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„Scientific Pluralism – A call for Action?
Reassessing Darwinism and Lamarckism"

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Inhalt

1 Introduction

What does a pluralist's practice in science look like? How could historical and epistemological work of historians and philosophers of science be integrated in science policy and laboratory life? In his latest work ***Is water H_2O? – Evidence, Realism and Pluralism***[1] Hasok Chang introduces his conception of scientific pluralism, which he characterizes as epistemic, normative and active. Chang tries to show that the theory of phlogiston was prematurely discarded after the rise of Lavoisierian chemistry. For doing so, the practice of "complementary science" which he introduced in his 2004's ***Inventing Temperature – Measurement and Scientific Progress*** renders an important tool for reexamining the phlogistonist's case. Through reassessing the Chemical Revolution, Chang aims at making a case for scientific pluralism in a scenario, where all preceding historians of science – for example Thomas Kuhn – have unanimously awarded Lavoisierianism with the winning trophy. By fighting the claim that scientists were more than justified in omitting phlogistonist theories by the end of the 18th century, Chang aims at opening the arena for more investigations of this sort.

Importantly, the topics of the disunity of science as well as of pluralism have been very prominently discussed in the last decades by several scholars. Two famous publications advocating a pluralist approach are John Dupré's 1993's ***The Disorder of Things - Metaphysical Foundations of the Disunity of Science***[2] and Helen Longino's, Stephen Kellert's and C. Kenneth Water's 2006's co-edited volume ***Scientific Pluralism***.

Strikingly, the individual notions of and the suggested approaches towards pluralism of the authors just mentioned are highly diverse. Whereas Chang's pluralism can be delineated as normative-active, Dupré's argues for metaphysical pluralism whereas Longino, Kellert and Waters canvass their pluralist stance rather cautiously on empirical grounds. It is a common practice for scholars to demarcate different versions of pluralism by referring to their "modesty" vs. their "radicalness" (Kellert et al., 2006, xiff). Thus, Dupré's and Chang's stances would be placed in the radical camp, whereas Longino's, Kellert's and Water's case would take a tentative position in the middle of the scale.

In this scheme "modest" pluralism is represented as a position, which interprets pluralism in science as symptomatic for shortcomings of our theories and methods but

[1] In the following, this monograph will be referred to as ***Is Water H_2O?***
[2] In the following, this monograph will be referred to as ***The Disorder of Things***

believes it will be resolvable in the future. On the other hand, "radical" pluralism is taken as the position, that there are numerous accounts of the world, whose "correctness" cannot be decided (Kellert et al., 2006, xiff). Thus applying such a scale on the aforementioned positions leads to a misrepresentation of Chang and Dupré as – in contrary to what is suggested by the scale - those positions seem to be fundamentally different (see Fig. 1):

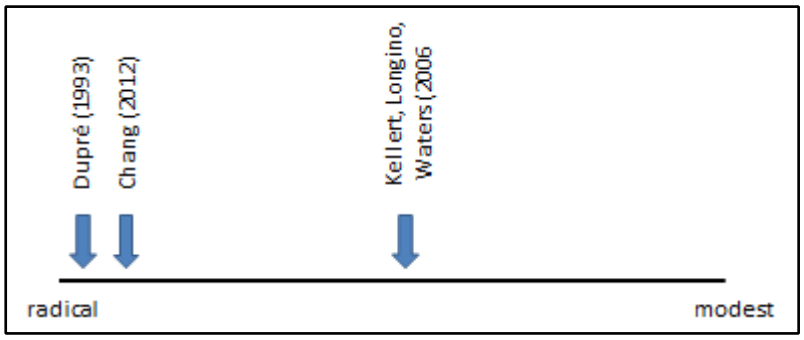

Figure 1 Radical and Modest Pluralist Positions

I claim that classifying pluralist positions by their extent of radicalness / modesty is not only strange wording but does also miss intrinsic features of the positions. Additionally, radical pluralists are often accused of being relativists in disguise and modest pluralists are characterized as undecided monists (Kellert et al., 2006, xiff). Thus, this scheme might not be able to capture the features of the various pluralist positions as tendencies towards relativism and monism might not be satisfactorily informative. Therefore, I also criticize the end-points of the presented scale. In my thesis I propose that finding other ways of classification will help to gain a better insight into the discrepancies and similarities of the many approaches towards pluralism. One way of demarcating the different position I envision is assessing them for the degrees of activity they demand and based on which calls for pluralist practice they imply.

