of pluralist positions, I will propose alternatives in this section. I aim to propose classifications that are interesting and actively help structure pluralist positions. I will suggest four ways of classifying. These classifications should not be regarded as definite programs. Instead, they reflect my interests and try to look at pluralist positions from different angles.

One of the four approaches classifies scientific pluralist positions by their relatedness to one of two "forefather" position. I will classify pluralist positions referring to either a "Neurathian" or a "Feyerabendian" theme. The second approach analyzes positions based on how they formulate the relation of realism and scientific pluralism. The third approach classifies positions regarding how they solve the problem of theory choice. The fourth and last classification revisits a problem extensively considered in Chapter Three: are theories integratable or non-integratable?

I take it as both expected and granted that under different classificatory scales, positions will group differently. This is indicative of the different aspects of pluralist positions. These classifications, in addition, highlight that from different points of view, certain positions might have more or less in common.

5.3.2.1 The Neurathian – Feyerabendian dimension

First, I will propose a classification that demarcates "Neurathian" and "Feyerabendian" SP. Both philosophers are early defenders of SP. Most scientific pluralists accept Neurath as a "founding figure." Feyerabend is much more contested. In a sense, Feyerabend's specter haunts pluralism. Whereas several authors, most prominently Cartwright, highlight Neurath's impact, demarcation from a Feyerabendian "anything-goes" approach appears to be a frequent feature of pluralist positions. Although the willingness to accept an intellectual connection to a Feyerabendian theme seems rather low, I will argue that several pluralist positions resonate with aspects of Feyerabend's work.

First, I will define the terms "Neurathian" and "Feyerabendian" pluralism. This classification focuses on particular "emphases." The "Neurathian" emphasis lies on the fact that – despite the disunity of science – a local solution can be produced to solve a scientific or a social problem (ON3). Neurathian pluralism tries to answer how scientists can work together even though theories are not grounded on the same foundations.

The emphasis of "Feyerabendian" pluralism is different. Counterinduction is most central to this kind of pluralism (PF2). Counterinduction means the active introduction of alternative approaches into the realm of science. Counterinduction serves testing established theories and achieving maximal contact with reality. The emphasis of Feyerabendian pluralism lies not so much on integration or on how to productively engage with other theories. In "Feyerabendian" pluralism, theories that might be radically different coexist.

The proposed classification operates by differentiating the "initial conditions" and the agenda of pluralist positions. The initial condition for a "Neurathian" pluralist are situations in which she observes a plurality of theories with no shared foundations. As a consequence, she explores how different theories interact. The Feyerabendian pluralist has a different agenda. She aims at creating more plurality. More theories should be introduced into the realm of science. In other words, the Neurathian approach tries to accommodate the consequences of plurality. Plurality, on the contrary, is the consequence of the Feyerabendian approach.

I will start with philosophers that qualify as "Neurathian" pluralist positions. Neurath is, of course, one candidate. Neurath claims that although knowledge might have different foundations, there is no incommensurability on the level of scientific action. Each local setup can secure ties through "universal slang" to enforce joint action. The unity of science is only possible in praxis (ON3). Similarly, I classify Galison as a "Neurathian" pluralist. Galison's "trading zones" are examples of how the unity of science is possible in practice is. TZs serve as the clay between the different "bricks" of science. In analogy to Neurath's "universal slang," Galison introduces the notion of a pidgin language (PG2).

Cartwright is another "Neurathian" pluralist. She proposes the "dappled world," in which there is no single foundation of knowledge. Knowledge has a "patchwork structure." Adopting the language of her metaphor, scientific action results when different areas of knowledge are "patched up." This process is contingent and only justified in a local setting (NC2). Mitchell's "integrative pluralism" is another instantiation of "Neurathian" pluralism. Theories are incommensurable on the abstract level. But in local, concrete settings, phenomena have one explanation, one single causal story (SM2 + SM3).

Having surveyed "Neurathian" pluralists, I will consider candidates for "Feyerabendian" pluralism. Feyerabend is an obvious candidate. Feyerabend's program is counterinduction. Counterinduction prescribes introducing alternative theories to prevent well-established theories from becoming myths (PF2). Chang also fits this scheme. His "active pluralism" aims at introducing as many systems of practice as possible (HC1 + HC2). I also group Dupré's Disorder of Things in the Feyerabendian program because he acknowledges different, incommensurable classificatory schemes. All of these schemes provide true accounts of the world (JD2). Dupré, however, refrains from introducing more theories. The last candidate for Feyerabendian pluralism is the Pluralist Stance. The stance accepts that for every single phenomenon, there might be various theories or explanations which are *not* integratable (PS1).

Suppes and Hacking do not fit the conceptualizations. They neither explore how different theories work together nor do they urge for introducing more theories. But both consider "plurality" as a starting condition of their investigations. They are, as a result, closer to "Neurathian pluralism" but lack its agenda.

Figure 22 depicts the proposed classifications graphically. "Feyerabendian" and "Neurathian" pluralists are placed on both ends of the scale and ranked regarding the extent of their commitment to "Feyerabendian" or "Neurathian" pluralism. It is important to note that the endpoints do not represent a dichotomy but rather a difference in emphasis. Each endpoint is the "extreme" of the respective emphasis.

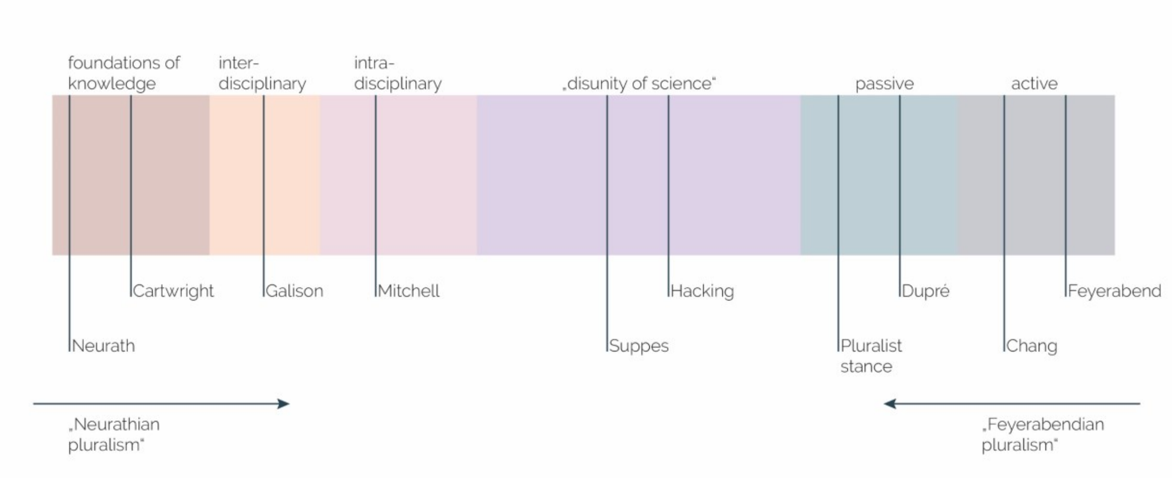

Figure 22 Pluralist positions grouped by their "Neurathian" or "Feyerabendian" character.

The "Neurathian" pluralists are grouped regarding the extent of metaphysical commitment and its import on how different approaches can interact. Neurath and Cartwright inhabit the endpoint of the scale. Both believe in metaphysical disunity and propose the world to be a disunified "library" or "patchwork" (ON1, NC1). Integration is theoretically impossible but a necessity in practice. Galison's position bears a little less on metaphysics. Disunity is, in Galison's account, primarily an empirical fact. The existence of trading zones that enable overcoming disunity in practice is also an empirical fact (PG2). Galison maintains that securing constraints of plurality is essential to securing the possibility of trading zones (PG3). The disunity of science, thus, is limited, to a certain extent. Mitchell's commitment to integration places her further down the scale. Mitchell believes that the integration of theories in the concrete is always possible (SM2, SM3). Her pluralism concerns accounts within the same disciplines, thus "universal slangs" and "trading zones" are not required. As a result, employing the measure of "Neurathian" tendencies also differentiates positions regarding their targets. "Extreme Neurathian" pluralism concerns the totality of science and argues for anti-foundationalism. Moving down the scale, targets of pluralism shift from anti-foundationalism to inter-disciplinary plurality to intra-disciplinary plurality.

Having discussed the "Neurathian" endpoint of the scale, I will examine the "Feyerabendian" pluralists. I assess the extent of their "Feyerabendian" tendency by considering how "active" their pluralist position is. The closer positions move to the endpoint, the more they require the active introduction of theories. Feyerabend and Chang inhabit the endpoint of the scale (HC2, PF2). The Pluralist Stance is less active. The Pluralist Stance does not maintain that more theories should be introduced for every phenomenon. Instead, they aim at examining whether a plurality of theories is required (PS1). Dupré is the most "passive" of the Feyerabendian pluralists. Dupré's position is not normative because he does not require the introduction of more approaches (JD3).

Suppes and Hacking are located in the middle of the scale. Both acknowledge and cherish the plurality of science. But they do not share the commitments to either augment plurality or to overcome plurality in practical settings.

5.3.2.2 Pluralism and realism – friends or foes?

In this section, I will consider a second way of classifying pluralist positions. I will propose to stratify pluralist positions by asking whether and in which way they accept realism. This mainly concerns the question whether there is a plurality of approaches and how these approaches make contact with reality. How pluralists commit to realism is often questioned. As a result, it is quite common for card-carrying scientific pluralists to define where they place themselves in the realism debate. Defenders of scientific pluralism often introduce their versions of realism, such as "promiscuous realism" (Dupré), "tempered realism" (PS), or "entity realism" (Hacking).

Before proceeding with the classification, I will provide a working definition of scientific realism. The *Stanford Encyclopedia of Philosophy* defines the core idea that motivates scientific realism as a "positive attitude toward the outputs of scientific investigation, regarding both observable and unobservable aspects of the world.[43]" Scientific realism runs together with a notion of success or achievement of scientific theories. Theories achieve truth or approximate truth. Our theoretical terms refer successfully to things in the world. In other words, our scientific theories yield knowledge of the world. I will work with Bas van Fraassen's definition of scientific realism in the remainder of this chapter.

Realism: "*Science aims to give us, in its theories, a literally true story of what the world is like; and acceptance of a scientific theory involves the belief that it is true.* This is the correct statement of scientific realism" (Van Fraassen, 1980, 8).

It is important to note that my classification will not hinge on the realism/antirealism dichotomy. Although some scientific pluralists might be considered more anti-realist than others, most card-carrying pluralists propose that approaches within their pluralist framework make contact with reality. Neurath is the only scientific pluralist one could describe as an anti-realist. This definition, however, does not seem to fit Neurath's position. Neurath considered proposition about *Wirklichkeit* – reality – uninteresting (ON3). As a result, Neurath's antirealism might not have a particular bearing on his pluralist position. In conclusion, when considering realism and pluralism, transcending the "realism-antirealism" dichotomy is required.

I will classify scientific pluralist positions concerning whether they claim that 1) different theories make contact with different realities, 2) theories account for different (and incommensurable) aspects of reality, or 3) theories provide different ways towards reality.

[43] https://plato.stanford.edu/entries/scientific-realism/

It is important to note that the notion that a plurality of theories augments the contact to exterior reality is a fairly common notion amongst scientific pluralists. The late Feyerabend (PF4) qualifies for option 1). PF4 even transgresses this classification in that he proposes the existence of several "manifest worlds." One "unified" reality that lies outside of them is not attainable. Other scientific pluralists qualify for either option 2) or option 3).

I will start with option 2). Two obvious candidates are Cartwright and Hacking. Cartwright believes in the disunity and dappledness of the world (NC2). Laws only hold in local, ceteris paribus conditions. In these restricted conditions, they capture reality. Two local laws belong to distinct patches of the world and point towards different aspects of reality. If one combines several local theories to formulate one universal law, contact to reality will be lost. Hacking's version of realism is another case in point. The metaphor of the Borgesian library makes his position explicitly clear: "each book of which is as brief as possible, yet is inconsistent with every other." Each book provides an accessible bit of nature (IH2).

Dupré's "promiscuous realism" also qualifies as option 2). Different classificatory systems and theories classify a single reality differently. Each approach captures a set of objects; no theory accounts for the same set of real objects (JD2). Each theory or classificatory system provides one aspect of reality, which is not complete but unique in its composition.

The Pluralist Stance is also an instantiation of option 2). Different accounts of one phenomenon have different explanatory agendas. They describe different aspects of the phenomenon. The Pluralist Stance is agnostic concerning how different theories make contact to reality or whether they do so at all (PS2). Approaches should not be integrated. Different theories approach the same reality, but each theory captures a different aspect of it.

While the "late Feyerabend" can be classified as a defender of option 1), the "early Feyerabend" (PF1-3) defends a version of option 2). The program of counterinduction aims at introducing various alternative theories which challenge ruling dogmata. Proposing alternatives is necessary for accessing new empirical facts. Different theories, thus, provide access to different aspects of reality.

Neurath is a particular case. Neurath claims that science cannot be tested against the "real world" (ON2). This sentiment recalls the late Feyerabend (PF4). Yet, Neurath does not propose "manifest worlds," as does Feyerabend. To Neurath, "reality consists of an infinitely manifold stream of events" but not of more than one world. Foundations, theories, and auxiliary hypotheses flow. Theories approach different aspects of disunified science. How science and reality relate is, to Neurath, no concern.

I will finally discuss pluralist positions that subscribe to option 3). One candidate is Mitchell. There is a plurality of idealized theories on the abstract level. In particular, however, different theories will be integrated, providing one single account. When accounting for a single (real) phenomenon, theoretical pluralism dissolves into an account of one reality (SM3).

Another candidate is Chang. Chang admits the possibility of interaction. One example is the integration of phlogiston and oxygen. Both concepts, phlogiston and oxygen, were combined in explanations to understand one phenomenon (combustion). Through this integration, contact with one reality was maximized.

Patrick Suppes also fits option 3). Science is an endeavor that solves specific problems. Our best theories are "local and designed to meet a given set of problems." (Suppes, 1978, 14f). There is an "outer reality" that might very well be complete and certain. Each problem constitutes one different junk of reality. Different theories solve different problems and therefore represent different paths towards reality. No theory is certain or complete (PS2). Galison's "empirically realist" outlook on trading zones is similar to Suppes' approach. In trading zones, specific, real problems get solved. A trading zone produces a specific way to address a problem.

Figure 23 depicts this classification on a scale. The classification considers first and foremost whether scientific pluralists subscribe to option b) or option c) concerning their particular takes on realism. The late Feyerabend is the only candidate subscribing to option a); he is an outlier at the end of the scale. Cartwright, Hacking, Dupré, and the early Feyerabend share the notion that different theories capture different aspects of reality. The Pluralist Stance and Neurath also accept this notion but refrain from defining how exactly different theories make contact with exterior reality. Neurath and the Pluralist Stance are agnostic regarding the nature of the world. They nevertheless subscribe to option b) because they insist that different, disunified approaches are grounded in different foundations.

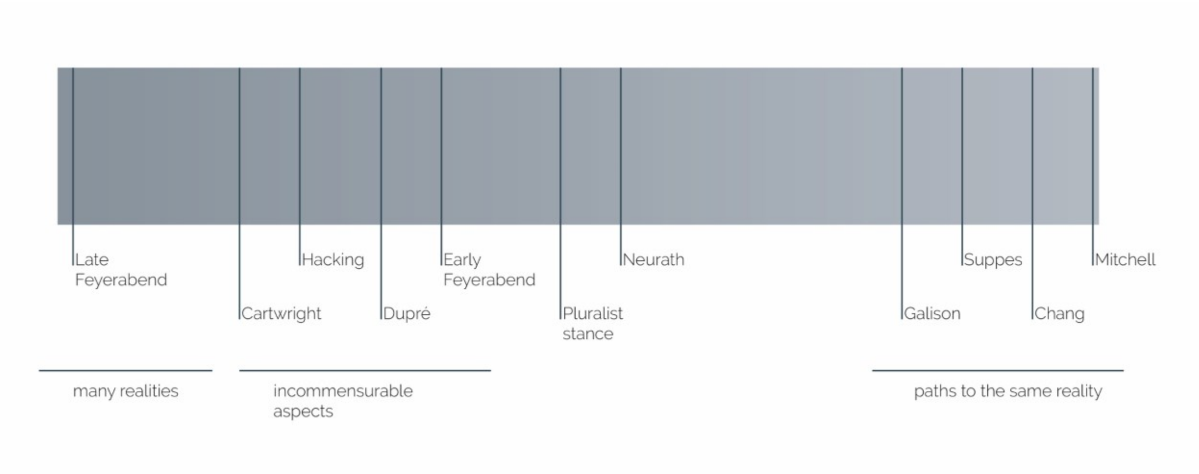

Figure 23 Pluralist positions organized regarding their commitment towards realism.

The other end of the scale designates those pluralists subscribing to option c). Mitchell, for example, proposes that a plurality of theories can be integrated to account for one specific phenomenon. Chang considers the integration of several systems of practice to describe one phenomenon as one desirable outcome of pluralism. Suppes and Galison also consider several theories as different paths to the same reality. Their emphasis, however, lies not so much on whether pluralism facilitates access to reality. They are rather interested in access to solving real problems. To be precise, Suppes claims that different theories are in touch with the same reality, but certainty and completeness are unattainable. For Galison, the formation of trading zones solves problems.

5.3.2.3 The problem of theory choice – and its solutions

The third way of classifying pluralist positions concerns the question of theory choice. There are three possible solutions regarding how to choose between theories within a scientific pluralist framework. The problem of theory choice is tightly linked to the problem of demarcation. Similar strategies might be involved in deciding which theory is considered “scientific,” and which is considered a “good” theory. To classify pluralist positions, I will focus on the problem of theory choice. I will first define the three ways of solving the problem of theory choice and then group the discussed scientific pluralists accordingly.

The first solution is the "anything" or "many things" go approach. In this framework, no strict criteria for choosing between theories are formulated. Both the "anything" and the "many things" go approach reject universal solutions to the problem of theory choice. The second solution is to propose epistemic standards or virtues to identify good and bad theories. The third solution relies on specific historical, local constraints that enable theory choice and is a task that is solved by practicing scientists. Proponents subscribing to this solution emphasize the term "scientific" in scientific pluralism. As scientific pluralism only concerns positions within "well-established" science, theory choice is solved in the particular by only admitting "scientific" theories.

I will review scientific pluralists regarding their takes on theory choice commencing with the first proposed solution, "anything" or "many things" go. Chang subscribes to this solution, proposing two different ways to enable "many things go." Bayesian prior probabilities instruct whether to admit a theory or not (HC3). Whether a theory can reap the benefits of toleration and interaction is another requirement for a theory to be admitted (HC1). A non-zero Bayesian prior probability is quite a permissive criterion. Whether a theory will be admitted is thus not so much decided by specific properties of the theory itself. Instead, how a particular theory interacts with other theories is decisive for its admission. I will develop this discussion further in Chapter Six.

Feyerabend's approach lies in the vicinity of Chang's. Contrary to Chang, Feyerabend emphasizes historically contingent standards. Although he rejects the idea that standards are universal, they are useful. Standards are required but should not be considered universal (PF3). Standards, however, should be permissive in the sense that they do not keep out theories that challenge dogmata (PF2).

The second way of solving the problem of theory choice is the implementation of epistemic standards or virtues. Longino describes such standards as "sets of heuristics," which help to choose between theories (PS3/HL1). These heuristics are purpose sensitive. Dupré also proposes epistemic virtues to decide between good and bad theories (JD3).

Neurath's and Cartwright's pluralism are instantiations of the third way of theory choice, which I described as somewhat "descriptive." Both do not spill much ink on theory choice. Theory choice works efficiently in the particular and is only justified in local settings (ON3, NC2). Theory choice is the product of local decision making. Theory choice is thus a task for scientists but is not a specific task for scientific pluralism. Mitchell, Galison, Hacking, and Suppes also subscribe to this position.

Plotting scientific pluralists on a scale regarding their commitment to theory choice also provides insight into the normativity of their positions (Fig. 24). As discussed in section 5.1, scientific pluralism is necessarily normative. There are, however, positions that emphasize descriptive positions that make the normativity of pluralism central to their position. This differentiation resonates with the surveyed commitments regarding theory choice. The first and second solutions regarding theory choice are the most normative ones. The respective means of theory choice create an incentive to introduce more theories. The "anything" or "many things" go approach creates very flexible entry criteria for alternative approaches. Introducing epistemic virtues or standards creates regulations within the pluralist enterprise but still admits several approaches if they comply with a particular set of virtues or standards.

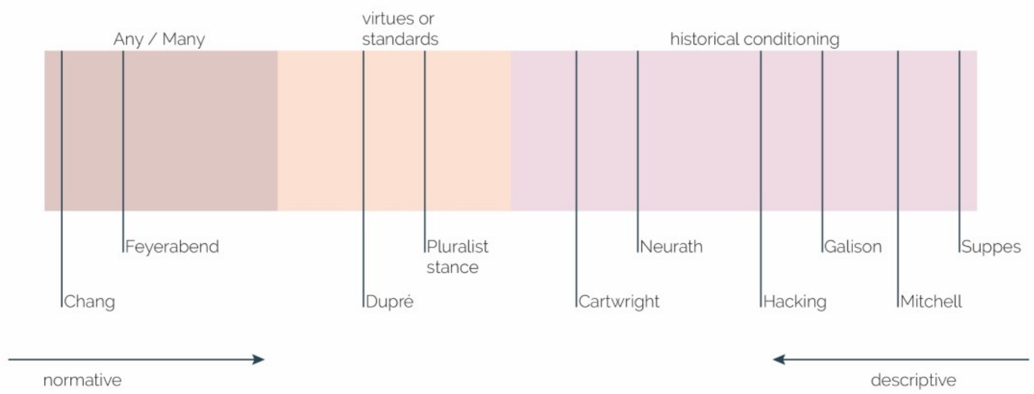

Figure 24 Pluralist positions organized regarding how they solve the problem of theory choice.

The third solution to theory choice is characteristic of positions that emphasize the descriptive aspects of pluralism. Pluralist positions emphasizing the local conditioning of theory choice or positions that limit their pluralism to well-defined borders of science belong to these rather descriptive pluralist positions. They emphasize the benefits of plurality observed but do not require the introduction of more approaches. They, thus, do not require tools, such as standards, to introduce more approaches.

5.3.2.4 Integration or non-integration?

The fourth and last way of differentiating pluralist positions is to consider how pluralists propose that different theories relate to each other. Are they integrable or are they not integrable? Whether card-carrying scientific pluralists propose approaches to be integrable or not can have several reasons. They could be epistemically or metaphysically motivated. In Chapter Three, I investigated the arguments scientific pluralists provide for the integrability or non-integrability of approaches. I argued against the position that specific "properties" of phenomena decide whether they require "integrative pluralism" or not. Although I oppose this differentiation, it is nevertheless a differentiation that is upheld by several card-carrying scientific pluralists. I will, thus, classify pluralist positions regarding whether they propose an integrative or a non-integrative version of SP.

I will first review philosophers that cherish integration or at least deem it possible. One candidate is Mitchell, whose position I have reviewed extensively in Chapter Three. Abstract theories can be integrated to provide an account of one specific phenomenon. Chang also emphasis ad hoc integration. If integration is not feasible, other modes of interaction are also possible. Examples are co-optation and competition. Chang considers, however, scenarios in which interaction is not possible, and only "toleration" can be attained. In conclusion, Chang does not require integration but favors it for its epistemological benefits.

The next candidate is Galison. His work focuses on TZs and how to overcome disunity in practice. TZs do not require integration of approaches but the formation of transient, hybrid clusters. Cartwright's and Neurath's agenda is closely related to Galison's. Both emphasize the disunified character of the sciences but believe in the possibility of solving problems by combining different patches of science.

Having examined scientific pluralists who emphasize "integration," I will survey those positions that discourage integration. The first two candidates are Feyerabend and Hacking. Both emphasize the heterogeneity within the plurality of approaches. Feyerabend proposes that alternative theories should be introduced. Because these approaches are different, they make different facts available. Introducing alternative theories enables testing other theories against new empirical facts. They test other theories against new facts. Integration is discouraged, as this would impede counterinduction. Hacking's pluralism concerns disparate styles of reasoning. Each style defines which sentences have a truth value in their realm. A particular sentence might have a truth value in one style but no truth value in another style. There is, thus, limited potential for integration.

Dupré also favors non-integration. He emphasizes the purpose relativity of each classificatory scheme. This purpose sensitivity makes integration obsolete. There is no reason to integrate those different sets of theories. The Pluralist Stance also discourages integration. As discussed above, proponents of the Pluralist Stance believe that integration of approaches entails monism. They regard it as epistemologically beneficial to keep accounts separate. In his writing, Suppes does not consider the question of integration. Yet, he applauds the plurality of approaches, making an analogy to liberal democracy. Drawing from this analogy, integration might not appear to be a desirable goal to him.

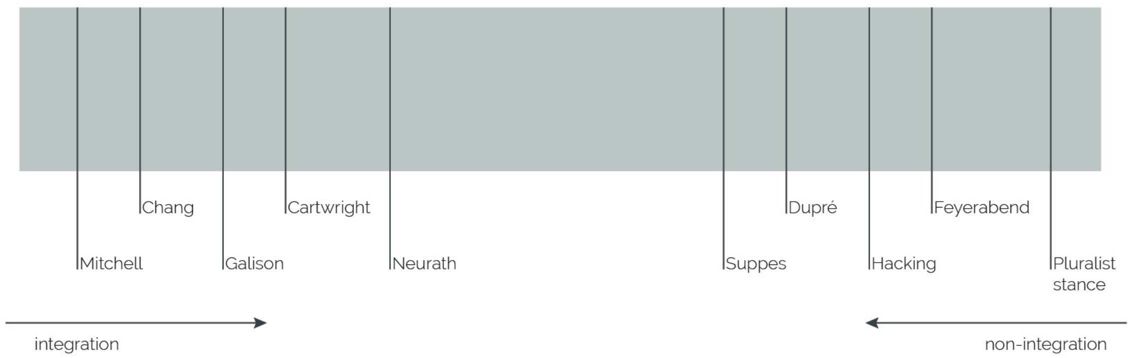

Figure 25 Pluralist positions organized regarding whether they regard different approaches as integratable, or non-integratable.

The question of whether approaches should be integrated or not divides scientific pluralists into two camps (Fig. 25). Integration is central to Mitchell's position. Integration is also very prominent in Chang's position for its epistemological benefits. Galison, Cartwright, and Neurath have a different take on the integrability of approaches. Although they deny that integration is possible in principle, they believe in the integration of different strands of research in local and contingent settings. The other end of the scale is inhabited by those pluralists who discourage integration. The Pluralist Stance is most explicit in rejecting integration as a goal of pluralism. In Feyerabend's and Hacking's accounts, integration is not explicitly discouraged but it seems not feasible. Dupré, too, emphasizes the purpose sensitivity of approaches, integration is not a goal that fits his agenda. Suppes, finally, does not consider the possibility of integration.

In this section, I have proposed four classifications that are alternatives to Kellert, Longino, and Waters' classification of pluralist positions. I regard these classifications as successful because they carve out peculiarities of pluralist positions, while at the same time classifying them. Depending on the respective classification employed, different pluralist positions group together. I take this to be a further refutation of the "radical-modest" differentiation. Pluralist positions cannot be adequately defined by analyzing them for their relativist or monist tendencies. With respect to different aspects, different card-carrying scientific pluralist group closer or less close.

In my critique of Kellert, Longino, and Waters' position, I showed that they conceptualize the relation of pluralism and relativism wrongly. I argued that a certain relativist tendency is pertinent to all scientific pluralist position, including their own. In the chapter to follow, I will examine this "relativist tendency" and discuss in more detail the relation of these two positions.

6 Bridging epistemic relativism and scientific pluralism

In this chapter, I will provide an examination of the relation of epistemic relativism and scientific pluralism. This examination is related to an issue that I discussed in Chapter Three: normativity. Several scientific pluralists aim at demarcating their position from relativism because they fear that their position will lose its normative content once they subscribe to relativism. In this chapter, I aim to disprove the notion that pluralist programs that accept relativism lose their normativity. Before examining the pluralists' arguments in detail, I will reconstruct the pluralists' concerns regarding normativity and relativism.

Normativity is a central ingredient in most pluralist positions. Chang and Longino are prominent examples. Both require that science should admit more plurality – more approaches should be introduced. They regard normativity as central to their claims. At the same time, both Chang and Longino are quite concerned about demarcating their position from relativism. They fear that they would have to give up on their normativity if they bought into relativism. Anti-relativist philosopher Paul Boghossian coins this problem "the loss of normative content" (Boghossian, 2015, 372). Boghossian claims that by adding (e.g., historical) context into the content of an assertion, the assertion loses its normative content and transforms into an entirely descriptive statement.

Boghossian provides an example to illustrate his claim. The assertion "Bellarmine was justified in believing that the earth goes around the sun" has normative content, Boghossian submits. The assertion "Bellarmine was justified in believing that the earth goes around the sun, considering his access to sources, his interpretation of sources, and the norms he followed when deciding which issues are to be decided by scripture and which by research" has descriptive content only. Contextualizing Bellarmine's assertion removes the normative content of the utterance.

Boghossian's example also bears on Chang's case study on the Chemical Revolution. The assertion "Joseph Priestly was justified in believing in phlogiston" has normative content. Following Boghossian's scheme, the assertion "Joseph Priestly was justified in believing in phlogiston, considering his background hypothesis, his expertise in chemistry and other factors that supported this conclusion" has descriptive content only. If this analysis is true, it would also be problematic for Chang. It is essential for Chang for his analysis of the Chemical Revolution to provide a normative conclusion.

It is, however, not clear whether a contextualization erases the normative content of a statement. Asserting that someone is justified, given a particular context, is normative. Being justified, I assert, is inherently good. Relativizing the assertion to a context does not extinguish normativity. There are no judgments that float freely in space and time, given the fact that the ones making the judgment are also situated. Claiming that Bellarmine or Priestley were justified entails that Bellarmine or Priestley took the right actions, relative to their context. Being justified and therefore taking the right actions is highly desirable, also and especially if relativized to a particular context. These considerations might, however, not consolidate the scientific pluralist who worries about losing normativity. In this chapter, I will provide an analysis of epistemic relativism and scientific pluralism that centers around normativity and epistemic values.

6.1 Definitions of pluralism and relativism and their relation

Questions regarding the relation of scientific pluralism (SP) and epistemic relativism (ER) are a common theme amongst advocates and opponents of SP. Card-carrying pluralists are often concerned whether SP entails ER. The relation of SP and ER, however, is rarely investigated in more detail. In this section, I will provide three analytical and conceptual strategies of investigating the relation of pluralism and relativism. First, I will review the characterizations of epistemic relativism (ER) and analyze whether ER entails scientific pluralism (SP). Second, I will examine the definition of SP that I provided in Chapter Five and scrutinize whether SP entails ER. Third, I will explore a different approach. I will draw from an idea introduced by Bas van Fraassen and developed further by Martin Kusch: I will test whether ER and SP qualify as stances. I will investigate whether characterizing ER and SP as stances provides new insights into their relationship. I will conclude by discussing the results of these three approaches.

6.1.1 Relativism does not entail pluralism (by definition)

Scientific pluralism is often alleged to be in close vicinity or even lead to versions of ER. It is, however, often not clear what critics mean when they say "SP entails ER." It will, therefore, be of extreme importance to carve out a definition of relativism.

One well-known critic of ER is Paul Boghossian. In his influential critique of relativism, *Fear of Knowledge*, Boghossian lists three ingredients of epistemic relativism: (A) epistemic non-absolutism, (B) epistemic relationism, and (C) epistemic pluralism. Epistemic non-absolutism is the position that no belief is absolutely justified. Epistemic pluralism claims that there are many "fundamentally different genuinely alternative epistemic systems, but no facts by virtue of which one of these systems is more correct than any of the others." (2006, 73). Epistemic relationism is the position that beliefs are justified depending on a specific epistemic system (2006, 72). Relativism, according to Boghossian, holds that there are "many equally valid ways of knowing the world" and science is only one of them (2006, 2).

There are several more refined ways of approaching what an epistemic relativist position entails. For instance, one could catalog the positions it embraces ("positive classification") or list the stances it rejects ("negative classification"). Both approaches do not provide a *definition* of relativism. This should not come as a surprise. There is hardly any consensus about the content of such a definition. Offering characterizations might be less controversial. Definitions require necessary and sufficient criteria. This is not true for characterizations. Characterizations provide more flexibility in two different ways: Characteristics might be interpreted in different ways. A relativist position does not need to include all characteristics. A characterization provides criteria that are usually necessary and often sufficient. Exceptions are, however, possible. I will provide an example of each possibility. Philosopher and sociologist of science Martin Kusch provides a "standard model of relativism" which surveys eight criteria collected from definitions given by friends and foes of relativism.

1) Dependence: the epistemic status of a belief is relative to a) an epistemic system, or b) a bundle of precedents, judged as virtuous;

2) Pluralism: there are, were or might be several of such systems or bundles;

3) Non-Absolutism: none of these systems or bundles are absolutely warranted;

4) Exclusiveness: some of these systems or bundles can be exclusive (that is, incompatible), either because a) they give different yes/no answers to the same questions (questions about the epistemic status of a belief) or because b) they involve incommensurable forms of action;

5) Symmetry: systems or bundles are symmetrical because either a) they are only based on local reasons for credibility or b) there is no neutral way of ranking them, or c) they are all equally good, or d) the question of ranking does not arise for the proponents of each bundle;

6) Conversion: switching from one system to the other is not possible nor plausible by rational comparison;

7) Semantic Relativity: the statement "A subject S is epistemically justified believing p" expresses a proposition of either one of the two forms: a) as per the epistemic system or the epistemic bundle, I (the speaker) am based on, is S epistemically justified to believe p (semantic contextualism) or b) S is epistemically justified to believe p. This proposition is relatively false or true, depending on the contexts of assessment. (semantic relativism);

8) Metaphysical implications: a) factual understanding: to be epistemically justified is a real relation between convictions and other factors. Epistemic relativism is the thesis that epistemic system or bundles are a central component within this relation; b) non-factual understanding: epistemic relativism is no metaphysical but a language-philosophical thesis (Kusch, 2019a, 338f).[44]

[44] Philosopher Maria Baghramian defines relativism about science by cataloging ten stances relativism rejects (Baghramian, 2004, 140): (a) scientific Realism – scientific theories describe the real world and there is one real description of it; (b) the unity of science; (c) the universality of science; (d) scientific knowledge is cumulative; (e) the theory-observation distinction; (f) foundationalism – "observations and experiments are the foundations and justification of scientific theories"; (g) scientific Method – there is one correct scientific method; (h) meaning invariance – concepts and terms have stable meanings; (i) fact/value and objective/subjective distinction; (j) context independence (2004, 140). Several of these tenets are, however, problematic. The relativist need not reject several of the listed criteria, such as b), f), and g). Some scientific

A relativist position need not accept all listed criteria. The emphasis on individual criteria might be specific for an individual position. Given this flexibility, there are many different possible versions of relativism.

Besides the number of possible versions of relativism, there is also a scale concerning how controversial the respective position is. On one end of an imagined scale for epistemic relativist positions, we find the mildly relativist position that the historical/social context influences theory choice and scientific decision making. I believe that such a position is rather uncontroversial. Historical conditioning of scientific action and decision making is hard to deny. The claim that scientific standards have developed historically relates to the question of access to absolutes. The notion that we do not have access to absolutes is not uncommon in the philosophy of science. Such arguments can also inform a "mild" and relatively uncontroversial form of relativism. It is, however, not uncontroversial altogether. Boghossian, for example, believes that we have access to *some* absolutes.

On the other end of this scale, one could locate a position that claims that all epistemic systems are equally good. But no card-carrying relativist supports this "silly form" of relativism ("equal validity"). Philosophers who aim at demarcating their position from relativism often settle the dispute with "equal validity." "Equal validity," most of the time, is a strawman.

Skepticism should also not be confused with the "silly form" (Ashton, forthcoming) of relativism: In so far that it is denying knowledge to everyone, skepticism involves the claim that all systems are equally good or equally bad and that it is impossible (or there is no justificatory basis) to judge between them. Although the skeptic denies knowledge outright, she admits that some believes might still be more justified than others, but will never constitute knowledge. She, therefore, also does not "renounce judgment."

It is important to note that the "equal validity" thesis faces severe problems from a relativist point of view. Where would the perspective come from that allows for the judgment that all

realists would, on the other hand, not necessarily agree with all the criteria. The scientific realist would, for example, also reject h), i), and j).

systems are equally valid? Acknowledging such a meta-perspective would point towards absolutism. Because relativist positions reject absolutism, it is impossible for a relativist to support "equal validity."

One more remark is in order. There is another aspect of Baghramian's characterization of relativism that I disagree with. I do not take the rejection of foundationalism to be a necessary criterion for relativism. Most relativists acknowledge that "observations and experiments are the foundations and justification of scientific theories" if taken as a descriptive point about how science works. The relativist about science, however, might argue based on groundlessness that there is no non-circular, exterior justification for employing such methodology as "the" investigative strategy. This does not mean that she denies the importance of observation or experiment. But she is open to the possibility of "plurality" of investigative strategies.

In the remainder of this chapter, I will adopt Kusch's definition of ER. I will comment on three issues, that of epistemic bundles, pluralism, and symmetry. I will start with epistemic bundles. Epistemic bundles (EB) are not to be confused with epistemic systems (ES). ES are collections of epistemic principles. An EB or a "bundle of precedents" is a somewhat looser concept. Within a bundle of precedents, one assesses the epistemic status of a belief relative to a set of prior cases. Instances of Kuhnian paradigms are examples of bundles of precedents. Within the realm of a paradigm, certain practices are judged as virtuous, while others are criticized. Comparing the "European" and the "Anglo-American" judicial system provides a helpful analogy. European judicial systems depend on specific laws and individual cases are assessed with regards to these laws. The Anglo-American judicial system rests on laws as well but also emphasizes precedents. A human action is judged as law-abiding relative to how the same behavior was judged in prior cases. An EB, therefore, resembles the Anglo-American judicial system. In the following, I will only use the term epistemic system because the differences between EB and ES are not essential for my discussion. When using the term ES, I take bundles of precedents to be included.

My next comment concerns the question of whether symmetry entails that all positions are equally valid. The notion of "equal validity" is part of the "silly view" of ER. Symmetry does not entail this view. For a relativist, it is impossible to entertain this belief consistently.

Equal validity necessitates a privileged god's eye perspective to judge that all epistemic systems are equally valid. But "dependence" prohibits this god's eye perspective. The equal validity thesis is, therefore, no characteristic of relativism as it violates "dependence." It is important to note that no card-carrying relativist maintains equal validity, and thus, the criterion of symmetry is often misunderstood. Consequently, a false notion of symmetry is central in arguments that claim that relativism "renounces judgment." In the course of this chapter, I will demonstrate that several card-carrying pluralists rely on this false notion of symmetry in their arguments against ER.

I will provide a concrete example to explain the symmetry principle. A relativist might conclude that an alternative ES is consistent and persuasive for actors within this ES. Yet the relativist does not have to treat this other ES as equal to her own ES just because the other ES is consistent or coherent (Kusch, 2019b). The realization that the constitution or structure of an alternative ES is similar to our own does not provide reasons to convert to it. The relativist could still hold that her ES is better because of differences between these ES. For example, both systems could rank specific values differently. But the relativist would realize that she might not convince proponents of the alternative ES to convert to hers. This example describes the type of symmetry that is characteristic of ER.

My last remark concerns "pluralism." Kusch's account of relativism lists pluralism as one of its characteristics. Intuitively, this suggests a close relationship between ER and SP. If pluralism is a characteristic of epistemic relativism, then all card-carrying relativists by definition need to support pluralism.. But are all relativists pluralists? At first glance, it seems intuitive for relativists to accept and cherish pluralism. I submit that this is, however, not the case. More terminological care is required. The relativist's notion of pluralism, as defined by Boghossian (2006) or Kusch (2019a), does not match the criteria for pluralism I provided in Chapter Five. The relativist's "pluralism" is similar to what I defined as "plurality." Kusch's and Boghossian's "pluralism" proposes the plurality of ES. But it does not propose a specific attitude concerning the plurality of ES. Kusch's and Boghossian's "pluralism" does not accept this plurality as inherently good. This version of "pluralism," thus, lacks the criterion of "acceptance" that I defined in Chapter Five.

I, therefore, suggest that the characterization be modified. Keeping Kusch's order, I suggest redefining "pluralism" to "plurality."

2') Plurality: there are, were, or might be several ES.

2')a) "Descriptive plurality": plurality is a "historical / empirical fact" which might give empirical evidence for ER;

2')b) "Normative plurality": plurality of ES is a highly desirable situation.

2')b) Appears to be a quite controversial claim for the relativist. Closer examination, thus, is required. While providing this examination, I will develop my first argument on why ER does not entail SP. In contrast to 2')a), 2')b) implies "acceptance." The relation of "acceptance" and "dependence" needs to be examined. The relativist asserts "normative plurality" from the perspective of a particular ES. But even though plurality might appear extremely beneficial relative to the perspective of one ES, a member of a different ES might come to a different judgment. Imagine the following scenario:

For speaker$_1$, member of ES$_1$, the sentence "plurality of ES$_1$ and ES$_2$ (and ES$_{(n)}$….) is highly desirable" is true. For speaker$_2$, member of ES$_2$, the sentence "plurality of ES$_1$ and ES$_2$ (and ES$_{(n)}$…) is highly desirable" is false.

In this scenario, the epistemic relativist (speaker$_1$) could grant that speaker$_2$ has good reasons to think that plurality is not highly desirable. Speaker$_1$ would nevertheless judge that speaker$_2$'s assertion is false, relative to ES$_1$. Would that be problematic for the epistemic relativist? It is often assumed that the relativist would have to conclude that both speaker$_1$ and speaker$_2$ are correct. This is not the case. Speaker$_1$ is wrong relative to the perspective of speaker$_2$ in ES$_2$. And speaker$_2$ is wrong relative to the perspective of speaker$_1$ in ES$_1$.

What I explicated in the previous paragraph differentiates, in fact, the relativist's normative plurality from SP. The pluralist often occupies a view from nowhere or a god's eye perspective when judging whether plurality is beneficial or not. The relativist rejects that a view from nowhere is possible. I take this to be the first reason why ES does not necessarily imply SP. Relativists who embrace 2'a) but not 2'b) are not necessarily pluralists. Relativists who embrace 2'b) might be pluralists but only in ways that do not violate "dependence."

My second argument why relativism does not entail pluralism rests on "exclusiveness." Some versions of SP do not consider the presence of several ES. Such versions of SP describe theories or explanations within a framework that is considered one ES. But even if a pluralist position proposes the existence of more than one ES, exclusiveness might not play a role. Some scientific pluralists argue for integration across ES, "paradigms," "systems of practice," or styles of reasoning. For example, Chang emphasizes ad hoc integration across "systems of practice."

Chang's example points to another problem of the notion of "exclusiveness" which is related to my elaborations in Chapter Three. Perceived differences in ES might be conceived differently by different actors. Scientific pluralists often neglect heterogeneity of that sort. Chang's account of the Chemical Revolution provides a good example. During the Chemical Revolution, some actors regarded "phlogiston" and "oxygen" as entities pertaining to two different systems of practice. Some worked with a "hybrid system" employing both entities, whereas some did not. Before Chang published his case study, the phlogiston system and the oxygen system were often presented as text-book examples of two different ES (Kuhn, 1962/2012, 86). Chang, however, emphasizes the hybridity of both systems. In conclusion, a particular view on the exclusiveness of the two systems might not be universal.