The aims of this master thesis are threefold. First I will try to contrast Chang's pluralism with Dupré's metaphysical "Disorder of Things" and the "Pluralist Stance" of Kellert, Longino and Waters. This will aim at developing demarcation criteria for the broad scope of pluralist notions. In the course of this, positions towards relativism, realism and monism will be discussed. Additionally, I will assess the reception of each individual pluralist position by the other authors mentioned. This discussion will lead me to develop new possibilities for schemes of classification.

Second, I will try to give a survey of critical studies on Chang's work, which try to show that some of Chang's historical claims are false or not accurate – thus weakening his

case. Additionally, I am going to critically assess the implications Chang's position of active pluralism has, concerning day-to-day scientific practice, and how much plurality might and could actually be tolerated by society. To do so, I will discuss Philip Kitcher's 1993's ***The Advancement of Science – Science without Legend, Objectivity without Illusions***[3] and his 2011's ***Science in a Democratic Society*** where he develops models for assigning the costs and benefits as well as the motivations for plurality in the sciences.

Third, as Chang's pluralism involves a commitment to action, I will "test" Chang's approach on another historical example, namely the battle between Lamarckism and Darwinism. This "battle" is still represented in today's scientific discourse by the debate of Neo-Lamarckians versus Neo-Darwinians concerning the interpretation of epigenetic phenomena. Similarly to examining whether scientists were justified in rejecting phlogistonist theories in favor of Lavoisierian chemistry in the late 18th century, I will examine the historical circumstances of the rejection of Lamarckism before and after Darwin published his thesis on species evolution. This case-study will prove to be quite delicate, as on the one hand, as of today, it seems, that some Lamarckian spirit has found its way back into current-state molecular biology through the identification of epigenetic phenomena. This might at the first glance suggest that scientists where premature to fully reject Lamarckism only to discover Lamarckian phenomena more than a century later. On the other hand, Lamarckism was not generally accepted when the Darwinian evolutionary theory entered the battlefield. On the contrary, Lamarck's theories were justifiably doubted by many scholars of his time, therefore this case might not be characterized as a text-book paradigm shift.

Thus it will be critical to assess, whether researchers at that time had good reasons to prefer Darwin over Lamarck. This will lead me to formulating and discussing my thesis, that although I interpret epigenetic inheritance as a Lamarckian phenomenon, the adoption of Lamarckist positions was not justified until very recently. Additionally I will question, whether pluralist practice concerning the case of Darwinism and Lamarckism could have reshaped scientific progress and I will close my considerations by assessing whether there are certain circumstances that allow pluralist practice as suggested by Chang.

[3] In the following, this monograph will be referred to as ***The Advancement of Science***

2 The Plurality of Pluralisms

In the following chapter I will focus on several points: First, I will give an outline on Chang's motivations for pluralism, its alleged benefits and a detailed assessment of what Chang defines to be "his" pluralism: active, normative, epistemic pluralism. This will be followed by a discussion of his stances concerning realism, monism and relativism. Additionally I will discuss implications Chang's pluralism would have on scientific practive as well as on the history and philosophy of science.

Subsequently I will proceed by giving an outline on the pluralist positions advocated by Dupré in his ***Disorder of Things*** and by Kellert, Longino and Waters in their volume ***Scientific Pluralism*** as well as in Longino's monograph ***The Fate of Knowledge***[4]. Having outlined the framework of those positions I will discuss their notions on monism, relativism and realism. At last I will contrast the various positions discussed based on their stances concerning several problems. These considerations will provide me with the grounds for developing a new scheme for the classification of pluralism: a system that is based on commitment to pluralist action.