As pointed out in Chapter Three, some disagreements might not be regarded as "real" examples of ES that clash. Disciplinary, ideological, and methodological divides cause such disagreements. These divides are perceived as real by the relevant actors and thus need to be investigated with great care. Barry Barnes's analysis comes in helpful:

> "In many cases it is irresistibly convenient to talk of the boundaries of a scientific field; but such talk is of a pattern people have managed to generate in their activity, not of something which determines their activity" (Barnes, 1982, 114).

In conclusion, the concept of ES is problematic in itself and its boundaries and its constituents are dependent on the heterogeneous perspectives of actors. I, therefore, abstain from a strong commitment to what makes two ES incommensurable and what is a "real" case of two ES. This is in line with my arguments regarding whether there are "real" cases of integrative or non-integrative plurality. How well two approaches, theories, or systems of belief go together is contingent and not essentially determined.

In the last paragraphs, I discussed a second reason why relativism does not necessarily imply pluralism. Relativism does not apply to forms of pluralism that do not invoke different ES. Also, relativism does not apply to forms of pluralism that formulate interactions of ES in ways that would violate "exclusiveness." The fact that relativism is not compatible with all instances of pluralism should not come as a surprise. In Chapter Five, I surveyed the different levels of SP. SP might concern cases ranging from Hacking's "styles of reasoning" to small-scale decision making on the bench. Relativism might not apply to all of these levels equally well.

My third argument why ER does not entail SP centers around the claim that SP rejects (any kind) of symmetry. The pluralist might violate any of the proposed forms of symmetry (5a-5d). Take, for instance, a normative pluralist position. A normative pluralist might not care about symmetry. In contrast, she could maintain that there is one, single, and absolutely best ES/approach. This would not keep her from cherishing the value of having more systems/theories than this one. For instance, she could point to Chang's benefits of tolerance and interaction. Or she might argue even though some other ES/theories are worse than others, they still capture interesting aspects of the world. I take this as the third reason why relativism does not necessarily imply pluralism: symmetry is often violated. After having provided three arguments that ER does not entail SP, I will examine the issue from a different angle. I will assess whether SP entails ER.

6.1.2 Pluralism does not entail relativism

Before examining the relation of SP and ER from a different angle, I will once again present my definition of SP. In Chapter Five, I listed three necessary and sufficient criteria and claimed that every scientific pluralist must conform to these criteria. All pluralist positions I surveyed in Chapter Five fulfilled these three criteria. I take this as a sign that the definition is adequate. The criteria for SP are:

1) Non-monism: it is not known or denied that the nature of the world is such that it can, at least in principle, be completely described or explained by one comprehensive account.

2) Plurality: there are, might be, should be (the verb be can have different modalities here) several theories, explanations, or methods to approach the same natural phenomena.

3) Acceptance: plurality is favorable.

In the previous section, I tried to show that relativism does not entail pluralism. I proposed versions of pluralism the relativist needs to reject. In this section, I will establish that pluralism does not entail relativism. In so doing, I will propose relativist positions a pluralist needs to reject. It is not my aim to show that there is no version of relativism that accepts pluralism, and vice versa. Primarily, I counter statements of the form “all x are y” by showing proofs of principle against such statements.

In the last section, I demonstrated that “pluralism,” as defined by Kusch and Boghossian, resembles the pluralist’s “plurality.” As a result, a card-carrying pluralist might not criticize ER based on the criterion of “pluralism” or, as I redefined it, plurality. The criteria of “non-monism” and “acceptance” deliver better reasons for a pluralist to reject ER.

It is important to note that although pluralists reject monism, they do not necessarily reject absolutism. Pluralists might claim that it is absolutely true that SP is the superior way of gaining knowledge. Thus, there could be absolutist pluralists. A pluralist might reject versions of relativism that argue that ES are exclusive and cannot be ranked. This might obstruct the pluralist’s agenda. SP usually accounts for many different approaches. Ranking these different approaches might be crucial for the scientific pluralist. “Non-monism,” however, might lie in conflict with certain relativist positions. As discussed above, the relativist does accept “plurality” but not necessarily “pluralism.” It is possible to be a relativist monist. As argued in Chapter Five, there are also monist interpretations of plurality. The epistemic relativist proposes that there is a plurality of different ES. But she might have a negative attitude concerning that fact. A relativist could, in addition, argue that within her ES, only one theoretical account should be maintained. A card-carrying scientific pluralist would need to reject such versions of ER.

A pluralist, in addition, needs to reject forms of relativism that violate "acceptance." In the relativist's account, the statement "plurality is highly desirable" might have different truth values in different ES. For the pluralist, the benefits of pluralism are absolute. If the pluralist aims to influence science policies and other decisions, she might need to absolutize "acceptance."

In the previous two sections, I proposed three reasons why the relativist would have to reject specific versions of SP and two reasons why the pluralist would have to reject specific versions of ER. The observation that relativists might reject specific versions of SP might appear surprising. It is often argued that "extreme" versions of pluralism are instances of relativism. As reviewed in Chapter Five, Kellert, Longino, and Waters support this position. This section offers arguments against this view. There are versions of pluralism a relativist needs to reject. Are these pluralist versions "extreme?"

There are several measures and scales to define what it means to be "extreme." I believe that terms like "extreme" or "radical" are not useful when describing philosophical categories. I will thus make this point differently: there might be versions of scientific pluralism that are more contrary to common sense or a received view than versions of epistemic relativism. Take the example of Chang's normative pluralism. Chang's position might be less intuitive than a position a relativist might hold. Chang claims that approaches like creationism should be integrated into orthodox science. The relativist might not grant that. She might claim that the creationist might have good reasons to believe in creationism relative to her ES. But she would maintain that creationism is still wrong.

In conclusion, the pluralist would have to reject specific versions of relativism but not because of the "renouncement of judgment." Instead, the pluralist needs to dismiss those relativist positions that interfere with the pluralist agenda. After having reviewed ER and SP relying on criteria and characterizations, I will try another approach. In the next section, I will examine which value judgments both positions share and do not share.

6.1.3 Pluralism and relativism as stances

In his 2002 book "The Empirical Stance," philosopher of science Bas van Fraassen proposes a new way of understanding philosophical positions. He argues that many positions are not well captured by a list of the dogmata they embrace. He calls this way of defining a position "principle zero":

> Principle zero: iff someone takes position X, she must believe or decide to believe a statement X+, which exists for every position X.

Van Fraassen argues that principle zero does not capture what it means to support a specific position. By embracing a particular position, one commits to bundles of values, emotions, policies, and preferences (VEPPs). Van Fraassen claims that one should understand philosophical positions as stances characterized by a set of VEPPs.

Van Fraassen exemplifies his idea on stances by taking "empiricism" as an example: "Being an empiricist cannot consist in believing some statement about what the world is like." (Van Fraassen, 2002, 47). He argues that one should rather understand empiricism as a rebellion against metaphysics. One should rather capture this position by accounting for the value judgments advocates of the position embrace. Van Fraassen lists the following VEPPs for empiricism (E): E rejects the demand for explanations by postulate, E recalls to experience, E rebels against theory, E idealizes epistemic rationality, E admires science… Van Fraassen concludes: "A philosophical position can consist in a stance (attitude, commitment, approach, a cluster of such – possibly including some propositional attitudes such as beliefs as well)" (2002, 47-48).

In this section, I aim to test whether ER and SP can be understood as stances. In so doing, I will survey their respective VEPPs. First, I will consider the possible benefits and drawbacks of this approach. If van Fraassen is right, conceptualizing positions as stances helps to understand what these positions entail. Also, it might help to distill differences and crystallize disputes between stances. It would make it easier to find a common denominator for advocates of one position. These advantages are highly desirable. There might, however, also be drawbacks to such an approach. Kusch lists some of these drawbacks: It is more difficult to argue for or against a stance than against principle zero. Also, it is not clear why one should focus on VEPPs and not on the very credo or dogma of a position which is motivated by VEPPs (Kusch, 2019b). Whether one should understand a position as a stance or not is an empirical question. There is a circular relation between VEPPs and dogmata.

Kusch maintains that specific VEPPs inform dogmata and these dogmata necessitate specific VEPPs. It is to be assessed empirically, whether advocates of a specific position foreground a doctrine or whether they emphasize VEPPs.

I judge the benefits of understanding positions as stances as convincing enough to test van Fraassen's approach to compare SP and ER. But which specific relativist and pluralist positions to analyze? I will select one form of ER and one form of SP to capture them as a stance: I chose David Bloor's Sociology of Scientific Knowledge (SSK) as a paradigm approach for ER and Chang's normative pluralism as a paradigm approach for SP.

Kusch argues that SSK is an excellent candidate to be analyzed as a stance because SSK emphasizes VEPPs rather than a specific doctrine. Kusch claims that Bloor's central doctrine "There are no context-free or super-cultural norms of rationality" should be understood as a rebellion. This doctrine foregrounds the policies SSK emphasizes. SSK proposes four policies to guide their research: SSK emphasizes how causes bring about belief. The term "causes" includes both social and other causes. Bloor opts for impartiality concerning truth and falsity, rationality and irrationality, success, or failure. The interest in either side of these dichotomies should be the same. SSK prescribes symmetry: the same types of causes should explain both failure and success, truth and falsity, as well as rationality and irrationality. Finally, Bloor requires reflexivity: SSK should apply to itself. By embracing these four doctrines, SSK rejects the "weak program" which maintains the dichotomies SSK rejects. Approaches that are considered part of the "weak program" explain failure but not success by referring to sociological reasons (2019b, 17f).

Bloor argues that the suggested policies are resonant with how empirical science operates. Admiration of science is thus also part of SSK's VEPPs. Especially, SSK emphasizes social science. SSK admires the plurality of different theories, paradigms, or epistemic systems. This plurality informs their studies on disputes within science. SSK faults epistemology and philosophy of science for lacking controlled data input. The results of case studies motivate SSK (2019b, 18).

The policy of SSK informs another value, the value of epistemic humility. Epistemic humility requires that we should not lay universal claim to rationality or truth. Epistemic humility is also a rebellion against any form of absolutism in metaphysics, epistemology, or ethics. SSK also encourages tolerance and equality. SSK emphasizes group processes as explanatory for the phenomena described. This can be captured under the principle of "normative rationality": there are different codifications of our reasoning properties, defined by interests and negotiations of different groups (ibid.).

I will proceed to analyze Chang's pluralism. Chang admires science, natural sciences especially. Chang appreciates the plurality of theories and systems of practice within science. He deduces a specific policy from these value commitments – normative and active pluralism. In every field of study, many different systems of practice should be cultivated. In the spirit of his policy, Chang reprehends ER for not being normative enough. ER, in Chang's opinion, cannot introduce new systems of practice. What motivates Chang's policy is the result of his hypothetical/counterfactual analysis of the phlogiston system.

By admitting a plurality of systems of practice, two other values get introduced: humility and prudence. Chang faults monism for disregarding these values: it involves hubris to believe that there is only one true system of practice and to believe that we already have it. Chang focuses on the individual. This commitment is in close relation to the emphasis on humility and prudence. Not only should science be guided by humility and prudence, but scientists should also acquire these personality traits. Chang emphasizes the individual over the group.

I summarize SSK and Chang's pluralism in the Figure 26:

	SSK	Chang
admiration	Science (Social Science); plurality	Science (Natural Science); plurality
policy	Four tenants of the strong program	normative, active pluralism
faults	Epistemology and Philosophy of Science for lacking controlled data input	Relativism for being unable to introduce new systems of practice
motivation	Results of case studies	historical/counterfactu-al analysis of phlogiston
encourages	Epistemic humility, tolerance, equality	Humility, prudence
rejects	Absolutism (any forms of metaphysics, epistemology or ethics that posit absolutes), "weak program"	Monism
emphasizes	Group processes ("normative rationality")	Individualism

Figure 26 Table of SSK and Chang's pluralism organized by their VEPPs.

What is the result of understanding SP and ER as stances (Fig. 26)? I will start with the shared VEPPs. SP and ER share an admiration for science and plurality within science but both emphasize different branches of the empirical sciences. ER and SP suggest policies, but there is a critical difference between policies in SSK and Chang's pluralism. SSK gives instructions on how to study science, whereas Chang gives instructions on how to change science. It is important to note that the relativist position also invokes policies. SSK is normative because its policies certainly are normative.

Case studies motivate both stances. Case studies that successfully implement the SSK policy motivate SSK. A historical case study motivates Chang's SP. This case study shows that plurality was beneficial during the Chemical Revolution. Chang, additionally, provides counterfactual arguments that pluralism should have been maintained. Both positions encourage humility. Chang points out that there might be more systems of practice than the one we have. SSK argues that there is no reason why our current system of practice is absolutely justified.

Having discussed the similarities of both stances, I will elaborate on what separates them. SSK and Chang's pluralism fault different disciplines within philosophy. SSK criticizes epistemology and philosophy of science for lacking empirical input. SSK would also have to reprehend Chang's case study for the lack of empirical input. Chang, on the other hand, faults relativism (and SSK, specifically) for not being able to implement Chang's active normative policy. Thus, both stances reject or rebel against particular positions. SSK rejects absolutism and the "weak program." Chang rejects monism. Finally, their respective preference for group processes as opposed to individualism, respectively, differs.

In conclusion, portraying SP and ER as stances helps to flesh out differences and commonalities of both stances. Let me, at this point, emphasize what I said at the beginning of this section. It might be an empirical question whether a specific position is to be understood as a stance or not. In this section, treating ER and SP as stances or positions leads to the same result: SP and ER have a different agenda. ER and SP commit to partly similar and partly different values. Because of that, ER and SP do not exclude each other: both positions or stances can coexist within the commitment of one philosopher. For example, one can acknowledge relativism between ES but cherish pluralism within one ES.

Yet, there are, of course, other issues to be solved that diverge from matters of mere definition. I will discuss these issues in the following sections.

6.2 Does pluralism entail relativism? Reviewing the arguments

In this section, I will provide empirical foundations for what I discussed in the previous section. I will analyze arguments provided by pluralists on why or why not SP entails ER. Within the reviewed positions, consensus on the definition of ER is far from clear. I will review how five scientific pluralists conceptualize the relation of SP and ER. I will examine Baghramian's conceptual pluralism, Chang's and Kusch's debate on SSK and the Chemical Revolution, and Longino's Pluralist Stance. These three cases enforce a sharp demarcation between pluralism and relativism. Then, I will discuss Dupré's and Feyerabend's pluralism as examples of philosophers who combine SP and ER in their position.

One common theme that will be encountered in this section is the "renunciation of judgment" which informs the "fear of relativism." Before concluding this section, I will deliver an analysis of the "renunciation of judgment" and ER. I will argue that ER does not entail the "renunciation of judgment." I will argue that it is not detrimental if a pluralist embraces relativism. By exploring ways in which relativists can be normative, I aim at countering the most common argument against relativism.

6.2.1 Baghramian's conceptual pluralism

The philosopher Maria Baghramian argues that relativism captures "essential insights … about what we know, think and believe". Relativism is the correct diagnosis, but "its failing is its response to this" (2004, 232). Baghramian holds that there is little value to embracing relativism:

> "the price is either intellectual and moral paralysis – the inability to compare and evaluate what lies outside our immediate cultural and conceptual surroundings – or the very predicament that the relativist wishes to avoid: parochialism and ethnocentrism" (ibid.).

The solution to this dilemma is "conceptual pluralism." A conceptual pluralist acknowledges "conceptual diversity." But there are limits to this diversity, and "we can distinguish between better and worse or more and less fruitful or productive, conceptual systems" (2004, 233). "The pluralist," she claims, "believes that there are culture-transcendent constraints on what is an acceptable belief- or value-system." (ibid.).

Baghramian suggests that the metaphor of "human beings as mappers, and conceptual schemes as maps or systems and principles of mapping the world" (2004, 240) helps to demarcate conceptual pluralism from relativism. The most crucial feature of mapping is that it allows us to retain the possibility of evaluating and comparing. She lists eleven features of maps to illustrate how to make sense of different conceptual schemes:

1) Maps are necessarily a view from somewhere;

2) there is no single best way of drawing a map;

3) maps are purpose-relative;

4) there are better and worse, more accurate and less accurate maps;

5) whether maps are good or bad depends, in part, on the respective purpose;

6) maps are not made true by the world but might be true to the world;

7) standards for mapping are historically contingent;

8) there is no single, absolutely correct map;

9) there is no global convergence; there is no map of all maps;

10) mapping can be applied to several contexts;

11) maps aim for structural isomorphism between representation and represented. (2004, 240ff).

Baghramian claims that if pluralism guarantees that it remains possible to compare and evaluate different conceptual schemes, then it avoids the charge of relativism. The comparative merits of conceptual schemes "depend on their usefulness and success – and their success, to a significant degree, is contingent upon the world they are mapping." (2004, 244). While acknowledging a diversity of conceptual schemes, Baghramian rejects conceptual relativism (ibid.). Pluralism allows preferring one framework over the others (ibid.). Considering my discussion of ER in the last section, it is clear that Baghramian presents a quite unattractive version of relativism. As argued in section 6.1, the relativist can also give preference to one conceptual scheme. The first part of Baghramian's argument, therefore, needs to be rejected.

It is also possible to challenge the second part of Baghramian's argument. The relativist can compare different conceptual schemes. The mode of comparison, however, might be different from that suggested by Baghramian. Her conceptual pluralism allows for comparing different conceptual schemes to reach a conclusion which conceptual scheme is better. For the relativist, comparison is not possible based on neutral standards. Comparison is always relative to a set of standards. But this is not unique to relativism. This is the case for Baghramian's conceptual pluralism as well. There is no "map of all maps." A "conceptual scheme of conceptual schemes" is, thus, also impossible. Comparing conceptual schemes on neutral grounds is, therefore, impossible. In conclusion, Baghramian is not successful in setting apart conceptual pluralism and relativism.

6.2.2 Chang, pluralism, relativism, and SSK

I have discussed Chang's pluralism in several previous parts of this dissertation. In this section, I focus on Chang's attempt to demarcate his version of SP and ER. He aims at demonstrating that pluralism is the more "radical" thesis. Pluralism commits to action; relativism does not. As discussed in the previous chapter, Chang classifies his pluralism as epistemological, normative, and active. Commitment to "active" and "normative" pluralism demarcates his position from relativism:

> "It is important to distinguish pluralism from relativism. Relativism involves an idle permissiveness and renunciation of judgment. Pluralism does not renounce judgment, yet maintains that it is better to foster a multitude of worthwhile systems, rather than only one. Pluralism as I conceive it actively engages in the work of

> proliferation; it is about knowledge-building, not just knowledge-evaluation." (Chang, 2012, xx)

It is important to note that Chang's position concerning relativism is not altogether clear. In "Is water H2O?," Chang claims that he does not understand why relativism should be a "dirty word." He provides David Bloor's 2007 defense of relativism as support for this notion. In a different paper written three years later, Chang argues that he is "for pluralism and against relativism" (Chang, 2015, 97). In yet another paper, written four years later, Chang provides a more sympathetic position towards relativism (Chang, 2020).

In "Is water H2O?," Chang maintains that the distinction of pluralism and relativism as pivotal. It is central to relativism to renounce judgment and commitment, at least to a degree. This is not the case for pluralism (2012, 261). While the "mature" pluralist engages with what she disagrees with, the relativist might say "whatever" (ibid.). Scientific pluralism aims at challenging monism. If the pluralist introduces a plurality of systems of practice, paradigms, conceptual schemes, theories, explanations, methods…, then why should not monism be one constituent of this plurality? In short, shouldn't pluralism also apply to itself? Shouldn't the pluralist be a pluralist about pluralism and thus allow monism? Chang denies this question. Although he allows a limited amount of monism in science, Chang argues that he does not support regarding philosophy as a part of science. Scientific pluralism is a philosophical thesis about science. As philosophy is not science, scientific pluralism does not apply to itself, because scientific pluralism is not a thesis about philosophy (2012, 314).

This move might be a bit too quick and easy for Chang. Chang's pluralism applies more globally than he admits at this point. For example, Chang argues that liberal democracy is pluralist in that it admits and tolerates even somewhat maverick positions. Liberal democracy even tolerates positions that are against liberal democracy to some extent. Thus, there are at least two institutions – politics and science – which he is a pluralist about. Chang needs to explain what differentiates philosophy from these two institutions and why philosophy should not be a pluralist institution.

Let me resume on Chang's discussion of relativism. He argues that pluralism is a more radical position than relativism. Chang shares my assumption that relativism does not necessarily imply pluralism (2012, 261), but for other reasons. Chang's argument goes as follows:

> "If relativism only insists on the equal treatment of any alternatives that do exist, there is no requirement that there should be multiple alternatives. If everyone actually agrees on something and no one seeks any alternatives, relativism has no strong way to oppose that state of affairs" (ibid.).

Chang maintains that relativism cannot criticize and replace a system of practice that impedes the introduction of plurality. At one point, Chang even speculates whether relativism is the doctrine that we should make our choices randomly or not at all (ibid.).

It is worthwhile to look at a critique of Chang's position advanced by a card-carrying relativist, Martin Kusch. Kusch's critique centers around the issue of (epistemic) justification. Chang claims that the "Chemical Revolution" was not justified: there were no satisfactory reasons for adopting Lavoisier's program and abandoning the phlogistonists' program. To counter Chang's argument, Kusch introduces a different perspective on the Chemical Revolution. Kusch suggests approaching justification via the Sociology of Scientific Knowledge (SSK). An SSK approach shows that practitioners in the 1780s and 1790s were, in fact, justified to adopt Lavoisier's chemistry.

In what follows, I examine the arguments put forward in Kusch's and Chang's dispute on relativism and pluralism. Kusch argues that Chang's analysis of the Chemical Revolution is wrong because Chang disregards evidence that an SSK approach can uncover: "Chang needs to convince us that it was irrational for chemical communities of the late eighteenth century to abandon phlogistonism." (Kusch, 2015, 74). Kusch aims to show that researchers had good reasons to convert to the Lavoisierian system in the 1780s and 1790s. If Kusch can show that researchers had good reasons, then Chang's case for prescriptive pluralism fails. It fails, at least, for the specific historical period Chang studied. Kusch specifies these reasons as "indirect" reasons. These reasons involve issues related to social indicators of reliability, amongst others. Kusch opines that such reasons are always intertwined with "narrow reasons." Narrow reasons are such that refer directly to experimental output. Most of the time, it is not possible to dissect narrow and broad reasons. Kusch argues that a sociological analysis of the Chemical Revolution makes it hard to maintain Chang's interpretation of the Chemical Revolution. Upon closer examination, Chang cannot uphold his claim that pluralism should have been maintained:

> "Chang's argument is based on the idea of two socially uncontaminated "pure" sets of good reasons for and against phlogiston. And these sets simply were not there to be had. The historical actors only had considerations that fused together experimental data, skills, tradition, religion, differential distributions of trust, and much else besides. It is hard to formulate pluralist prescriptions concerning such complex considerations" (2015, 78).

Let me make a remark regarding Chang's motivation for prescriptive pluralism in his case study. If Kusch is right, then it might be hard to formulate pluralist prescriptions for the Chemical Revolution. Nevertheless, I will describe what motivates Chang's prescriptiveness. Having studied the history of phlogiston, Chang concludes that phlogiston theory was fruitful and that researchers were justified to hold on to it. This leads to his conviction that the phlogiston theory should have been preserved. In his most recent work, Chang even claims that phlogiston has a "degree of reality" (Chang, 2018, 33).

Kusch finds Chang's adoption of the agenda of the "losing side" problematic. Kusch criticizes Chang for adopting the phlogistonists' actor's sociology: reconstructing the phlogistonists' arguments, Chang adopts these arguments for discrediting oxygenists (Kusch, 2015, 73). In so doing, Chang violates some characteristics of ER. For example, he violates "conversion." "Scientific revolutions" are often considered textbook examples of conversion. In times of a scientific revolution, there is no reason for changing one's ES by rational comparison. Other processes cause "conversion" from one paradigm to the other. Chang considers "conversion" detrimental because conversion causes Kuhn-loss. Even though there might be "sociological" reasons which justify conversion, one should resist them. If there are no compelling "scientific" reasons, then nothing should happen. This, however, rejects the core assumption behind the relativist's "conversion," no rational reconstruction of "jumping ship" is possible prior to conversion.[45] Chang rejects the very idea of conversion, as he does not believe that "broad" reasons (should) influence theory choice. In conclusion, if Chang denies the importance of "social reasons," he might also reject "conversion." Chang's rejection of "conversion" might lie at the core of Chang's and Kusch's dispute.

Before concluding this section, I will examine Chang's arguments on why it is reasonable to dislike what happened during the Chemical Revolution. Chang rejects the Sociology of Scientific Knowledge because he believes that, through its impartiality principle, SSK avoids value judgments. In other words, Chang fears that SSK "renounces judgment." Chang suspects that Kusch believes that Chang comes to the wrong normative conclusions (2015, 96). Chang criticizes Kusch for an "unspoken normativism" about SSK. This normativism is the belief that

> "an individual ought to behave in a way that our best theories of social dynamics dictate, thereby following good social reasons. An individual who offers resistance to the standard social forces would actually be regarded as irrational, in the sense of behaving incomprehensively." (2015, 97)

Chang localizes his disagreement with Kusch in the fact that he (Chang) argues 18th-century scientists ought not to have followed Lavoisier, whereas Kusch thinks they ought to have. Chang objects to focusing on what is reasonable for the group. The choices of the individual should be emphasized. If it is only rational to do what the social group does, then the choices of the individual scientist could be characterized as irrational (ibid.). Besides, Chang accuses Kusch of thinking that "good social reasons should tend towards monism."

Two aspects of SSK need to be differentiated at this point: methodological prescriptions and the results of case studies. Methodologically SSK does not favor monism. As is the aim of most social sciences, SSK aims to draw generalizations from case studies. One of the most prominent generalization is the Experimenters' Regress. In case studies on the Experimenters' Regress, monism is often the result of closure. "Closure" does not necessarily mean monism. "Closure" of the regress could also mean that practitioners agree

[45] Bas van Fraassen, for example, uses Kafka's metamorphosis as an example. One cannot rationally reconstruct how the father changed his opinion from "This is my son, who is ill" to "This is a beetle that we must kill". However, the sister's emotional breakdown was a socially salient factor.

to keep both theories they disagreed about. It would be highly exciting to find such instances of the regress. Also, it would be quite exciting for the SSK scholar to describe social processes that encourage plurality or even pluralism. It, thus, becomes possible to identify a misunderstanding by Chang: Chang dislikes SSK approaches because he thinks that they require the closure of disputes to result in monism. This, however, is not the case.

What is there to learn from Chang's and Kusch's dispute? Chang and Kusch both maintain that pluralism and relativism are two different and not necessarily related positions. Their dispute exemplifies reasons why the relativist might find pluralism objectionable and vice versa. Chang rejects SSK-informed relativism because he believes that ER rejects the normative and active commitment he envisions. Also, he criticizes the impartiality of ER in general. I do not share Chang's opinion that Kusch favors normative monism or thinks social reasons should always produce monism. Kusch makes it quite clear that SSK is compatible with scientific pluralism (Kusch, 2015, 78). SSK case studies can inform case studies on pluralism. For example, in Chapter Three, I used Collins' case study to make a point about pluralism. SSK scholarship provides essential input for refining scientific pluralism.

In conclusion, I believe that Chang's and Kusch's accounts do not straightforwardly weaken each other. Kusch adheres to a descriptive interpretation that shows how theory choice in times of dispute is not settled by "narrow" but by "broad" reasons. Chang agrees that there were no narrow reasons to abandon phlogiston. I interpret their disagreement as a form of a philosophers' regress. Both neither agree on the correct interpretation of a specific historical period nor on what the relevant data for the correct interpretation consists in.

I think that both Chang's normative and Kusch's descriptive position can be maintained. If Chang is ready to admit that broad reasons might settle debates (and often erase plurality), then it is more than necessary to consider SSK investigations. He might consult SSK case-studies to ask what measures for maintaining a regress ad infinitum would be. What is required to force pluralism as the outcome of closure? SSK studies might aid Chang in answering these questions.

6.2.3 Helen Longino and the Pluralist Stance

In this section, I will review the strategy of the Pluralist Stance to demarcate SP and ER. In Chapter Five, I criticized the Pluralist Stance by challenging their classificatory system. In this section, I will examine what they take to be "radical" pluralism: forms of pluralism that entail relativism. I will review their arguments on how to exclude "radical pluralism."

According to the Pluralist Stance, there are several forms of pluralism. Different forms of pluralism have different consequences. Modest pluralism leads to monism; radical pluralism leads to relativism. The Pluralist Stance explicitly characterizes Dupré as a relativist pluralist. Kellert, Longino and Waters criticize Dupré's "promiscuous realism" because Dupre presupposes that

> "there are an indefinite number of ways of individuating and classifying the objects in the world, each of which is responsive to different interests, and no one of which is more correct than the others" (2006, xiii).

Thus, Dupré's pluralism amounts to "radical relativism" (ibid.). Kellert, Longino, and Waters, also, advance criticism on relativism in general. The authors "are committed to the idea that there are constraints that limit the variety of acceptable classificatory or explanatory schemes" (ibid.). Kellert, Longino, and Waters deny that the Pluralist Stance entails "anything goes." Pluralists should not accept instances of bad research in their pluralism. The "worst" research, they argue, is dogmatic and demolishes pluralism (2006, xxvii).

Especially in Longino's writings, it becomes clear that the adoption of values serves the purpose of shielding the Pluralist Stance from relativism. In section 6.3, I will argue that invoking epistemic standards does not avoid relativism. Instead, it shifts the "problem" to another level. If the choice of the values/virtues to assess a theory is always dependent on the local context or the precise question one is asking, then one might quickly adopt a relativist position on value choice. At one point, Kellert, Longino, and Waters argue that "which explanation is appropriate depends on the precise question one is asking." This statement recalls of Dupré's position on purpose relativity. In conclusion, the authors of the Pluralist Stance mischaracterize relativism concerning the "renunciation of judgment." At the same time, by invoking the purpose dependence of which explanation is appropriate and which is not, they cannot successfully show how their version of SP differs from ER.

6.2.4 Dupré, disorder and relativism

In "The Disorder of Things," Dupré anticipates the question of whether his promiscuous realism is a version of relativism. At first glance, Dupré's pluralism appears to sympathize with relativism. He emphasizes that scientific theories are good or bad relative to specific purposes. Dupré argues that if purpose-relativity entails relativism, then his pluralism accepts relativism. Different questions might arise from different purposes. A specific explanation is appropriate relative to whether it addresses the purpose of the question. Given its emphasis on purpose sensitivity, it becomes even more apparent that the Pluralist Stance is not as different from Dupré's promiscuous realism as the authors of the Pluralist Stance propose. In what follows, I will give a brief example of Dupre's notion of "purpose-dependence."

Dupré emphasizes the purpose-dependence of classifications: classifications of "onion" and "garlic" are an example. There is more than one way of classifying "onion" correctly. For example, there are culinary and biological purposes. If I wanted to know about the phylogeny of "onion," I would group onion and garlic together because both species are closely related. If I wanted to know how vegetables taste when cooked, I would have to use a different classification. Onion and garlic would not be grouped together. If I were to cook onion soup, it would be detrimental to use garlic instead of onion. Dupré insists that both classifications are correct and that none is "more correct" than the other. Realism is promiscuous.

Dupre maintains, however, that he disagrees with particular versions of relativism. Relativism is often characterized as the "denial that any scientific account could be true, or the insistence that all scientific theories have equal epistemic credentials and equal value" (1993, 261f). Dupré provides SSK as an example of this version of relativism. Whereas he considers SSK innocuous as a methodology, he does not accept SSK as an epistemological or metaphysical doctrine. Dupre claims that SSK entails "equal validity." As discussed above, “equal validity” characterizes the “silly version” of ER and is not part of the epistemic relativist positions of SSK.

While accepting a version of relativism that focuses on purpose dependence, Dupré emphasizes the limits of pluralism. It would be a fatal flaw of SP if it were not possible to acknowledge the superiority of evolutionary theory over creationism (1993, 242). To address this problem, Dupré introduces “virtue epistemology” for discerning what good and bad science is. The following virtues are relevant for making such decisions: sensitivity to empirical fact, plausible background assumptions, coherence with other things we know, exposure to criticism from the most extensive variety of sources, amongst others. These virtues have “the capacity to capture the wide variety of projects of inquiry, without conceding that anything goes.” By introducing these standards, Dupré’s position becomes “overtly and unashamedly normative.” (ibid.). Introducing this virtue epistemology trades being able to demarcate “science” from “non-science” for being able to discern “good” from “bad” science. Dupré argues that his position relates closely to that of Feyerabend. Dupré cherishes Feyerabend’s epistemological anarchism and theoretical pluralism as therapy for science (1993, 263).

As I have shown in the previous paragraphs, Dupré is not concerned that pluralism entails relativism. Instead, Dupré examines whether his pluralism *is* relativism. His answer is yes. His relativism primarily hinges on a position that is similar to the relativist “dependence.” The epistemic relativist argues that the epistemic status of a belief is relative to the respective ES; Dupré argues that theories are relative to particular purposes. I will reformulate “dependence” to make it resonant with Dupré’s version of SP:

> Dependence/Dupré: the applicability/success of a specific theory is relative to the respective purposes of a research field.

Dupré also embraces a form of non-absolutism that is very similar to the epistemic relativist’s version of it. Kusch defined non-absolutism as the position that no ES is absolutely warranted; I will reformulate non-absolutism for Dupré:

> Non-absolutism/Dupré: There are several different purposes, and various theories are good or bad relative to these purposes. None of these purposes are absolutely warranted.

For example, there are several purposes relative to which it is possible to classify garlic and onion. Each approach is good or bad relative to these purposes. None of these purposes, neither the biological nor the culinary, are absolutely warranted.

What differentiates Dupré's relativism from the standard characterization of ER is his rejection of symmetry (Fig. 27). Dupré believes that there are ways of discerning whether a theory is "good" or "bad." Admitting that a project is justified relative to a specific purpose does not mean that this project is good. Internal justification can be easily attained if a particular approach embraces matching purposes. This does, however, not say anything about whether the approach is good or bad. Assume that creationism is a project that presents itself as scientific. The purpose of creationism is to prove the Bible right. Relative to the goals and purposes creationism embraces, creationist approaches seem warranted. But at this point, epistemic virtues come into play. If assessed by epistemic virtues, creationism fares poorly. While admitting justification to all kinds of intellectual projects relative to their purposes, Dupré's pluralism judges which projects are better or worse. My interpretation of Dupré is that it does not matter whether we call creationism scientific but "bad" or whether we stop using the term "scientific" as a predicate altogether.

It is, however, essential to note that discerning between "good" or "bad" theories and using specific values or VEPPs for doing so can also be part of the relativist's position. What the relativist denies is only the absolutist character of this judgment. Thus, ER and promiscuous realism might have several similarities. My remark on VEPPs invites the question of whether Dupré's version of pluralism can be regarded as a stance. In the remainder of this section, I will try to capture Dupre's position as a stance. In so doing, I will assess how Dupré's stance compares to the pluralist (Chang's) and relativist (SSK's) program.

The metaphysical intuition that the world is disordered motivates Dupré's position[46]. Dupré admires science, more particularly, natural sciences. Dupré rejects absolutism, monism, essentialism, determinism, reductionism, and scientism. Dupré emphasizes group processes over the decisions of individuals. He preaches caution not to regard science as a monolithic community, however. He reprehends "scientism" and the "equal validity" thesis. There is no specific policy attached to Dupré's position, yet he encourages disunity and virtue epistemology. As a result, Dupré's stance compares to SSK and Chang as depicted in Figure 27.

It is easy to see that Dupré shares commonalities with both Chang and SSK. This shows that his position is correctly characterized as relativist and pluralist. Dupré rejects the equal validity thesis. If we accept that equal validity is not part of the relativist program, then Dupré does not disagree with SSK. Of course, as he is also a pluralist, there are some areas in which he overlaps more with Chang than with SSK. In most cases, he shares parts of the pluralist and parts of the relativist position. For example, Dupre rejects absolutism and monism and he emphasizes both group processes and individual processes. Dupré is, therefore, an example of a pluralist who accommodates relativist tendencies.

[46] In "The Disorder of Things," Dupré claims to start with a metaphysical presupposition to motivate his analysis. In personal communication, Dupré insisted that it is not clear whether a metaphysical intuition or empirical validation of that intuition came first in his intellectual history.

	SSK	Dupré	Chang
admires	Science (Social Science); plurality	Science (Natural Science) Plurality	Science (Natural Science) Plurality
policy	4 elements of the strong programme	None, "descriptive pluralism"	Normative, active pluralism
faults	Epistemology and Philosophy of Science for lacking controlled data input	Scientism, "equal validity"	Relativism for being unable to introduce new systems of practice
motivation	Results of empirical case studies	Metaphysical presuppositions on disunity	Historical/counterfactual analysis of phlogiston
encourages	Epistemic humility	Virtue epistemology, disunity	Humility, prudence
rejects	Absolutism (any forms of metaphysics, epistemology or ethics that posit absolutes), "weak program"	Absolutism, monism, essentialism, reductionism, determinism, scientism	Monism
emphasizes	Group processes	Group processes and individual processes	Individuals

Figure 27 SSK, Dupré's pluralism and Chang's pluralism organized by their VEPPs.

6.2.5 Feyerabend: pluralism and relativism – happily ever after?

Paul Feyerabend's position provides a compelling perspective on the relation between pluralism and relativism. Let me start by contextualizing his proverbial "anything goes." Most philosophers that advocate the one or the other form of scientific pluralism distance themselves from this doctrine. Most pluralists (wrongly) quote Feyerabend and provide reasons why their position prohibits “anything goes.” The resentment towards Feyerabend can be explained by the fact that “anything goes” is often equated with the renunciation of judgment. Feyerabend is considered the poster-boy of relativist (radical) pluralism. While “anything goes” is one of Feyerabend’s core doctrines, it is a no-go for most of the pluralists that came after him. There is, however, a tension between rejecting “anything goes” and maintaining an alluring pluralist position. While trying to avoid any suspicion of relativism, pluralists need to keep their position as strong and interesting as possible: if very little “goes” then it might be hard to find many (non-trivial) cases of SP. If there is a fear of investigating unorthodox theories, then it might be hard to achieve a plurality of theories after all.

I will examine the relation of pluralism and relativism in Feyerabend’s works. In addition to pluralism and relativism, epistemological anarchism is another essential concept. In earlier works, such as the examples I reviewed in Chapter Five, Feyerabend embraces pluralism, relativism, and epistemological anarchism. In later works, tensions arise between anarchism, pluralism, and relativism. Feyerabend’s notion of relativism changes in order to accommodate other aspects of his position. In his earlier works, relativism resembles standard definitions of ER. In later works, Feyerabend’s relativism diverges from these standard definitions because the exchange between multiple ES becomes an essential aspect of Feyerabend’s relativism. This tendency starts with “Science in a Free Society,” where he elaborates on the conditions of “free” and “undogmatic” exchange. Feyerabend calls such versions “practical relativism” and “democratic relativism” (Kusch, 2016, 109). In this section, I will discuss what motivates Feyerabend's anarchism, pluralism, and relativism, respectively. I will point to possible tensions between these positions but also discuss how these positions relate to each other.

Feyerabend’s relativism is based on the assertion that no standard is absolute. This is also true for assumptions that are central to our scientific world view. Concepts such as rationality, causality, and objectivity are not absolute. Feyerabend describes these concepts as standards that might change in response to certain discoveries.

Feyerabend’s anarchism is based on the assumption that there is no rule such that there might not be an occasion in which it is worth breaking. No rule is principle sacrosanct.

Feyerabend differentiates his position from "naïve anarchism." The "naïve anarchist" holds that both absolute and context-dependent rules have their limits. As a result, they are worthless and should be given up. Feyerabend's epistemological anarchism is different. While Feyerabend believes that all rules have their limits, rules should not be given up (1975/2010, 231). Rules need to be implemented so they can be broken. Epistemological anarchism, that is, breaking the rules, requires orthodoxy, that is, the existence of rules, in a certain sense. We advance science by establishing rules as well as developing research programs and theories that break these rules.

Feyerabend's theoretical pluralism is closely related to the concept of counterinduction. In order to test orthodox theories and introduce new empirical content, alternative theories should be introduced. Practicing counterinduction leads to theoretical plurality. Theoretical plurality is a situation in which there are many different alternative theories. This is beneficial because alternative theories make new empirical facts accessible and are tools for criticizing orthodox theories. In addition, alternative theories also save orthodox theories from becoming "myths" by losing their empirical content.

Let me investigate how these three concepts relate. It is important to note that anarchism might be problematic for several versions of pluralism. Pluralist positions that insist on a definite demarcation between science and non-science or "good" and "bad" theories will not agree that all rules should be broken. This is not the case for Feyerabend's position. Counterinduction and anarchism are closely related. Counterinduction is the activity that implements breaking rules and standards. Through formulating a theory that is not following specific rules or standards, one executes counterinduction. Therefore, counterinduction is an epistemological anarchist's central activity. Feyerabends' relativism supports anarchism because it provides arguments for why every rule should be broken: no standard is absolute; there is no good argument why standards are untouchable.

I propose a scheme to illustrate how Feyerabend's' positions relate (Fig. 28): relativism informs anarchism through its rejection of absolute standards. The anarchist performs counterinduction, that is, she breaks the rules, once in a while. Counterinduction leads to theoretical plurality. If Feyerabend embraces plurality, then it leads to theoretical pluralism. Based on this scheme, it is possible to deduce that relativism and pluralism operate on different levels.

Figure 28 The relation of epistemological commitments in Feyerabends' positions.

It is also important to note that neither relativism nor pluralism has the most central role in Feyerabend's framework. Rather, epistemological anarchism (theoretically) and counterinduction (practice) locate at the core of his position[47].

In conclusion, pluralism and relativism operate on different levels. Feyerabend's pluralist epistemology and methodology – the introduction of several alternative theories - has certain limitations. It is contingent on the specific historical situation in which standards are in place and can, therefore, be broken. Feyerabend's counterinduction and theoretical pluralism remain in the realm of "many things go" due to the limitations set by the availability of alternatives and the historical contingency of standards. Relativism "gives an excellent account of the relation between dogmatic world views" (1975/2010, 268). Looking at the history of science, "anything goes." At different times, different standards are in place.

In the last sections, I discussed several pluralist positions and their individual assessment of relativism. Some of the pluralists reviewed insist that their position does not entail relativism. One central concern of these philosophers was that accepting ER would lead to renouncing judgment. Some pluralists, however, admit relativism within their pluralist

[47] There are, of course, other possible interpretations. Kusch, for examples, bases relativism epistemological anarchism and pluralism as the three central tenets of Feyerabend's program. Counterinduction follows from the combinations of these three tenets (Kusch, 2016).

positions. To conclude this section, I will discuss one issue that kept appearing in my discussion on the relation of pluralism and relativism. I will review the validity of the claim that relativism involves "renouncing judgment." I will counter this notion. It is my aim to show that relativist positions can also serve the program of pluralism and that a "relativist touch" or embracing relativism alongside pluralism is not detrimental to a pluralist position. On the contrary, certain aspects of relativism might add to the pluralist agenda.