Importantly – and this demarcation holds true for all discussed authors – notions of plurality and pluralism need to be distinguished. Attesting *plurality* is the result of an empirical observation, which usually leads scientists and philosophers of science to accepting that a scientific field is at a certain time not fully monist. This is neither a normative nor a metaphysical statement. Contrarily, scientific pluralism means that, depending on the strength of the pluralism, a plurality of theories, approaches and explanations is an intrinsic and not resolvable feature of the respective scientific field or even of science in general.

2.1 Chang's Pluralism

Chang develops his position on pluralism by discussing three scientific fields of discourse that led to the formula of water – H_2O. In this vein, he gives an account on the rise of Lavoisierian chemistry and the rejection of phlogiston. These events led to the paradigm shift which is often referred to as the Chemical Revolution. Additionally, he discusses the problem-fields that arose with the invention of electrochemistry and electrolysis and the different atomic models. By doing so he tries to convince the reader, that water is not simply H_2O, but that there were always other good theories around that implemented other, valuable conceptions of water. In each of the three cases, Chang concludes, that the empirical sciences and the

[4] In the following, I will use the term „pluralist stance" exclusively to refer to the pluralist position of Kellert, Longino and Waters, as they specifically assign this term to their notions.

experimental data they produced were at that time not capable to settle debates, thus he comes to the conclusion, that consensus was reached premature if not even unjustified, due to underdetermination of theories by scientific facts.

Additionally, he aims at showing that there was much more plurality at the end of the 18th century than is normally accounted for in the relevant literature, and that there were also, after the publication of Lavoisier's treatise, a lot of scholars who were either "die-hard" phlogistonists, new converts or opposing to Lavoisier's view out of other reasons. Thus, his general case for pluralism is based on two assumptions: On the one hand, the investigation of this example shows that many other paradigm changes might have to be revisited. On the other hand, he shows that there were some features that would have been worth preserving (Chang, 2012, 254).

For the understanding of Chang's notions of plurality and incommensurability the term of "system of practice" needs to be introduced. A system of practice can be characterized as "a coherent and interacting set 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." Thus, the epistemic success of a system of practice can only mean its overall effectiveness in realizing various epistemic values of this system (Chang, 2012, 16 and 260). In the following section, I will discuss the motivations for and benefits of fostering many different systems of practice at the same time.

Why Pluralism?

When discussing the main motivations for pluralism I will demarcate the reasons for plurality he provides from the benefits, which the cultivation of plurality will bring about. First I will introduce Chang's conceptions of humility and hubris, which comes along with his conception of scientific maturity. I will question this concept of maturity and I will ask whether it can really be taken as a guarantee for plurality. Then I will give an account on the proposed benefits of interaction and toleration. At last, before engaging witch the implications of his active, epistemic, normative pluralism, I will give a short teaser on Chang's beliefs on pluralism as an antidote to Kuhn-loss and as a desideratum of a liberal democracy. I will discuss these notions in more detail in chapters 3 and 4.

Humility, Hubris, Prudence

When Chang assesses the motivations for pluralism, he discusses three different concepts: humility, hubris and prudence. One of the basic motivations for pluralism is humility: Chang claims that the external reality is rather complex. This renders it impossible for the human

mind to find “the” perfect system. Thus, it is a responsible action to apply as many good systems as we have to capture as much of the external reality as we can. Chang illustrates this claim with an example he draws from Priestley, who cherished the fact that the more light is shed on science, the greater the boundary of darkness gets. Nevertheless, humility is not to be interpreted as admitting some defeat to our possibilities of understanding the exterior world. Contrarily, Chang suggests to feel great satisfaction and awe at how many things we are able to shed light on (Chang, 2012, 255f).