6.2.6 "Renunciation of judgment" revisited

In this section, I will address a problem that is omnipresent in the literature but rarely dealt with properly. "The renunciation of judgment" is one of the most prominent accusations against ER. Most critics of ER legitimate this accusation by pointing to the fact that relativists embrace "symmetry." Because relativists hold that there is no objective or independent way of judging between different systems, they refrain from judging between systems at all. There is a peculiar asymmetry concerning these accusations. These accusations are regularly upheld in arguments by pluralists against relativism. It is not, however, easy to find relativists who address this specific issue. In particular, there is no defense offered by an epistemic relativist who discusses the symmetry criterion and its impact on the philosophy of science. Several case studies on ER, however, concern the sciences. One well-known example is the case of Galileo and Cardinal Bellarmine. In this section, I will try to offer a relativist's reply to the accusation. I intend to show that embracing ER does not weaken one's pluralist position. On the contrary, ER can add to a pluralist perspective.

I will start by challenging Chang, who submits that relativism is too passive. Chang argues that relativism has "no bite": the relativist can only address situations in which there is more than one system of practice in place. The relativist cannot address situations in which there is only one system of practice. Contrary to the relativist, the pluralist has the agenda of introducing more theories. Pluralism is thus the more active doctrine. Because relativism "renounces judgment," ER cannot change situations in which monism prevails. In situations of monism, pluralism would require introducing more "systems of practice." Relativism has to take instances of monism as they are. In what follows, I will counter Chang's view. I will propose two ways of how the relativist can address and criticize periods of monism.

I will introduce two approaches for the relativist: the "historical approach" and the "unfolding approach." Both approaches start with suspicion. Although relativists do not subscribe to "non-monism," a relativist might nevertheless be suspicious about situations in which she can only detect one ES. Plurality is an essential feature of ER. If the relativist conjectures that plurality is characteristic of most human intellectual projects, then she would undoubtedly be surprised by an instance of monism. It is, in fact, an understatement to say that she would be surprised. More precisely, the relativist would be suspicious about a field that does not display plurality. She would have enough reasons for this suspicion. There are enough case studies that show that monism is often not stable. For example, Kuhn suggests that "immature" science always displays a plurality of theories. In addition, the notion of "scientific revolution" suggests that different theories follow each other in one research field. The absence of plurality is, therefore, a justified reason for suspicion. Taking this intuitive suspicion as a starting point, there are two strategies to address monism.

First, I will describe the historical approach. Assume that an epistemic relativist is interested in a specific field of inquiry. For the sake of our example, it does not matter whether this concerns the cutting-edge state of this field or another historical period. After examination, the epistemic relativist would conclude that the field displays monity: only one theory dominates the field and there are no alternatives that challenge orthodoxy. Even upon closer examination, it remains hard to find alternative theories or scientists who support alternatives or criticize orthodoxy. It is Chang's opinion that the relativist would have to stop her investigation at this stage. I disagree. As the relativist presupposes plurality, she will find: "that's odd!" But she does not have to stop here.

She could, for instance, look at the history of the field. She would have to investigate the conditions of how the ruling theory became dominant, which theories were left behind, and which theories challenged the orthodox theory in the past. The relativist can then look at periods of theory change or controversy between the relevant practitioners. For a scholar of the Sociology of Scientific Knowledge, it would, for instance, be interesting whether the Experimenters' Regress was at certain times an issue. Thus, the relativist could "excavate" past theories in order to contribute to plurality.

Next, I will describe the “unfolding approach.” This approach requires even more suspicion. The relativist would not stop with the conclusion that “monity” is the outcome of previous periods of plurality. Rather, she would question this state of monity. Even though a theory might seem dominant and efficient in keeping researchers from following other directions, this does not mean that no other approaches are present. Such an analysis can be executed for a specific historical period or a cutting-edge scientific field. To discover heterogeneity within a seemingly homogenous research field, the relativist would have to apply a microhistorical or micro-sociological approach. She would use these approaches to locate and unravel alternative approaches. The relativist would try to find the mavericks, the personae non-gratae, the enfants terribles. At this point, it is important to note that such an approach might lend itself more for a cutting-edge period. Nowadays, scientific communication and exchange are transcending the well-known loci of academic journals and conferences. Media like Twitter and publishing platforms like bioRxive display much more transparency. Things that should not be said in papers or conference talks might be discussed in twitter feeds or manuscript drafts or comments on such drafts. The effects of “gate-keeping” can be bypassed. The unfolding approach is also possible for a historical period. Many brilliant case studies in the history of science show that microhistory and the unraveling of plurality in periods in which monism seems dominant is also a worthwhile exercise in the history of science.

Taken together, both the historical as well as the unfolding approach function as “creators of plurality.” The historical approach offers a perspective on past and abolished plurality. The unfolding approach shows recent but overlooked plurality. Both approaches can complement each other. The historical approach might inform the unfolding approach for which kinds of theories to look out for. Vice versa, by showing which theories are currently part of a research field, the unfolding approach can inform the historical approach for which theories to look in the past.

It is legitimate to ask whether there are periods of “pure” monity, both historically and in cutting edge science. It might be the case that if one assesses a field of study closely enough, one always encounters plurality. This is, of course, a problem of both scope and scale: On the one hand, there are different levels of organization within research on which to look for pluralism. There might be a plurality of methods, theories, disciplines, or ES. On the other hand, it is not at all clear which theories are still considered part of a specific field. The relevant practitioners might judge the most unorthodox approaches not to be part of their research field. As demonstrated in Chapter Four, boundary work regarding which approaches are part of a specific research field was ubiquitous amongst small RNA inheritance researchers.

Given the two approaches I discussed above, I will revisit Chang's critique of relativism. If the ability to create plurality is what differentiates pluralism and relativism, then the two approaches discussed disprove this view. Pluralism can introduce new theories and relativism can do so as well. It is not clear to me how pluralism is different in this respect. Of course, the aims of both positions might be different. This is also exemplified by the different VEPPs both positions embrace.

In this section, I have approached the relation of pluralism and relativism in two steps. I discussed how Baghramian, Chang, the Pluralist Stance, Dupré and Feyerabend envision the relation between pluralism and relativism. Baghramian, Chang, and the Pluralist Stance are quite explicit in rejecting relativism. They fault relativism for "renouncing judgment." "Renouncing judgment" disqualifies relativism for the pluralist. Baghramian and the Pluralist Stance worry that being a relativist pluralist entails accepting any theory. Chang localizes another problem with "renouncing judgment": relativism is far too passive for his taste. To authors like Dupré and Feyerabend, the relation of pluralism and relativism is less problematic. Dupré accepts certain forms of relativism. In Feyerabend's position, relativism and pluralism operate on different levels.

After having reviewed the arguments of five scientific pluralists, I proceeded to debunk the argument that relativism renounces judgment. I showed that epistemic relativism does not renounce judgment. I introduced two different strategies for how relativists could criticize periods of monism. In addition, I showed how relativists could introduce plurality. Case studies that are produced by the "historical" or the "unfolding approach" could, therefore, significantly contribute to the pluralist agenda.

In conclusion, this section shows that pluralism and relativism are two separate positions. None of them entails or implies the other necessarily. Some pluralists embrace relativism and the same is possible vice versa. Even without adopting both positions, pluralism could benefit from relativist case studies and vice versa. In the next section, I will review the issues of values, virtues, and other demarcation strategies more closely. I will argue that even if the pluralist manages to exclude ER at the level of theory choice, she cannot avoid relativism on the level of value choice. I will thus home in on my argument advanced in Chapter Five. I will demonstrate that all pluralist positions, at least if they invoke sets of values, have a "relativist touch."

6.3 Values, virtues, and norms

In the last section, I reviewed arguments on whether pluralism entails relativism. I concluded that particular versions of pluralism are also relativist. When this is the case, pluralism and relativism apply on different levels or fulfill different purposes. Although there are exceptions, most scientific pluralists maintain that SP does not entail ER. The most prominent argument in the literature is that relativism renounces judgment, whereas pluralism does not.

Central to the issue of renouncing judgment are the problems of demarcation and theory choice. Scientific pluralists are concerned that if they embrace ER, they would have to accept all theories as equal. Pluralists believe that ER entails an "anything-goes" attitude. This rests on the false assumption that ER implies the "equal validity" thesis of symmetry. With regard to demarcation, the problem of "equal validity" is also central. Scientific pluralists fear that if everything is equally valid, there might not be a distinction between science and non-science. They suspect that ER entails the belief that all approaches to solving a particular problem are equally good. By embracing ER, scientific pluralists would abandon the distinction between what is scientific and what is not. In this dissertation, I focus on the first issue, that of theory choice.

In general, one could reject both concerns right away. Both arguments rest on the assumption that ER entails "equal validity." In the last section, I demonstrated that relativist positions do not entail "equal validity." Therefore, one can reject the pluralists' arguments out of hand. Even though I have undercut both concerns already, let me narrow the focus and not presuppose my argument on equal validity. Without taking my assumption for granted, I will explain pluralist approaches to solve the problems of demarcation and theory choice.

Pluralists usually focus on the problem of theory choice rather than on demarcation. I submit that this is justified. If one deals with one of the two issues sufficiently, the other is not much of a problem either. Assume we have firm criteria for demarcation. In that case, only scientific theories will remain plausible candidates for theory choice. Thus, the problem of theory choice is radically reduced: robust criteria for demarcation will only leave a limited number of scientific theories in the running. Indeed, some pluralists would be entirely satisfied with this outcome. Something similar can be said for the opposite situation. Assume we have robust criteria for theory choice. Then it is unlikely that any pseudo-scientific theory would survive. As we shall see in the following sections, pluralists mainly focus on the issue for theory choice. I will do the same when reviewing their arguments on how to "keep out" relativism.

Chang's, Dupré's, and Feyerabend's are a case in point. The question of demarcation to them is obsolete or unproductive. Dupré opts for omitting the need to distinguish between scientific and non-scientific theories. Chang argues that as long as theories have non-trivial prior probabilities, one should admit them. Feyerabend emphasizes the importance of introducing "lunatic" theories in the course of counterinduction. But all the discussed authors, Chang, Dupré, and Feyerabend alike, provide ways of differentiating between good and bad theories: Dupré introduces his "virtue epistemology." Chang advocates for "benefits of tolerance" and "benefits of interaction." Feyerabend points to counterinduction as a strategy to increase the empirical content of theories. Thus, the "goodness" and "badness" of theories remain an issue, while demarcation is left out.

In this chapter, I will review the issue of theory choice in a specific light. I will look at how to execute theory choice in practice. This will tie in with the issue of relativism and my critique of Kellert, Longino, and Waters in particular. As argued before, many pluralists claim that they reject ER because they do not renounce judgment. But how does pluralism preserve judgment? The answer is: by the introduction of specific sets of norms, standards, virtues, or values for executing theory choice. In this section, I will argue that introducing values does not set pluralism apart from relativism. The introduction of values, instead, brings into view a further level on which relativism applies. That is, relativism applies to the level at which values are chosen.

There is another general point to make about the relativity of values. There is a circular relationship between values and theories. "Paradigm theories" are often used to distill a set of epistemic values. These values, in turn, describe which characteristics our best theories should have. Our preferred theories, thus, often act as "intuition pumps" (Dennett, 2013) for values. At the same time, we use the same values to justify or choose theories. Values generated by analyzing paradigm theories are used to evaluate new theories. Theories judged valuable, in turn, serve as "paradigm theories" to distill values from them. As a result, the relation of values and valuable theories is circular. Values are contingent and relative to paradigm theories available.

In the next sections, I will analyze the strategies for theory choice of four scientific pluralists: Chang, Longino, Dupré, and Feyerabend. I will mainly focus on their respective sets of values. Having reviewed their strategies, I will provide a general argument for why I believe that all of their approaches fail to set apart pluralism and relativism. Before introducing the pluralists' values, I will survey traditional epistemic values in order to compare them with values introduced by pluralists.

6.3.1 A short survey of traditional values

Kuhn lists five values that usually guide theory choice: (1K) "Accuracy:" "A theory should be accurate within its domain, that is, consequences deducible from a theory should be in demonstrated agreement with the results of existing experiments and observations." (2K) "Consistency:" A theory should be internally consistent and consistent with other currently accepted theories "applicable to related aspects of nature." (3K) "Scope:" A theory should extend its applicability and consequences far beyond the "particular observations, laws or sub-theories it was initially designed to explain." (Kuhn, 1977, 103). (4K) "Simplicity:" A theory should be simple, so it can connect theories that would otherwise be isolated or confused. (5K) "Fruitfulness:" A theory should be fruitful in that it reveals new phenomena or connects phenomena in new ways, or both. These five values, Kuhn argues, provide the shared basis of theory choice (ibid.).

In his book *Doubt Truth to be a Liar,* Graham Priest argues that consistency should not be a *sine qua non* of rationality. There are many irrational views, Priest argues, that are consistent. A theory should instead be judged by its virtues: (1P) "Adequacy:" A theory should be adequate to the data it handles. (2P) "Simplicity:" A theory should be "clean and elegant." (3P) "Unity:" For a theory, it is optimal to invoke the least ad hoc hypotheses as possible. (4P) "Explanatory power:" A theory should be able to explain other phenomena within its domain. (5P) "Parsimony:" A theory should not multiply entities "beyond necessity" (Priest, 2006, 123).

Chang provides a synthesis of Kuhn, van Fraassen, and the philosopher William Lycan. I will only explain those virtues which I have not listed before: (1C) accuracy; (2C) consistency; (3C) simplicity / elegance; (4C) scope / completeness; (5C) unifying power: to relate and synthesize as much theories as possible, reducing the overall number of theories; (6C) explanatory power; (7C) fruitfulness; (8C) testability: there are multiple ways of testing the theory most rigidly (one could also say, refutability); (9C) conservativeness: preserving as much prior beliefs as possible. "Empirical adequacy" is no value on its own but comprises accuracy, consistency, and scope (2012, 230).

With this collection of received values in mind, I will discuss values proposed by scientific pluralists.

6.3.2 Feminist values in Longino and the Pluralist Stance

Longino elaborates extensively on her pick of values to set apart pluralism and relativism. Therefore, reviewing the policy of the Pluralist Stance, along with Longino's proposed values, will be most illuminating for this section. Longino aims at a clear demarcation of pluralism and relativism. The main difference between these two positions, she argues, is that pluralists can pass negative judgment on "instances of bad research." To be precise, Longino's Pluralist Stance aims at keeping out two different sets of troublesome theories: "bad science" and theories that are dogmatically monist.

The Pluralist Stance involves two steps of decision-making. The first step of decision-making consists of excluding specific theories from being considered possible alternatives. The authors of the Pluralist Stance aim at keeping out theories that enforce a monist agenda. The second step concerns the "empirical commitment" of the Pluralist Stance: for every phenomenon, several theories should be introduced. As a next step, one has to assess whether plurality is beneficial. If this is the case, plurality prevails. If this is not the case, the field returns to monism. Values govern both decisions.

I agree with Chang's critique of the "empirical commitment" of the Pluralist Stance. Chang claims that it is hard to see under which circumstances Longino and her collaborators would conclude that plurality is not desirable (2012, 292). After having introduced several theories, one can immediately profit from the "benefits of tolerance" and the "benefits of interaction." If plurality disappears, this profit cannot be seized anymore. Because this is undesirable, one should never get rid of plurality.

I agree with Chang, though my reasoning is different. The problem lies in the fact that the proponents of the Pluralist Stance do not explain the use of epistemic values well. Do they use the same set of values when making two separate decisions: introduction of desirable theories and judging whether plurality is desirable? If this is the case, it is impossible to exclude theories after having introduced them. I will explain why. Assume that the scientific pluralist has selected several theories, based on the values she embraces. As a next step, she assesses the state of plurality. For some reason, she concludes that the resulting plurality is not desirable. Which theories would she exclude, if all of them are individually highly desirable, based on the values she embraces?

The proponents of the Pluralist Stance could solve this problem by proposing two different sets of values for the two different stages of decision-making. Yet, they do not propose this differentiation in the literature. This also points to another issue. There are two ways of assessing a theory. On the one hand, it is possible to assess whether a theory individually fulfills specific values. On the other hand, one can assess how different theories together fulfill specific values. To illustrate my concern, I will first introduce a set of values proposed by Longino and then use one of these values to home in on my argument.

Longino argues that

> "theoretical virtues or values are those qualities or properties of a theory, model or hypothesis that qualify it as at least praiseworthy, but also plausible, and even worthy of acceptance, or whose absence qualifies the theory for suspicion or rejection" (Longino, 2008, 69).

Longino lists five feminist values:

(1) "Empirical adequacy": "agreement of the observational claims of a theory with data" (ibid.). Received and feminist sets of values share empirical adequacy. Longino emphasizes that empirical adequacy is not sufficient for theory choice. One of the classical problems is the underdetermination of theory by data. Underdetermination suggests plurality: more than one theory might match the empirical data. To constrain underdetermination, both monists and pluralists need to resort to further values. Of course, the difference between the two is that the monist aims at ultimately picking one and only one theory, whereas the pluralist would be content with a less restrictive choice.

A second value is (2) "novelty" (2008, 70). Novelty allows breaking with established theoretical frameworks. Novelty is particularly important when it helps to choose theories that break with traditional sexist theories. Novelty can be in tension with some of the received values, particularly consistency and conservativeness. Conservativeness and consistency often perpetuate androcentrism in accepted theories and explanations (ibid.).

(3) "Ontological heterogeneity" (2008, 71) is another value. Rather than being a formal requirement, it specifies the content of a theory. In order to satisfy ontological heterogeneity, a theory needs to propose different kinds of entities and causal factors. If theories are ontologically homogenous, they might miss significant differences between causal agents. Feminists mainly criticize that males are often treated as "the only causally effective agents in a population" (ibid.). Ontological heterogeneity is in tension with several traditional values, mainly simplicity, parsimony, and unification.

The next value is (4) "complexity of interaction" (ibid.). The complexity of interaction defines how interactions between entities should be defined. Interactions should be represented as complex, mutual, and reciprocal (ibid.). Theories that do not reduce complex processes to simple causal structures are preferred. "Complexity of interaction" is in tension with simplicity, parsimony, and unification.

(5) "Pragmatic dimension of knowledge" (2008, 72) is the next value. Knowledge should be for people. For example, research that alleviates the needs of women should be preferred over research for military purposes or research for knowledge's sake only.

The final value is (6) "decentralization of power" (ibid.): "Forms of knowledge and its application in technologies that empower beneficiaries are preferred to those that produce or reproduce dependence relations" (ibid.). Both (5) and (6) "have to do with the expansion of a theoretical approach in an empirical tradition" (ibid.). They are in tension with fruitfulness and testability. "Decentralization of power" and "pragmatic dimension of knowledge" solve social and practical challenges outside of the research context.

Longino argues that the traditional values that I surveyed in the first section are not useless. But there are situations in which these values clash with the values she suggests (ibid.). In such cases, theories that match her proposed values should be preferred. In the following, I will consider the implications of her proposed values for the Pluralist Stance.

I argued above that implementing the Pluralist Stance's agenda requires two ensuing value judgments. First, for a specific research question, one has to introduce several theories. These theories are selected based on Longino's proposed values. Because of the Pluralist Stance's empirical commitment, theories that are present but probably support monism or fail with regards to other proposed values also need to be accounted for. In the end, their position is "empirical" in the sense that they make case studies central to their approach. This poses a question regarding Longino's proposed values. Are they used to evaluate theories individually or do they evaluate the plurality several (probably individually monist) theories produce?

To illustrate what I mean with this question, I will take the value "ontological heterogeneity" as an example. There are, in principle, two levels on which this value could apply. On the one hand, it could be used to evaluate individual theories: a theory should propose several heterogeneous causal agents. On the other hand, it could also apply to the meta-level. Several theories might propose singular causal agents, yet, taken together, they produce ontological heterogeneity. In her writing, Longino instead focuses on evaluating each theory individually. As a scientific pluralist, she should, however, also consider "ontological heterogeneity" on the meta-level.

If ontological heterogeneity on the meta-level is sufficient, then no individual theory should be criticized for being ontologically homogenous. Consider a research field that displays a plurality of ten different theories. Each proposes only one type of causal entity. These ten theories are ontologically homogenous on the individual level. Ontological heterogeneity is still the status of the research field. As a result, the pluralist would have no reason to exclude "monist" theories as long as they contribute to "ontological heterogeneity" on the meta-level. There remains only one possible normative assessment against a monist theory: so-called "predatory theories" or theories proposed by researchers that want to monopolize the field can be excluded, based on Longino's proposed values.

The Pluralist Stance, thus, need not be necessarily concerned about "monist" theories, that is, theories that only propose one entity, for example. Instead, the Pluralist Stance needs only to be concerned about monists, that is, researchers that push a monist agenda. A differentiation between pluralism or monism as a position, plurality, and monity as characteristics of a discipline, as well as monity and plurality as attributes of a specific approach, is, up to now, not achieved in the literature.

I will conclude this section by making one remark about values and their relativity. Longino's proposed values are a case in point for a set of values that might not be embraced by all philosophers of science or scientists in general. Some philosophers might insist on the superiority of the "traditional" values and prefer their realization over Longino's proposed values. Longino's pick of values is, of course, conditioned by a specific agenda, that agenda being eradicating inequalities in science. There are, thus, principles that guide value choice. But who decides on these principles? Is there a non-circular way to motivate value-choice? In my opinion, the criteria based on which to choose values already presuppose the chosen values. There is no neutral way of choosing between those criteria. I will come back to this issue later in this chapter.

6.3.3 Dupré's virtue epistemology

As pointed out in Chapter Five, Dupré does not uphold the demarcation of "science" and "non-science." Instead, he is interested in whether a particular approach is "good" or "bad." This sentiment can be illustrated with his example of classifications of onion and garlic. Relative to a specific purpose (for example, cooking), the culinary classification is better than the genetic classification.

As a result, "pseudo-science" should not be excluded based on a demarcation criterion. Instead, "pseudo-science" should be excluded because it is not "good." For example, Dupré insists that there must be something to say about why evolutionary theory is better than creationism. As a consequence, Dupré proposes a toolbox to judge which theories are "good" or "bad": "virtue epistemology." A theory that does not fulfill the proposed virtues can be excluded, not because it is unscientific but because it is a bad theory.