A second motivation for pluralism is to refrain from hubris: believing that we could grasp the truth about our external reality is labeled by Chang as a belief. It stems from ancient thinking and was compatible with monotheism. But, as science has matured, its hubris and faith can now be abandoned (Chang, 2012, 256f).

Yet, in spite of that optimism, I believe that it remains questionable, whether the attestation of maturity does not involve some of Chang’s much-feared hubris, because his characterization of mature science and especially of a mature scientist reflects solely his desiderata. Even more importantly, it does not necessarily represent a definition most scientists might provide for “mature science”. Thus, Chang’s definition of the maturity of science seems somehow problematic, as his argument becomes circular, when he characterizes mature science as pluralistic while providing maturity as an argument for scientific pluralism.

Importantly, there arises a second problem with Chang’s characterization of mature science: as he claims, that the main feature of mature science is pluralism, then cutting-edge is not mature. This is due to the fact that neither scientific areas nor most individual scientists can be described as favoring pluralism.

Chang also discusses what characterizes mature science and mature scientists. While admitting, that the term “mature” should rather be conceived as a metaphor, he uses attributes for describing mature science which all immediately remind of terms, with which a mature human being would be described: mature science has developed and performed enough experiments (thus, it is somewhere behind its take-off point), it knows its strengths and weaknesses, it knows where to go for further development and shows some degree of specialization. An idealized mature scientist values tolerance, humility and circumspection and maintains a tough questioning attitude, a range of theoretical ideas and a versatile

methodological toolbox (Chang, 2012, 237f). Thus, Chang's conception of the maturity of science and scientists merge.

Taking this into account, it does not seem quite clear to me whether constructing a parallel between a general view on the intellectual development of a human being (accepting other opinions, self-assessment…) and the features of mature science is a legitimate operation. To provide an example, fields of science and scientists are not only motivated by their characteristics provided by maturity, but also by ambition and concurrence what certainly impedes – at least in some instances – maturity.

In addition to the factors of humility and hubris that remind of vocabulary used when discussing morality and ethics, he also introduces a rather neutral "prudence argument", which states, that it is important to endow science with ways of surviving even if our best theories might fail. This is also motivated by Chang′s conviction that each paradigm will eventually fail as it is an ever-existent and the most important aim for human beings to broaden their scope of knowledge. Thus, no paradigm can perpetually live up to these demands (Chang, 2012, 257f). Having discussed Chang′s main motivations for pluralism I will now turn to elaborating the alleged benefits of pluralist practice.

Benefits of Toleration and Benefits of Interaction

Chang defines two broad categories of the benefits of pluralism: benefits of toleration and benefits of interaction. Tolerant pluralism does not require that researchers actually are pluralists, thus it represents a first and tentative step towards pluralism. On the other hand, interactive pluralism requires researchers to be pluralist, as they are required to confront themselves with works done within other systems of practice. This kind of pluralism requires a level of shared common language so that cross-fertilization is possible.

In this section, I will first discuss the four benefits of toleration Chang provides and then I will proceed by discussing the three benefits of interaction. In a concluding section I will discuss problems arising when embracing those benefits.

The more tentative way of claiming the epistemic value of scientific pluralism is to claim the benefits of toleration. The first argument for tolerant pluralism also satisfies positions that hold that there exists some objective and univocal truth. In any case, it is beneficial to nourish multiple lines of inquiry in the hope that one of them might actually give us ultimate clues about the external world. This position is supported by Larry Laudan's pessimistic meta-induction, which claims that throughout history, people often thought they were inevitably

scientifically right. Yet, they were proven to be wrong by our modern understanding (Laudan, 1981, 47). Thus, it seems to be most rational to hedge one's bets if it is not clear who the winner will be (Chang, 2012, 270f).

Another benefit of toleration arises from Chang's claim, that it is often sensible to divide the domain. Different systems only partially fulfill a certain aim, so different lines of inquiry can be joined. There are also pragmatic implications for having different systems of practice cultivated: even if one single theory could in principle cover all domains in one scientific field there are other theories, which do answer certain questions faster and more efficiently (Chang, 271ff).