To judge whether a theory is good or bad, Dupré proposes several virtues: sensitivity to empirical fact, plausible background assumptions, coherence with other things we know, exposure to criticism from the most extensive variety of sources, amongst others. It is interesting to note that Dupré's values resemble traditional ones. Notably, they recall the received values of testability, coherence, and empirical adequacy.

As a consequence of Dupre's "promiscuous realism," the "goodness" or "badness" of a theory is always purpose-dependent. It follows that the same purpose-dependence must apply to the level of his virtue epistemology. Depending on specific purposes, different sets of virtues are relevant. Dupré's notion of family resemblance and scientific theories comes in helpful. Necessary and sufficient conditions cannot define scientific theories. To Dupre, "scientific theory" is a family-resemblance concept; not all theories have to satisfy the same criteria. If there is a list of all possible virtues, various theories might fulfill some of them and not others. This underscores the importance of purpose dependence. Different purposes might require different values. Specifying the purpose limits the choice of theories to those that incorporate a selection of values. In conclusion, the purpose-dependence of value choice highlights the relativist element on the level of value choice.

6.3.4 Reformulating Chang's benefits as meta-theoretical values

Chang does not propose a set of values but introduces "benefits of tolerance" and "benefits of interaction" to assess theories. In this section, I will elaborate on the differences between Chang's strategy and traditional value-based strategies. I will argue that the primary difference is that evaluation does not operate on the level of the individual theory but the level of the research field. I will start by reviewing Chang's argument against using values for theory choice.

Chang opines that values should not guide theory choice. He feels uncomfortable about value hierarchies, especially between different sets of values. Chang argues that

> "The discomfort of a strict hierarchy of values between empirical adequacy and the pragmatic virtues becomes clear when we ask whether a tiny advantage in empirical adequacy would not be outweighed by a great deal of additional simplicity or other virtues." (2012, 26f).

Chang motivates his concern about value-hierarchies with his emphasis on "systems of practice." Systems of practice do not only differ in theory commitment or specific problems. Researchers within different systems of practice might embrace different values or prioritize values differently. In each system of practice, values define the solutions to the problems deemed relevant (2012, 27).

Choosing between different systems of practice means to restrict one's choice of values. Chang's analysis of the oxygenist and the phlogistonist system provides a good example. Oxygenists emphasized the value of novelty, while phlogistonists emphasized the value of conservationism. Oxygenists, in addition, valued simplicity, while phlogistonists valued completeness. To choose one system of practice over the other reduces the number of overall values. To overcome this problem, Chang introduces pluralism as an antidote. Pluralism helps to preserve as many values as possible. Keeping many SPs around maximizes the amount of theories and values that can be seized.

Chang's strategy involves two steps for assessing theories. Chang proposes non-trivial Bayesian prior probabilities as the first step of theory choice. The second step consists of narrowing down the choice of possible theories. But how to narrow down the choice? The "benefits of toleration" and "benefits of interaction" help narrow down theory choice. A theory can be assessed regarding how many and which benefits it realizes. It is important to emphasize that both the "benefits of toleration" and the "benefits of interaction" operate on the level of the research field. They describe how a specific theory will contribute to a research field. Theories might realize these benefits differently. In what follows, I will provide an example.

Assume that "Darwinism," "Lamarckism," and "Creationism" are three distinct systems of practice. As of now, Darwinism and Lamarckism have more in common than creationism has in common with either of them. How would these systems of practice seize Chang's benefits? For the sake of simplicity, I will consider evolutionary theory as one research field. Evolutionary theory might profit from Darwinism and Lamarckism through the benefits of interaction. As I showed in my case study on small RNA inheritance, small RNA researchers who propose Lamarckian interpretations interact and compete with researchers who emphasize the exclusiveness of Neo-Darwinism. They also sometimes provide explanations that provide "ad hoc" integrations of both accounts to provide a concrete explanation. Darwinism and Creationism could realize the benefits of tolerance. "Hedging one's bet" is a benefit of toleration Chang proposes. If – for some reason – we had to admit that Darwinism is entirely wrong, we could take Creationism as a starting point in order to maintain the field's inquiry. The point I want to make here is that some systems of practice are closer related than others. Which "benefits" can be seized depends on this relation. Of course, I do not believe that this relationship is caused by the "nature" of specific mechanisms or entities that are proposed in different systems of practice. I instead believe that this is dependent on the respective research traditions. Nevertheless, the historically dependent constitution and relation of particular fields of research define which benefits can be realized. But as long as a system of practice realizes one benefit, it should be valued.

This discussion leads directly to the central point I want to make about Chang's approach. The decisive criterion by which to assess systems of practice is whether they contribute to the research field as a whole. This is an empirical and historically contingent question. Chang might want to exclude specific systems prima facie nevertheless. If a system of practice is too dogmatically monist, it might endanger the whole research field. Therefore, it is necessary to assess whether tolerance or interaction between systems of practice is possible in principle. Here it is again important to consider scientists as part of this equation. There might be several reasons why one would not want to interact with a particular system of practice. Some of such reasons might be "narrow," and others might be "broad." These reasons also change throughout time, depending on how research traditions develop.

To conclude my discussion on the "benefits of interaction" and "benefits of toleration," I propose that Chang introduce a new value that operates on the level of the research field only. I call this value "meta-fruitfulness." Kuhn defines fruitfulness as (5K) "fruitfulness": a theory should be fruitful in that it reveals new phenomena or connects phenomena in new ways, or both. I define meta-fruitfulness as

> Meta-fruitfulness: A theory should be fruitful in that it contributes to the research field by interacting with other theories with the outcome of ad hoc integration, co-optation, competition; or by tolerating other theories within the field with the outcome of hedging one's bets, division of the domain, satisfaction of different aims, multiple satisfaction.

When relying on meta-fruitfulness, a theory gets judged on the level of the research field exclusively. Chang does not reject epistemic values entirely but only rejects values that operate on the level of the individual theory. This particular approach differentiates Chang from other pluralists.

6.3.5 Feyerabend's standards

Feyerabend admits the importance of standards but stresses that no standard is absolute. He emphasizes the role of standards throughout the history of science. Feyerabend does not suggest a specific set of values. As new instruments have to be developed to make sense of new situations, new values are developed to accommodate these situations. (1975/2010, 237). Nevertheless, in the spirit of epistemological anarchism, every standard can be worth undercutting. Although Feyerabend accepts that all rules have their limits, rules are not worthless. (1975/2010, 242). There is no exceptionless methodology. In conclusion, although Feyerabend elaborates on the importance of standards, he does not propose a normative program of how standards should guide the problem of theory choice.

It is important to note that the scope of Feyerabend's standards is quite broad. Feyerabend argues that concepts such as "truth," "clarity," and "objectivity" are contingent standards as well. In order to secure the progress of science, there should not be any limitation on which standard to accept. A psychological or even pathological longing for truth, clarity, or objectivity often limits the range of possible standards (1975/2010, 12). One should not assess standards by whether they are rational or reasonable. Feyerabend claims: "The one man's 'reason' is the other mar man's insanity" (1975/2010, 165). Standards must never "bend minds." The world can be captured in many different ways. Standards that grant freedom for one way will severely constrain the other. The danger of absolute standards is that people disobeying those standards get robbed of their reason. Thus, it should not come as a surprise that "anything goes" is no "absolute" methodology. If the world is proven to be finite, "anything goes" becomes an unappealing methodology for approaching it.

Feyerabend's program of "counterinduction" is built on similar premises as Chang's "meta-fruitfulness." By breaking the rules and standards of a research field, one can introduce new theories. The optimal outcome of counterinduction is to increase the empirical content of theories. This is also true for the meta-level. Through counterinduction, more theories are introduced, and thus the field as a whole increases its empirical content.

Having reviewed and criticized four scientific pluralists and the role of values within their positions, I will review values as a strategy to govern theory choice in general.

6.3.6 The relativity of scientific values

In previous sections, I reviewed four approaches towards theory choice. Chang, Feyerabend, Longino, and Dupré emphasize the importance of standards. Yet, standards and values have different purposes within these positions. Generally, one can differentiate between two different objectives of values and standards.

Chang and Feyerabend are representatives of one objective. Both acknowledge values as pivotal for governing decision making within research fields. Neither Feyerabend nor Chang propose a specific set of values for theory choice or to "keep the looneys out." Instead, they assess theories on the level of the research field. That is, they judge theories by the contribution they make to the field as a whole. Theories are valuable if they make available new facts, improve another theory, or are beneficial for the field in any other sense.

Dupré and Longino are representatives of a different approach regarding values. Values have a central role within their positions. Both propose a set of values to govern theory choice. There is one crucial difference. While Dupré's values resonate with the traditional epistemic values, Longino's values are heterodox but also aim at improving knowledge. As Longino pointed out, there are numerous instances in which feminist values clash with traditional values. In such situations, one has to decide which set of values to give preference. If pluralism is to be considered as a value of its own, this is also the case. Pluralism might clash with values such as simplicity and scope. Admittedly, most epistemic values are highly desirable. Yet one's personal preferences and agendas define which values one gives priority. In the remainder of this section, I will discuss whether values shield SP from ER, given the fact that value choice is purpose-dependent. I will show that this is not the case.

I will initiate this discussion by reviewing Cartwright's analysis of relativism in the philosophy of science. Cartwright argues that science is necessarily relative to several concepts. The most common theme is to relativize science to epistemic, moral, political, and aesthetic values. Relativizing science to epistemic values "is not pernicious… because they conduce to knowledge or truth" (Cartwright, 2010, 90f). Cartwright, however, judges epistemic values to be problematic. Epistemic values are vague and cannot provide real grounds for choice. They might not point in the same direction. They do not narrow down options to a single one (2010, 91). Cartwright argues that scientific acceptance is always relative but not in any singular way. If scientific theories are not relative, then scientific values are. The question of who decides on these values is ever more pressing (2010, 97).

I agree with Cartwright's analysis. Yet I submit that some problems are more pressing than others. For instance, I do not think that values should narrow down the choice to one theory only. For a pluralist, this might not be desirable at all. There are three problems of values: the relativity of value choice, the circular relation of values and theories, and the question of the level at which values apply. I will conclude that pluralism cannot be cleared of relativism by introducing values that guide theory choice because values introduce a new level at which relativism applies.

First, I will discuss the relativity of value choice. My review of Longino, Dupré, Chang, and Feyerabend demonstrates the heterogeneity of sets of values selected by scientific pluralists. In some cases, these sets of values might also be in tension, given the fact that they might privilege different theories. Apart from values proposed by scientific pluralists, other sets of values are undoubtedly imaginable. For instance, philosophers and researchers that favor monism might put forward different values. Values that enable "self-enclosed science," that is, science just for knowledge's sake, might be shared amongst philosophers or researchers. Acknowledging this variety of values invites specific questions: What defines which values are to be adopted? How are values ranked? Does the choice of values happen randomly?

Value choice does not happen randomly. Additional commitments motivate value choice. In section 6.1, I discussed the idea of the "stance." The adoption of specific VEPPs determines "stances" One obvious way that informs choosing and ranking values is to refer to VEPPs in that process. Let us take feminist epistemology as an example. If feminist epistemology is to be understood as a rebellion against sexist theories, then their choice of values would be part of the policy to change or exclude sexist theories. A similar argument can be made about pluralism as a stance. If pluralism is a rebellion against monism, then the choice of values would be part of the policy to challenge monity and monism in science. Therefore, value choice does not happen randomly but is defined by an overarching set of commitments. For instance, it could be explained by the adoption of a specific stance and a commitment to respective VEPPs.

The second issue is theory relativity. As pointed out at the beginning of the chapter, values are often developed with an emphasis on what we regard as exceptionally good theories. Values describe what distinguishes our best theories. At the same time, values are a tool to criticize or exclude theories. Values are formulated in response to dominant theories. They are thus informed by either a) their admiration of a particular dominant theory or b) their rejection of a particular dominant theory. Values and valued theories are thus in a circular relation.

The last issue is the question on which level values apply. Some values assess theories on an individual level. Other values operate on the level of the research field, assessing a theory for how it contributes to realizing a particular value for the field as a whole. In the previous section, I exemplified this differentiation with the value of "ontological heterogeneity." Some values are more apt for the one or the other level, yet others can apply on both levels. Values that assess theories on the level of the research field might be more relevant for pluralists than such that assess an individual theory. I will provide two reasons for that assertion. On the one hand, certain benefits might only be attainable on the level of the field. For instance, Chang's benefits of interaction presuppose that there are different approaches. There is no interaction if there is only one approach. On the other hand, pluralism on the level of the individual theory might not be practical. Requiring values to operate on the level of the research field might be more realistic. If pluralism is a policy to change science, it seems more feasible to demand the introduction of more theories, rather than to demand individual scientists to change their theories to include, for example, more theoretical entities. There might be some situations where both options are possible. In most cases, I suspect the benefits of pluralism can be satisfyingly realized through the level of the field.

I will conclude this section by addressing the problems raised by Cartwright and then coming to a general conclusion. Let me start with answers to Cartwright's concerns. It does not pose a problem for the pluralist if values cannot narrow down a single theory. Although it might be true that values do not point in the same direction, this should not come as a surprise given the relativity of values discussed in previous paragraphs. I do not agree that values cannot provide real grounds for choice because they are vague. If values are one component of the VEPPs that constitute a stance, values need not do the work on their own. They are, nevertheless, a decisive part of the decision process, and in combination with other commitments, choice is undoubtedly possible. Concluding this point by a reference to VEPPs and stances also brings me back to my general point, the intrinsic relativity of values. Assume an advocate of a particular stance commits to specific values, yet a proponent of another stance commits to others. Even though the first advocate might be able to reconstruct the choices of the second, this does not provide grounds for adopting the second advocate's values. In conclusion, values are a crucial tool for theory choice. As there is an intrinsic relativity to values, they do not exorcise ER.

In this chapter, I argued against a demarcation of SP and ER. Although some versions of SP or ER are not compatible, it is possible to combine both positions. I explored options for relativist programs that might contribute to a pluralist agenda. I demonstrated that most arguments offered by card-carrying pluralists hinge on a straw-man argument against relativism. I showed that the most common strategy of scientific pluralists to demarcate pluralism from relativism fails. By introducing values to eradicate relativity on the level of theory choice, they introduce relativism on the level of value choice. In conclusion, pluralism does not entail relativism. But pluralism and relativism are programs that share several commitments, and thus, a combination of both is not only possible but beneficial.

7 Conclusion

In the previous two chapters, I have reviewed several issues about scientific pluralism in general and its relation to epistemic relativism in particular. In the following chapter, I will conclude my thesis by synthesizing issues discussed throughout this dissertation. Given my empirical results regarding the case of pluralism and plurality in small RNA inheritance and considering the issues regarding pluralism and relativism that I discussed in the second part of the dissertation, I will develop my pluralist position with regards to small RNA inheritance. My pluralist position will be a unique contribution to the scientific pluralism literature, in that it is based on an empirical, sociological case study that scrutinized the research field before coming to any normative or descriptive conclusions.

In Chapter Four, I assessed whether small RNA inheritance researchers are pluralists. In so doing, I described the dynamics of a field that displays plurality. In this chapter, it is my task to formulate a pluralist position that takes into account the results of the case study as well as the theoretical considerations regarding SP. I intend to formulate a position that addresses and acknowledges scientists' aims and goals. I will make several claims. First, I will emphasize the importance of scientific practice as a source of plurality. Second, I will argue against the necessity of integration. Integration is not the only way of accommodating different approaches. I will discuss the benefits of avoiding integration for the specific case of small RNA inheritance. Third, I will examine the promise of pluralism in small RNA biology. I will argue for pluralism while acknowledging the fact that the actors are concerned about the stability of their field and regard plurality as a source of instability. In so doing, I will show how the prescriptive claims formulated in this section can benefit researchers. Fourth, I will consider some implications of my approach. Specifically, I will ask whether my pluralism is a version of SP that accepts ER.

The first section will rely strictly on the empirical data provided in Chapter Four. A practice-oriented pluralist position will be developed. In the second part, I will amalgamate pluralism observed in the field with issues raised in the philosophy of science, precisely the issue of (theory) integration. I will introduce "theory presentation" (Love, 2013) as a conceptual tool. In the third section, I will formulate a normative pluralist position concerning small

RNA inheritance that contributes to the field and addresses the aims and goals of researchers. Next, I will compare my pluralist position with the positions of other scientific pluralists who emphasize the importance of scientific practice. As a conclusion, I will synthesize my pluralist position with issues discussed in Chapter Six. Specifically, I will show that my position is relativist and normative at the same time.

7.1 Pluralism beyond theories

In the last section of the third chapter, I proposed several possible pluralist interpretations for small RNA inheritance. I discussed mechanistic, explanatory, and theoretical plurality. I listed epistemological benefits of adopting a pluralist position concerning small RNA inheritance and pleaded for a particular emphasis on plurality of theories and a "Lamarckian" interpretation of small RNA inheritance. In this scenario, small RNA inheritance creates plurality because it necessitates the introduction of another framework. This framework is needed to account for all phenomena of inheritance and evolution.

As discussed in Chapter Four, interviewees switched the "subject" of pluralism quite often. Small RNA inheritance researchers de-emphasized the importance of "theory" in their considerations. Some explained that they "come from mechanism" and are not concerned with theory. In the last sections of Chapter Four, I concluded that discourse around "theory" serves as a mode of representation to the outside. As a result, formulating a pluralist position for small RNA inheritance researchers which focuses on theory might not address researchers' aims and goals. Scientific practice needs to play an important role when formulating a pluralist position about small RNA inheritance. I will approach this goal in two consecutive steps. First, I will identify the research phase in which a plurality of repertoires turns into a plurality of theories. Second, I will emphasize the importance of repertoires concerning the stability of plurality-supporting claims.

Many interviewees associated pluralism with aspects of scientific practice. Pluralism was considered a virtue when planning or performing experiments, interpreting results, and hypothesizing. Maintaining plurality during the research process was considered an epistemic virtue. By performing specific epistemic activities, researchers foster plurality. Pluralism becomes almost prescriptive at this stage. It is, however, vital to keep in mind that some interviewees are "epistemic heuristic pluralists." They believe that there comes a stage in which pluralism is not desirable anymore.

Even though pluralism is an almost natural component of several epistemic activities, there are stages when pluralism is not expected anymore, and even discouraged. For those researchers who expressed monist commitments or are epistemic heuristic pluralists, there comes the point when plurality should turn into monity. There are also points in which institutional requirements demand the erasure of plurality. Interviewees pointed out that in several stages of scientific practice, for example, writing a paper, the plurality that is omnipresent during experimenting, theorizing, and interpreting needs to disappear.

There is no intrinsic necessity of erasing plurality in scientific papers. That is, there is no official guideline in journals that prohibits displaying plurality or expressing pluralist attitudes. Presenting research as monist is, however, perceived as a standard, a necessary standard for publishing. Exposing plurality (of, e.g., repertoires) might be sanctioned by gatekeepers such as editors and reviewers. Thus, when writing a scientific paper, a plurality that is specific to scientific practice vanishes. Instead, a different kind of plurality emerges. In the case of small RNA inheritance, this is the plurality of theories. Speculations about Lamarckian interpretations are part of small RNA inheritance research papers. What can we make of this transition?

Different aspects of plurality which are omnipresent in scientific practice get erased when presenting research to an audience. But plurality does not vanish entirely. It turns up on another level, that of theoretical pluralism. Experiments presented in scientific publications were not designed to support a specific type of pluralism. They were not designed as experiments to verify or falsify theory pluralism[48]. Instead, following Lynch's interpretation,

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[48] In *Art and Artifact in Laboratory Science*, ethnomethodologist Michael Lynch observes that: "treating the contingent productions of scientific work as evidence for principled epistemological positions and arguments holds the work accountable to issues and questions which are not demonstrably part of the practical setting in an invariant fashion." (Lynch, 1985, 291). While I do not agree that there is no epistemology involved in laboratory practice and the presentation of research, I agree that researchers do not design experiment to confirm a certain epistemological commitment.

these experiments were designed in a way that they can support theoretical pluralism post hoc. This marks the transition of the plurality of repertoires to the plurality of theories.

In Chapter Four, I demonstrated that "repertoires" have a particular bearing on plurality in scientific practice. MOs, sets of expertise, and the history of research traditions are sources of plurality. After having examined how the plurality of repertoires transforms into a plurality of theories, I will demonstrate how repertoires are decisive for the perceived stability and acceptance of claims about small RNA inheritance.

Several factors determine the relative stability of a field that displays plurality. One decisive aspect is how well the constituents of repertoires are aligned. How well a field can accommodate plurality depends on established model organisms, accepted techniques, modes of data sharing, the founding history of the field, relations of practitioners within the field, and the structure of centralization (Ankeny & Leonelli, 2016).

To exemplify this assertion, let me compare the relative stability of claims about small RNA inheritance in mouse vs. plant model organisms. Experimenters in both fields utter the same or very similar claims: evidence supports the notion that small RNA inheritance is an alternative form of inheritance. This claim is not upsetting at all for researchers working with, for example, *Arabidopsis thaliana*. When reviewing the literature, such claims asserted within the context of plant research do not receive noticeable criticism from the outside. This is entirely different in the mouse field. Repertoires explain this situation: alignment in the plant field is much more successful than in the mouse field.

The mouse MO repertoire has a unique standing. Its representational scope covers humans and its representational target is human-associated physiological problems. As a result, claims about mouse MOs enter dangerous territory because they are of substantial socio-political impact. Evidence on small RNA inheritance produced in mouse MOs cannot be accommodated by classifying it as "weird mouse things." The representational target of mouse MO research is human physiology. In contrast, findings in plants on small RNA inheritance are very often labeled "weird plant things." Such findings might, however, concern a much more extensive range of organisms. Even if the effects observed in mice were limited to mammals, they would nevertheless be considered as tremendously important. Anthropocentrism is omnipresent in biological research.

In Chapter Two, I explained that repertoires also account for social and political tensions like those discussed in the previous paragraph. This is what differentiates them from the Changian concept of “systems of practice.” These social and political tensions add up to the problems of alignment within the mouse MO. Ankeny and Leonelli argue that because mouse research is primarily application-driven, successful alignment of repertoire components remains impossible. In conclusion, comparing the same type of claim uttered for different MOs shows an essential feature of scientific pluralism in practice. The stability of a research field that exposes plurality might be highly dependent on its specific repertoires and the successful alignment of its parts.

Investigating plurality and pluralism in a research field and formulating pluralist positions requires diligent attention to the role of repertoires. While there might be significant changes concerning repertoires in a research field, theories might remain quite stable. As I have argued in Chapter Four, small RNA inheritance researchers use “theory” as a means of representing their field to the outside, while they identify repertoires as sources of plurality within the field. While different groups of researchers might present similar claims, their respective repertoires determine the success and stability of the claim.

If philosophers of science, or scientific pluralists in particular, focus on theories and theories change when they are studying research fields, they might miss key episodes of plurality. At least in my case study, repertoires have proven to be a much more common source of plurality than any abstract concept, such as a plurality of theories, mechanisms, or explanations. Scientific pluralists need to be prepared to find disputes about theory on a somewhat rhetorical level. Theories, although part of the discourse, are primarily a means for RNAi inheritance researchers to frame the dispute. Plurality of theories is used as an interpretation of small RNA inheritance experiments after experimentation at the stage of presenting research. A philosopher who is a pluralist regarding small RNA inheritance needs, thus, to be a pluralist about repertoires and research traditions. Theoretical or explanatory pluralisms play a role on a different level. In the next section, I will cover the issues of theoretical and explanatory pluralism for small RNA inheritance as a means of representation to the outside. I will identify a "need for integration" of different approaches as a pressure to the field. Alleviating this need by integrating small RNA- and DNA-based approaches is only at first glance a means to alleviate this pressure. Consequences are, however, detrimental.

7.2 Integration – vice or virtue?

In this section, I will investigate the promise of integration for small RNA inheritance. I will prime this discussion with the philosopher of biology Alan Love's work on "theory presentation." Love emphasizes the purpose-dependence of different ways of presenting a theory. Animosities in the Modern Synthesis (MS) vs. the Extended Evolutionary Synthesis (EES) can be explained as clashes of different forms of theory presentation. I will take Love's elaborations as a template and apply them to the debates centering around small RNA inheritance. I will discuss the promise of integration for the "molecular" and the "evolutionary" level. I will conclude that because of different explanatory goals and foci of both approaches, integration should be discouraged. In the section to follow, I will discuss how this conclusion can contribute to the field of small RNA inheritance.

In his 2013 paper, Love introduces the proverb "theory is as theory does." We should characterize theories "in terms of their activities rather than their advertisements" (2013, 325). We should understand the structure of theories "in light of how they are used or how they behave." Love acknowledges a recent "frustration" with theory-centered approaches because these accounts do not resonate with scientists' epistemic activities. "Theory presentation" is a way of bridging scientific practice with abstract ideas on the concept of "theory."

Love assesses evolutionary theory and the debate surrounding the EES. "Because the goals organizing and regulating scientific activity are heterogeneous, the same theory can be structured differently and sometimes incompatibly" (2013, 326). Love introduces the term "theory presentation," which describes the idealizations of theories. Structures of theories are determined by heterogeneous goals that derive from scientific practice. Several presentations of one theory are possible, with no universal theory structure in the background (ibid.).

Love remarks that opponents and proponents of the MS treat evolutionary theory as a cohesive unit. But "evolutionary theory plays different roles in response to a variety of different kinds of goals; different "structures" are adopted to achieve specific goals in various combinations." (2013, 331). "Theory structure will differ when the goal is not to test or confirm hypotheses contained within a theory but rather to explore domains and limn the contours of living systems that might be germane to a theory" (2013, 332). This is the case for the versions of evolutionary theory that the EES and the MS provide, respectively.

Love lists several methodological goals that require different theory structures:

> "(1) Articulating relationships among different scientific disciplines;
>
> (2) Facilitating ongoing research;
>
> (3) Presenting the theory in pedagogical contexts for others to assimilate;
>
> (4) Comprehending the theory's historical development;
>
> (5) Providing a means to respond to challenges against the theory;" (ibid.).

Theory presentation is the "partial but structured display of theory elements that has advantages in fulfilling some set of goals." (ibid.). It reflects how theories are used in practice. Structural peculiarities of theory presentations reflect what scientists do with the theory. Presenting a theory is an epistemic activity.

Different theory presentations are not necessarily rivals. Whether they are rivals or not depends on how they align concerning their goals (2013,333). "Preferred idealizations (theory presentations) metamorphose to become central or the 'core' of a theory from the perspective of particular research communities." (ibid.). According to Love, this is precisely the reason for the animosities in EES vs. MS debates. Exposing forgotten idealization choices helps to showcase that these choices were epistemologically and not metaphysically motivated. By arguing that these choices are not "metaphysically motivated," Love maintains that theory presentations do not omit certain factors because they are causally irrelevant or do not exist at all. Instead, their inclusion did not fit the fulfillment of a specific goal (ibid.).

Theory presentations are similar to models. And theory presentations are used by practitioners as models. As we use different models to represent different aspects of a phenomenon, we use different theory presentations to showcase a heterogeneous theory. Love concludes that it is not necessary to invoke Kuhnian paradigms or hyper-theories in order to understand the debates surrounding the EES. Different presentations are incompatible idealizations and thus generate these debates. There is no common core to all theory presentations. Having no common core should, however, not be unsettling, says Love, unless one is an epistemological monist. Love criticizes epistemological monism for its focus on what is possible in principle or what might be possible in the future. Through this focus, epistemological monism neglects what scientists do in practice and how they use theories (2013, 335).

Love's account of theory presentation prompts a critical question: given the purpose-dependence of different forms of theory presentation, is theory integration valuable at all? According to Love, integration might not be a good goal. Idealizations are based on different epistemic goals and their integration might thus be pointless. Cues from scientific practice shape Love's argument. He inquires what researchers aim at doing with a theory. The purpose sensitivity of each theory presentation is thus a focal point in his deliberations. The incommensurability of different theory presentations is an unintended result of how researchers "do a theory."

In Chapter Four, I demonstrated that small RNA researchers agonized over the question of how to relate approaches. For example, interviewees were quite concerned about pluralism if identified with the exclusiveness of different approaches or an "either-or" version. The reasons for this concern were worries about orthodoxy and becoming "too nutty." Concerns were also tied to their belief that on the molecular level, both trajectories coexist. Because of this "molecular coexistence," "either-or" seems counterintuitive to them. In scientific practice, it is unproblematic to accommodate both ideas when planning, executing, and interpreting experiments. The prescriptive content of Love's position might thus not resonate with researchers' aims and goals because of two reasons. First, presenting small RNA inheritance as an "alternative" might harm the status of the field. Second, practitioners do not consider small RNA inheritance an “alternative” because, on a molecular level, it coexists with DNA-based inheritance.

In conclusion, the integration of small RNA inheritance with more traditional modes of inheritance appears to be a goal for small RNA inheritance researchers. Integration might not be a good goal, says Alan Love. In what follows, I will assess whether discouraging integration should be part of my normative pluralist position.

Let me commence this discussion with one observation of Love:

> “epigenetic inheritance has differential relative significance in mechanistic molecular science versus quantitative dynamical evolutionary science due to the different roles it plays in theorizing (Griesemer 2011). The different roles emerge from different goals these research communities adhere to and therefore lead to different stakes for whether to include epigenetic inheritance within their theory structures” (2013, 328).

This observation is essential for the question of integration. The question of whether small RNA inheritance and DNA inheritance are integratable has two parts. Answers for “mechanistic molecular science” and evolutionary science need to be treated separately. This also corresponds to empirical results. Interviewees describe scientific practice as a place in which small RNA inheritance and more traditional forms of inheritance go together. This is not the case for controversies concerning evolutionary theory. Explanatory goals of “mechanistic molecular science” are different from those of evolutionary biology and require separate treatment.

There are, however, also similarities between these two levels. Proponents of small RNA inheritance uphold for both levels:

1) This process is happening;

2) This process is important.

Different types of opponents challenge either 1) or 2). Opponents advance similar arguments on the molecular as well as on the evolutionary level. On the molecular level, they argue that small RNA inheritance can be explained by the fact that DNA encodes small RNAs and their effector proteins. Thus no additional account on top of a DNA-based account is required. On the evolutionary level, opponents argue that small RNA inheritance can be explained by the fact that it evolved through natural selection of random mutations. No additional account on top of Neo-Darwinism is required.

In the remainder of this section, I will show why integrating small RNA inheritance with existing theories, approaches, and explanations is not beneficial. I will advance arguments for the molecular mechanistic as well as for the evolutionary level. My argument is confined to the case study and not a general argument against integration. I take it nevertheless as a "proof of principle" that "integration" should not be regarded as a general or compulsory goal for pluralism.

I will argue that on the molecular mechanistic and the evolutionary level, explanatory goals of small RNA inheritance are different from those of "DNA-based" and "Neo-Darwinist" accounts. Integrating small RNA inheritance with more traditional trajectories masks peculiarities and relocates explanatory focus. While providing an account that sympathizes with SP in general, I argue for a version of pluralism that is not integrative. Integration is not desirable, neither on the molecular mechanistic nor the evolutionary level.

I will start with the molecular mechanistic level and then move "up" to the evolutionary biological. Let me first examine the explanatory goal of accounting for small RNA inheritance phenomena. What researchers want to know when studying small RNA inheritance is the following: how is an adaptive trait, acquired in a directed manner in generation F_0, maintained in generations $F_{0+(1-n)}$? As "small RNA inheritance" specifies, the goal is to explain this maintenance by referring to specific properties of small RNAs.

Small RNA inheritance researchers observe that small RNAs "persist" throughout generations. "Transmission" mechanisms are not of primary importance. Proposals of how a specific small RNA is synthesized in one generation, maintained and packaged, sent to the germline, inherited, and spread through the soma of the next generation (in other words, how it is transmitted) are not a primary explanatory focus. Thus, an emphasis on mechanistic transmission, which is central in DNA-based explanations, is absent in small RNA-based explanations. “Persistence” rather than “transmission” is the explanatory target.

Besides deemphasizing mechanistic transmission, there is yet another striking difference. To explicate this difference, I need to come back to some of the mechanistic details of small RNA inheritance that I discussed in Chapter Two. Metazoans consist of many cells. Even C. elegans consists of more than 1000 cells. An environmental stimulus is a stimulus to somatic tissues and not to the germline. Small RNA inheritance is responsive to such environmental stimuli. For example, there are changes in relative concentrations of heritable small RNAs. The response to an environmental stimulus causes changes in pools of small RNAs. This change does not happen in one somatic cell but a significant number of somatic cells.

“Transmission” of causally relevant small RNAs would require them to reach the germline, be transmitted through the germline, and then cause a phenotype in the next generation. This poses an insurmountable “administrative” problem. It does not matter whether one several, or a pool of small RNAs are causally relevant for a specific phenotype. In either case, it remains unclear how the specific causally relevant entities can be selected, trafficked, and delivered. It is also unclear how each germ cell could acquire the same amount of an individual small RNA or the same concentrations of pools of small RNAs. In the next generation, it is impossible to divide small RNA populations equally amongst all the daughter cells following each cell division. One compelling reason for this is that no mechanism ensures that precisely the same amount of small RNAs reaches each daughter cell. A focus on “transmission” seems thus pointless also because of the problem of “administration.”

Although small RNA inheritance researchers are interested in the persistence of small RNAs, they might not be interested in the “transmission” of small RNAs. In other words, the stability of a specific trait, caused by small RNAs, can be maintained without the stability of a specific entity (in this case, a small RNA). The stability of a specific small RNA is impossible because transmission is not feasible.

Taking together what was discussed in the previous paragraphs, two concepts for small RNA inheritance can be established. The first is that of the "stability of traits." Stability of traits (SoTs) means

> SoTs: the phenomenon when a phenotypic trait persists throughout generations without the necessity of a specific entity, causally related to the phenotype, being transmitted from the parent.

In an SoT scenario, traits are stable, while causally relevant entities are not. In small RNA inheritance, the stability of small RNA induced traits is ensured without the stability of small RNAs.

The second concept is that of "reconstitution." Reconstitution is the process through which phenotypic similarities in the parent offspring line are transgenerationally maintained in a population[49]. Reconstitution does not require the replication of a causally related entity. Reconstitution allows for both mechanistic and other kinds of explanations, such as topological ones. Due to the problems of "administration" discussed above, it is clear that no mechanistic replication framework can account for small RNA inheritance. Small RNAs are not replicated one-by-one by a polymerase, as is DNA. They can, of course, be amplified, but amplification cannot maintain exact concentrations within a pool of small RNAs. The pool of small RNAs is not replicated. Thus, the goal of small RNA inheritance researchers will be to focus on reconstitution rather than on replication.

To sum up, I have identified two epistemological goals on the molecular level that are characteristic for small RNA inheritance. Because small RNA inheritance researchers are interested in explaining the persistence of small RNA-caused traits throughout generations, they have to focus on "reconstitution" and the "stability of traits." Explanations of DNA inheritance aim at fulfilling different goals. DNA-based explanations rely on the faithful replication of information and its faithful transmission to future generations, ensuring the stability of a specific entity, DNA. Explanatory targets and explanatory goals are different for small RNA inheritance.

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[49] The concept of reconstitution was also proposed for the stability of microbiota – host communities (holobionts) across generations (Deulofeu et al., 2019).

If integrated, these different targets and goals cannot be preserved. Let me provide a practical example of the drawbacks of integration. When publishing in journals with a high impact factor, it is usually required to report specific types of proof. The types of proof required are mechanistic. To prove that a specific process takes place, at least one causally relevant entity has to be identified. To show that this entity is relevant, two conditions have to be met:

1) Taking this entity out of the system must inhibit the process.

2) The presence or introduction of the entity must sustain the process.

These requirements also define prerequisites for publishing findings in small RNA inheritance. A specific small RNA has to be identified in order to show that an observed phenomenon takes place. For example, the up or downregulation of one specific small RNA in response to a stimulus needs to be demonstrated. Reappearance of this specific small RNA in tissues of the next generation will be monitored. These evidential standards are, however, highly problematic for the case of small RNA inheritance.

The standards described above are entirely shaped by a framework that presupposes replication and the stability of entities. Even if it were possible to provide the experimental proof required, it would not correspond to what small RNA inheritance researchers want to explain. As argued above, the persistence of a small RNA induced phenotype is the explanatory focus, not the stability of one small RNA throughout generations. The focus is on how a phenotype and a small RNA state is reconstituted and not how it is replicated and transmitted. I take this to be a prime case that demonstrates the dangers of aiming too much for integration and a combination of both processes. By forcing concepts central to one approach (replication, stability of entities) onto experiments of another approach, the most important aspects of this other approach vanish.

Having discouraged integration on the molecular level, I will discuss the dangers of integrating small RNA-based processes with canonical evolutionary theory. In so doing, I will exemplify Love's arguments concerning theory presentation for small RNA inheritance. It is essential to note that I am agnostic concerning whether there “really” is “one theory” in the background. But researchers act as if there was.

Small RNA inheritance, I argued in the third chapter, can be accounted for by a Lamarckian framework. This Lamarckian framework has different explanatory resources and emphases than a traditional MS framework. The goal is to explain how the use and disuse of faculties in one generation affect these faculties in the next generation. Understanding how adaptive changes are installed in a directed manner is another core aim. Lamarckian frameworks emphasize quantitative aspects of processes. Within a Lamarckian account, the “how much?” (of exposure, of change in use/disuse) is in focus. Also, the reversibility of these shifts is possible in the Lamarckian account. These aspects of the Lamarckian framework are not part of a general “MS,” DNA-based account.

The standard strategy to “integrate,” if not “reduce” Lamarckian inheritance is to argue that the entities which are causally relevant for the specific type of inheritance are encoded in DNA and, thus, have “evolved” through a “Neo-Darwinian” mechanism. They have evolved through natural selection of random mutations. In Chapter Two, I provided arguments for rejecting this claim. I referred to the biological properties of small RNA inheritance systems. Here, I will make an epistemological argument: even if it were plausible that all “Lamarckian” effectors have evolved through Neo-Darwinian selection, we still should not push “integration” or “reduction” too far. By framing “Lamarckian” evolution in a “Neo-Darwinian” way, emphases get shifted. While Neo-Darwinian explanatory strategies focus on the population level on gene quantities, they focus on qualitative aspects of a trait on the organism. Also, the irreversibility of a trait on the organism level is emphasized. They downplay the question of “how much?” as well as the role of an “inducing” environment.

While it is possible to explain away alternative trajectories of acquired traits by arguing that they are the result of natural selection, it is not fruitful. Instead, we should realize, that by embracing a Lamarckian and a Neo-Darwinian explanatory framework, we can apply different explanatory resources in order to account for different types of phenomena. If Lamarckian use/disuse is explanatory for specific phenomena, there is no good reason to explain via natural selection of random mutations. Not only explanatory goals but also explanatory tools are radically different. This difference should be appreciated and not "integrated away."

7.3 Pluralism for small RNA researchers

In this section, I will formulate a pluralist position that is responsive to researchers' aims and goals. I will incorporate the observation that researchers often consider plurality as destabilizing and address the tension between the plurality of theories and the plurality of "repertoires" and "scientific practices." I will home in on my conclusion of the previous section that integration should be discouraged.

By now, it will be apparent that the pluralism which I am advancing for small RNA inheritance is purely epistemological[50] and also local. It is an open question whether conclusions I draw from this case can be extended to other related cases. In this section, my tasks will be to

1) dissect the links between "theory" and other sources of plurality and formulate a pluralist position for both aspects and

2) address the destabilization that plurality is perceived to bring about and suggest strategies to tackle this instability.

What kinds of pluralists should we be about small RNA inheritance? Should the fact that theory does not play a significant role within the field discourage theoretical pluralism? Does the observation that the plurality of repertoires is much more prevalent push us towards emphasizing this form of pluralism? Is the plurality of repertoires "too little" or "too modest" – a "pluralism light?" I will answer these questions bearing in mind the differentiation which I introduced in the previous section. Small RNA inheritance contributes to two levels of discourse: the molecular-mechanistic and the evolutionary. In Chapter Four, I showed that "theory" is a pressure that trickles down from the evolutionary-biological. Small RNA inheritance bears on evolutionary theory and challenges presentations of evolutionary theory. Small RNA inheritance sits better with alternative ways of presenting

[50] By stressing the epistemological nature of my pluralism I intend to distinguish it from metaphysical plurlaism. I do however not home in on the "epistemological" – "ethnomethodological" distinction, upheld by, e.g., Michael Lynch. My position is, in that sense, both epistemological and sociological.

evolutionary theory. On the molecular mechanistic level, however, it is hard to see how small RNA inheritance violates a specific theory.

I believe being a theoretical pluralist about small RNA inheritance makes sense nevertheless. But it is essential to carefully dissect the different discourses and the different subjects of pluralism. This will also prove helpful for small RNA inheritance researchers. They are, as the analysis showed, not rigorous when referring to the "subject" of plurality. This lack of rigorousness can be explained by the fact that the challenge "from above" and issues emerging from the field are not well separated. I will thus propose two levels of pluralism for small RNA inheritance.

The "practice turn" in philosophy of science has enabled philosophers to realize that significant changes in research fields might occur without the necessity of theory change. Without any visible change (or challenge) to theory, a whole research field might change. The plurality of repertoires and thus, MOs, is a necessary ingredient for pluralism about small RNA inheritance. This point is not trivial or a "pluralism light." In what follows, I will elaborate on this issue. I will formulate a specific type of pluralism concerning MOs that abstains from metaphysical commitments and is purely epistemological.

MOs are not sources of plurality because of their metaphysical properties. Metaphysical commitments, such as those of Mitchell or Dupré, are not required for pluralism in biology. I will not, for example, refer to contingency or complexity of biological phenomena when arguing for scientific pluralism regarding MOs. I will offer a different account of the MOs' properties, an account that avoids metaphysics. Relevant properties of the MO that contribute to plurality are not related to any metaphysical or essential properties. Instead, the history of establishing the MO, components that were mobilized to form the MO repertoire, experimental standards, and socio-political factors define the properties of the MO. These aspects characterize the MO. And these aspects differentiate different MOs.

Defining such epistemological factors to determine what causes the plurality of MOs suggests a particular perspective on "consensus" about small RNA inheritance. Accepting the plurality of MOs in the epistemological sense that I defined, we cannot expect a unification of the field through consensus. Consensus about evidential standards, methods, and the “controlled” vs. “natural” debate, amongst others, will not be reached. Small RNA inheritance is a heterogenous subject because it is studied in different MOs. These MOs differ by evidential standards, components, alignment of different repertoires, and disciplinary histories. Attaining monity regarding these aspects in small RNA inheritance is impossible. But this has nothing to do with metaphysics.

The specific type of plurality I advocated in the previous paragraphs is exemplified by the boundary work observed in the case study. Interviewees criticize findings on small RNA inheritance from the perspective of a specific MO. This circumstance can be explained by the fact that the “representational scope” of experiments on small RNA inheritance in one MO is not well defined. It is thus not clear what specific small RNA findings in one organism mean for small RNA inheritance in other MOs. The absence of consensus on “representational scope” causes the excessive boundary work observed.

In conclusion, the plurality of repertoires is not trivial or “pluralism light.” The constitution of the different MOs as different repertoires makes monity impossible. In the small RNA inheritance field, there is a lack of pluralism, which causes an insistence on the homogeneity of outputs across MOs. This lack of repertoire pluralism amongst researchers also causes the observed boundary work and is a source of instability. Thus, pluralism about repertoires should be encouraged.

The second point, of course, is the one concerning the “pressure from above.” Should we embrace explanatory and theoretical pluralism? One point that I elaborated on both in the previous section and in the previous paragraphs bears on this issue. Ontological hypotheses should not influence one’s pluralist position. The fact that two processes happen at the same time or co-occur within a cell should have no impact on whether to formulate an integrationist version of pluralism.

I encourage abstaining from integration because of the reasons discussed in the previous section. Accounts of small RNA inheritance differ from traditional accounts in having different explanatory goals, both on the molecular mechanistic and on the biological evolutionary level. Molecular relations and metaphysical properties should not dictate integration. Instead, epistemological features of accounts of small RNA inheritance define whether to integrate accounts. In the specific case of small RNA inheritance, concepts, explanatory goals as well as proposed principles are not congruent with traditional modes of theorizing or explaining inheritance and evolution. Therefore, in order to maintain the explanatory strengths of small RNA-based explanations, integration should be discouraged.

The last point to address is the instability that comes with plurality. In Chapter Four, I identified plurality as a source of instability. There are two sources of plurality that cause instability. Plurality of repertoires causes instability. Interviewees feared that this plurality could harm the reception of the field. Not only were researchers worried but they also perceived this type of plurality as a sign of immaturity. Plurality of theories is another source of instability. In Chapter Four, I identified "theory" as Janus-faced. While it serves as a means of representation to the outside, it destabilizes the field inside. The challenge to evolutionary theory small RNA inheritance bears on concerns the plurality of theories. This kind of plurality is different from the one described before. Participating actors are diverse and opponents are hard to define. As mentioned earlier, the plurality of theories is pressure from above. This pressure forces small RNA inheritance researchers to meet an explanatory goal, which is not part of their repertoires.

What is there to say in defense of plurality, if it is a destabilizer? I believe that it is a task for philosophers to address the reasons why plurality appears destabilizing and to offer solutions. Let me suggest a specific entry point. In Chapter Four, I provided two figures when opening my discussion of the empirical results. I depicted how many interviewees sympathize with plurality in general and how many interviewees agree with pluralism as an interpretation of small RNA vs. DNA inheritance. There was a tension between these results because some interviewees "converted" from not agreeing with pluralism in the "big picture" to finding some agreement with pluralism about their specific case. There could be several explanations for this switch. It is possible to identify this "switch" as a strategy. Pluralism about their specific case, which faces both "theory pressure" from above and "repertoire pressure" from within, might seem an excellent way to alleviate these pressures.

Plurality might be a threat, but pluralism can be a strategy to diminish it. It is a task for philosophers sympathizing with SP to create conditions that facilitate the establishment of pluralism. A critical step for a successful establishment of pluralism is work that disentangles the intertwined issues of theory vs. repertoire plurality. As of yet, this has not been sufficiently done. I consider this dissertation as a first step in doing so. It also needs to be highlighted what the benefits of becoming a pluralist concerning theories and repertoires are. Besides epistemological benefits discussed in previous paragraphs, strategic benefits and how it would alleviate pressure are of course included.

The strategy discussed above resonates with the normative-descriptive-normative transition invoked by the practice turn. A priori normative pluralism is discouraged. What has first to be done is to provide a descriptive account of the field (in this case, through a sociological case study). The next step is to incorporate the results of the case study in the pluralist position. Practitioners perceived plurality as a source of instability; thus, the instability plurality might bring needed to be considered within the pluralist position formulated. The philosopher has to show what has to be done so that plurality is not conceived as a threat anymore and highlight the perks of embracing plurality.

For 30 years, practitioners have continuously tried to integrate, synthesize, or even reduce alternatives or "exceptions" in inheritance or evolution with standard rules and dogmas in biology. The epistemological benefits of doing so are quite scarce. The threats might by far outweigh the benefits of integration. As philosophers and pluralists, we should try to alleviate this situation. It might be challenging to convince researchers to frame the discourses to which they are contributing differently. Nevertheless, ideas on how to achieve this are quite straightforward: take a case study and showcase the benefits of the framing. It is also essential to have such work published in places that are read by biologists. Contacting those researchers for whom such research might be relevant is even better. From my own experience, this strategy is, though it is bottom-up and slow, sustainable and effective in the long run.

7.4 Synthesis with SP literature

In this section, I will interpret the results of my case study concerning whether or not they align with claims made by several influential scientific pluralists. I will assess how Chang's, the Pluralist Stance's, and Mitchell's claims resonate with the results of the case study.

These three positions were advanced after the “practice turn” in philosophy of science. In contrast to earlier works, they share a commitment to focus on aspects of scientific practice when investigating scientific pluralism.

7.4.1 Chang

In general, my pluralism is compatible with Chang’s pluralism in terms of its conclusions. Some groundwork is different, however. One of the most critical aspects of Chang’s pluralism is that he emphasizes that his stance is “active.” Chang’s pluralism is a call to action, as he pleads for the introduction of plurality into every field of research. The aspect in which I disagree with Chang recalls the debate between Chang and Kusch, reviewed in the previous chapter. In my version of pluralism, I emphasize the importance of the case study. It is an essential means for analyzing the social structure of the target research field, effects of plurality to the field, and reasons why researchers favor or do not favor pluralism. This methodology allows that a case study might show that actors have “good social reasons” or “good broad reasons” not to favor plurality or pluralism. In that sense, the philosopher’s normativity will be case-sensitive. The types of philosophers’ prescriptions for a field might also change considerably concerning the specific case investigated. For example, in the case of small RNA inheritance, one primary suggestion was to encourage pluralism but not an integrative type. I argued that the explanatory strategies and goals of the reviewed approaches are too different for being integrated. Through integration, concepts, and aims characteristic of these approaches would get masked. There might be other cases in which a case study would produce different results.

Besides, the toolbox I developed in Chapter Four for characterizing researchers’ positions allows for a more fine-grained approach when reconstructing actors’ positions. For Chang’s case study, for example, it would be interesting to see how actors during the Chemical Revolution would be characterized following the produced toolbox. Dividing them into “pluralists” and “monists,” as did Chang, might not be as telling as asking whether they were integrationists or non-integrationist, dualists, or singularists. If Chang’s actors were pluralists, it would be interesting to try to look for the resonance of their positions with the positions I reconstructed in Chapter Four.

Concerning the practical implications, Chang's and my position are congruent. Chang's argument against Kuhn-loss also gets a further twist through my case study. Not only are small RNA-based approaches and DNA-based approaches "highly successful systems of practice." We should keep both and the synthesis of both should be discouraged. In his writing, Chang emphasizes that integration should only be "ad hoc" to address a specific problem. I take Chang to define integration as a temporally limited epistemic activity. Integration is then not concerned with exploring similarities of two approaches, universal principles, or common targets. That is, his version of integration does not involve metaphysical assumptions about the properties of causally relevant entities or the target phenomenon. Although I am not sure whether ad hoc integration is beneficial to small RNA inheritance, given the concerns raised above, I sympathize with this formulation of integration in general.

One last point that requires attention is Chang's suggestions for pluralism-promoting tasks for HPS. To "bring out the plurality" within a field (2012, 287) was accomplished with this case study. Beyond that, it was my goal to differentiate between heterogeneous attitudes towards pluralism and plurality on the sides of the actors, to offer a more in-depth account of the effects of plurality in a research field.

Chang also discourages preoccupation with closure (ibid.). As far as this case study goes, I believe it also lived up to this suggestion. I discourage closure if it means "integration" or "unification" or "monism" for this specific case. However, reaching back to the Chang-Kusch debate, "closure" does not necessarily mean any of these things. "Closure" means that debate is settled amongst the core set of investigators. "Closure" can mean the acceptance of plurality or even embracing pluralism. In that sense, I argue for closure, namely embracing a pluralism of an "alternative" kind concerning DNA-based and RNA-based explanations on one level and "Neo-Darwinian" and "Lamarckian" theory on the other.

Finally, I believe that specific parts of this case study count as complementary science, if extended to not only "overlooked" or "forgotten" systems of practice but also to those that are currently marginalized. In a sense, complementary science was not used as a "treatment" but rather as a "vaccination" against ongoing and future attempts that might threaten a well-running SoP.

7.4.2 Pluralist stance

Similar to Chang, my position has some convergences with the Pluralist Stance but also some areas of disagreement. For the specific case I am advocating, I share the Pluralist Stances "non-integrationist pluralism" and their notion that "integration leads to loss of content" (2006, xiv). I also fully share the authors' practice-oriented view that:

> "There can be plurality of representational or classificatory schemes, of explanatory strategies, of models and theories, and of investigative questions and the strategies appropriate for answering them."… "and that analysis of meta scientific concepts (like theory, explanation, evidence) should reflect the possibility that the explanatory and investigative aims of science can be best achieved by sciences that are pluralistic, even in the long run." (2006, ix).

In these quotes, the importance of researchers' explanatory aims and goals is emphasized as a source of plurality. Kellert, Longino, and Waters thus promote a practice-oriented form of pluralism. In parts of their works that do not explicitly emphasize the variety of what they call "meta-scientific concepts," they remain very much in the realm of "theory." It was one result of my case study to identify the "pressure" of theory as a challenge that comes from outside the field. If this pressure remains unidentified, a specific pluralist interpretation could be forced onto the field that is not in accordance, or resonant, with actual sources of plurality within the field. I believe, therefore, that "practice-orientation" was pushed further in my position than in that of the Pluralist Stance.

Unlike the Pluralist Stance, I am not in principle against "integration," and I do not believe that it leads to monism. The authors of the Pluralist Stance ground their position on "exclusiveness," on the idea that every approach is formulated as a theory and is to be taken as a model or idealization:

> "In fact, an acceptable scientific model might describe some facets of the situation extremely well (e.g., the potential causal influence of some factors) while actually distorting other facets (the potential causal influence of other factors)." (2006, xiv).

I want to call for the empirical commitment of defenders of the Pluralist Stance. Why should we expect that each case of pluralism will be (or has to be) an instance of "non-integrationist plurality?" If they ground this commitment with an idea about what phenomena "are like," then they would violate their commitment that metaphysics is not decisive for their pluralism. If the argument is epistemological, it would be of the sort that there are inherent epistemological benefits to treating approaches as non-integratable. I would argue that this requires empirical demonstration for any case. I also think that the integrative pluralist could argue that there are as well inherent epistemological benefits to treating approaches as integratable. Besides, although the authors of the Pluralist Stance encourage a concise examination of scientific practices, I would encourage them to perform a closer examination of the research fields at stake. Positions such as pluralism and monism, dualism and singularism, integrationism, and non-integrationism might all be present within one research field. These tendencies go together in several combinations and also reflect the positioning of sociological or political aims. Thus, the question of what is epistemologically better does not remain a simple one. There might be a vast heterogeneity within the field. It would better match the empirical commitment of the Pluralist Stance to regard the decision whether integrative or non-integrative pluralism is to be recommended as an empirical matter.

7.4.3 Mitchell

Mitchell shares some notions of the Pluralist Stance in that "Theories abstract and simplify to achieve precision and generality" (2003, 190). But contrary to the stance she claims

> "However, the concrete explanatory situations on which we bring the abstract models to bear are messy, perhaps unique products of historical contingencies and interacting, multiple causal factors." (ibid.).

Mitchell supports a position that discourages both "reduction" and "isolation" – isolation meaning to separate a field that displays plurality into different subfields or different "levels." Her "non-reductive integration" is explained by the notion that "while integration of the partial accounts is, indeed, required for explaining a concrete particular, unification at the theoretical level is unlikely to be very robust." (2003, 217). As discussed above, I take issue with taking this as a general prescription for any case of plurality because threats of integration might outweigh the benefits.

Let me also locate a second disagreement with Mitchell's position. Mitchell differentiates between "compatible" and "competing" approaches and takes the dispute between Darwinism and Lamarckism to be of the latter kind. My disagreement with Mitchell concerns the issue which I have excessively discussed in Chapter Three and reopened in this chapter. I do not believe that there are "naturally" occurring compatible or competing approaches. This ties in with a related issue. Mitchell justifies "integrative pluralism" by referring to the metaphysical properties of certain phenomena. I argued against tying one's "integrationist" or "non-integrationist" pluralist aspirations to the ontological properties of phenomena. Pluralism and plurality are sufficiently explained through "repertoires" and the trajectories of the respective research fields. Thus, while I believe that there are good exemplary cases in which Mitchell's pluralism should be an epistemological prescription, this is case sensitive. It is necessary to assess the promise of integration case by case, which can and should be done without metaphysical presuppositions.

7.5 Relativism and pluralism in my position

In the previous sections of this chapter, I laid down my pluralist position. I characterized it as epistemic and normative. Given the fact that I have extensively discussed the relation of SP and ER in Chapter Seven, I need to answer the question: Am I a relativist? And is my pluralism one of the kinds that are compatible with relativism? The short answer is "yes." In the following paragraphs, I will consider the import of relativism on my position as a methodology and as a stance separately.

I will discuss methodology first. Given my methodological discussion and the authors, which guided my investigations, my proximity to a version of the "Strong Program" of SSK might be apparent. I realized the four characteristics of the Strong Program, causality, impartiality, symmetry, and reflexivity in my case study. When examining the small RNA inheritance field, I maintained impartiality regarding whether specific claims of actors are "true" or "false," "successful," or "unsuccessful." I examined the causes of why particular actors believe that specific claims are true or false. Finally, I investigated those claims that were judged “wrong,” or those actors who were considered “unsuccessful” by the community in the same manner as I investigated the claims that were judged “right,” and those actors who were considered “successful.” In conclusion, the criteria causality, impartiality, and symmetry guided my approach.

The criterion reflexivity, in particular, requires special attention because it applies on two levels. One level concerns reflexivity regarding my position as an analyst and actor. Owing to the normative positions I advanced in previous sections of this chapter, it is clear that I am not only analyzing small RNA inheritance researchers. I have published on small RNA inheritance and am quite explicit in my opinion on what might benefit the field. Thus, of course, my sociological analysis of small RNA inheritance researchers is also applicable to me, in situations in which I qualify as an actor. The other level concerns the analyzability of my sociological research on small RNA inheritance by the same principles that guided my approach. Analyzing my empirical output and conclusions utilizing SSK’s principles is, of course, possible. I realize reflexivity, therefore, in two ways.

Besides relativism as a methodology, the more interesting question is, of course, whether I subscribe to relativism as a stance. Chapter Seven might have made it evident that I sympathize with relativism. It is also evident that I believe that pluralist positions can profit from the incorporation of relativism, even though both positions might operate on different levels. A good starting point for answering this question is one of the empirical observations which I presented in Chapter Four. Interviewees, if they were sympathizing with pluralism, motivated their pluralism with a version of non-absolutism rather than non-monism. Given the discussion of pluralism and relativism in Chapters Five and Six, it becomes clear that some of the positions expressed by interviewees had a relativist tendency. In Chapter Six, I discussed that Feyerabend’s relativism motivates several of his other commitments. Something similar applies to some of the interviewees who support pluralism. Some motivated their pluralism with the relativist ideas of non-absolutism and dependence. In conclusion, not only the philosopher might maintain pluralism and relativism, but this can also be found amongst practicing researchers.

The fact that small RNA inheritance researchers supported a version of non-absolutism is, of course, not a justification for supporting ER in my position. It gives, however, a different important cue. In Chapter Seven, I discussed that card-carrying scientific pluralists are often concerned about the reception of their position if they tend towards relativism. My empirical results might address such worries. If a relativist motivation of pluralism is found amongst scientists' commitments, then accepting ER as a scientific pluralist might not be as problematic as commonly believed. Of course, it is also clear that the "fear of relativism" one encounters in the philosophical literature is also due to a missing examination of what it would mean to accept relativism as a pluralist. With this dissertation, I hope to have cleared these issues up.

My position regarding my SP about small RNA inheritance and its relation to ER still needs to be expressed. I have defended a non-integrationist version of SP regarding small RNA inheritance because I identified several problems of integration in this particular case. Does this mean I am proposing different ES, the DNA-based and the RNA-based, about which I am a relativist? This would contradict the empirical results of my case study. Small RNA inheritance researchers are dualists, that is, they use both explanatory strategies, depending on the particular phenomenon they are approaching. It would be insensitive to the empirical data to propose an ER of the ES of "DNA-based" and "RNA-based" biology.

In Chapter Six, I expressed my position regarding the character of ES. I rejected strong commitments to essential incommensurability between ES. ES are real, yes, because actors make them real. The boundaries of ES might, however, be minded by one group of researchers but transgressed by another group. With regard to my take regarding ES, it might become apparent that proposing and emphasizing different "ES" might not be part of any version of relativism that I support.

My relativist position is, I think, specifically illustrative regarding normativity and relativism. From my suggestions for small RNA inheritance researchers and my more technical arguments regarding reconstitution and the stability of traits, it might be clear that I believe that small RNA inheritance is a good explanation for particular phenotypes. By applying grid/group analysis, I reconstructed the organization and commitments of parties in the dispute. Based on that perspective, it becomes possible to regard the defense of the "MS" and the campaign for the "EES" as stances. Both parties, although they have some shared grounds, have different VEPPs, and regard other aspects core to their commitments.

Different ways of theory presentation, which are informed by the preferred VEPPs, also play a role. By reconstructing the arguments of defenders of the MS and EES, it is, therefore, possible to understand their judgments and how they rank values. At the same time, I believe that some assertions of the defenders of the MS are wrong, relative to my beliefs, which are quite close to the EES. In fact, a relativist commitment, in these scenarios, is almost necessary to understand and reconstruct the dispute.

What I established, therefore, for the relation of pluralism and relativism in general, is also true for my position in particular. First, both commitments operate on different levels and sometimes the one informs the other. Second, relativist methodologies can help uncover plurality. Third, normativity, while subscribing to a relativist position is possible. I am, thus, a scientific pluralist, and an epistemic relativist regarding small RNA inheritance at the same time.

8 Literature

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9 Attachments

9.1 Abstract (German)

Der Wissenschaftspluralismus (WP) etablierte sich in den letzten Jahren und Jahrzehnten zu einer weitverbreiteten Position innerhalb der Wissenschaftsphilosophie, und besonders in der Philosophie der Biologie. Aufrufe gegen „Einheit der Wissenschaften," und Reduktionismus, und für einen epistemologischen und ontologischen WP waren in der zweiten Hälfte des 20. Jahrhunderts noch kontroversiell, mittlerweile ist der WP aber kaum noch umstritten. Gegnerinnen des WP lassen sich nur noch schwer lokalisieren. Wurde der WP also universell, und unkontroversiell? Ich argumentiere in dieser Dissertation, dass zwei Probleme des WP noch aufgelöst werden müssen. Erstens, wurde die Übereinstimmung des WP mit den Zielen von Wissenschaftlerinnen noch nicht überprüft. Dieses Problem bedarf einer praktischen Lösung. In dieser Dissertation führe ich eine soziologische Fallstudie durch, die das Potenzial von WP in einem speziellen Feld der Biologie, kleine RNA Vererbung, erfasst. Zweitens, gibt es nicht nur ein praktisches, sondern auch ein theoretisches Problem. WP wird oft mit einer anderen in der Philosophie bekannten Position assoziiert, dem erkenntnistheoretischen Relativismus (ER). Beide Positionen akzeptieren „Pluralität." Die Wissenschaftspluralistin postuliert eine Pluralität von unterschiedlichen Ansichts- und Herangehensweisen. Die erkenntnistheoretische Relativistin postuliert eine Pluralität von unterschiedlichen epistemischen Systemen. Obwohl es also diese intuitive Nähe beider Positionen gibt, versuchen Wissenschaftspluralistinnen ihre Position vom erkenntnistheoretischen Relativismus abzugrenzen. Kritikerinnen des WP behaupten, WP würde zu ER führen. Obwohl sich solche „Anschuldigungen" in der Literatur oft finden, gibt es keine tiefergreifende Diskussion zum Verhältnis von WP und ER. Im zweiten Teil der Dissertation werde ich daher die Beziehung von WP und ER diskutieren. Im abschließenden Teil der Dissertation werde ich die praktischen Probleme aus dem ersten Teil der Dissertation, und die theoretischen Probleme aus dem zweiten Teil der Dissertation zusammenführen.

9.2 Abstract (English)

In recent years, scientific pluralism (SP) has become one of the household positions within philosophy, and philosophy of biology in particular. While pleas for the “disunity of science,” against foundationalism, and for epistemological and ontological SP were quite controversial in the last decades of the 20th century, SP is nowadays a rather standard position. There are fields within philosophy of science, where one might ask who defends a position that is not a version of SP. Has SP, thus, become universal, and uncontroversial? There are two major reasons. First, SP bears on several practical issues within the sciences. If we accept SP, this entail that we require the scientific disciplines to change. SP most of the time requires a normative commitment. The sciences should become more pluralist. The scientists should adopt pluralist interpretations of target phenomena. The question how well SP resonates with practicing researchers has, however, not been answered yet. The first reason for why SP needs to be reconsidered, is therefore practical, and requires empirical examination. In the first part of the dissertation, I will address this problem. The second reason is a theoretical one. SP lies in the vicinity of another position, that is well known within philosophy, and the philosophy of science: Epistemic Relativism. For example, both positions accept a form of “plurality.” The scientific pluralist believes that there is, or should be, a plurality of different approaches. The epistemic relativist believes that there is more than one epistemic system. Although there seems to be some intuitive vicinity, card-carrying scientific pluralists often demarcate their position from epistemic relativism. Critics of scientific pluralism often ask whether SP entails relativism. Although such “accusations” and responses are quite frequent in the literature, there is no in depth elaboration on these issues. Most of the time, card-carrying scientific pluralists claim that because they do not “renounce judgment,” they do not support relativism. If one studies works of defenders of relativism, it becomes clear that card-carrying relativists also do not renounce judgment. What is, then, the relation of pluralism and relativism? The second reason, in conclusion, why SP needs to be reconsidered, is a missing, in depth account of the relation of SP and epistemic relativism. In the second part of the dissertation, I will address the relation of SP and epistemic relativism. In the concluding section of my dissertation I will synthesize ideas and conclusions of the two parts.