The third benefit of tolerant pluralism lies in the fact that it can help in satisfying the different aims of scientists. As most scientists are driven by more than one epistemic desideratum at a time, and the desiderata vary amongst scientists, cultivating a pluri-axial system will make it possible to satisfy multiple aims, as at least for the near future Chang predicts that it will not be possible to accommodate all needs, aims and interests under one position (Chang, 2012, 273ff).

In Chang's last argument for tolerant pluralism, he turns his personal delight and joy about pluralism into an argument in favor of it. Thus, he states that it is very delightful to be able to look at a certain phenomenon from different perspectives, such as on quantum theory from a Heisenberg, Schrödinger, Feynman or Bohm point of view. In his opinion, the only thing that is logically required from two theories on the same phenomenon that are not equivalent to each other is that they do not contradict each other. This means, that even if we have a theory that adequately covers a phenomenon, we should actively engage in producing an abundance of observational knowledge to delight ourselves (Chang, 2012, 275ff).

Accepting the benefits of interaction involves a commitment of scientists to accept pluralism or at least to be aware of the fact that there is a certain degree of plurality within their field. Thus it requires the willingness to work with such plurality. In this vein, Chang discerns three different benefits of interaction, which are gradually more compatible with anti-pluralist positions.

Chang's first argument for the benefits of interaction is integration: as there are cases, were different systems of practice cannot additively achieve a certain aim, the integration of the systems might yield better results (Chang 2012, 279f).

If integration is not possible or not desired, the development of one system of practice can be helped by ideas taken from another. Thus, empirical results, theoretical ideas, mathematical techniques, instruments or material of different systems might be co-optated in other systems to achieve the specific aim of one system of practice (Chang, 2012, 280ff). In an ideal scenario, the flow of co-optation is not only top-down (from the "leading" system of practice to the "lagging" ones) but can also be bottom-up. In Chang's opinion, each system develops under certain constrains, that might be shaped by its experimental practice or theoretical mind-set, which can prevent researchers from making certain observations or performing certain experiments. Thus, cooption is the way how these sciences still can advance – through input by another, incommensurable system of practice. Notably, cooptation usually requires some incommensurability first which needs to be overcome and is not simply the activity of drawing from every result other researches have produced, as this is a process which is a feature of practice within one system (Chang, 2012, 282).

As a last resort, if neither integration nor cooption can take place, at least there can be productive competition. Importantly, most competition only arises if systems are not in total isolation from each other. Chang defines real-life competition as "an interactive process, in which competitors pay attention to each other and become influenced by each other's behavior" (Chang, 2012, 282). In a pluralistic system, Chang believes that competition of other accepted systems of practice will make it much harder for an even to a certain degree "dominant" system to justify their approaches and "selling" them to their funders, potential collaborators or even lab heads.

Importantly, when discussing productive competition, Chang also needs to assess what happens if ongoing competition has finally rendered a "winner": First, the fact that one theory "won", or rather that one theory was shown to have serious shortcomings in respect to the other theory, should not lead to monopoly. On the contrary, pluralist practice should ensure that a field is always open for new competitors and that the costs of entry are affordably low. Additionally, arguing conservative pluralism, even if one system fails to fulfill even the aims of its own supporters, it should be kept around, so that those aims, as long as they appear valuable do not get forgotten (Chang, 2012, 283f).

After having provided a survey of the benefits of toleration and interaction discussed by Chang, I will now discuss problems that might arise if embracing one or many of these benefits.

Chang's arguments for the benefits of toleration involve either practical implications or the capability of making everybody happy. Whereas I agree that both "hedging one's bet" and the "division of the domain" are benefits that would be evident also to people that do not embrace pluralism, I believe that especially his last two arguments that focus on satisfaction and happiness about pluralism will not meet the demands of someone who appreciates the beauty of monism.

Importantly, the benefits of interaction Chang provides need both to be reconsidered in the light of science policy as well as the aims of individual researchers. I believe the process of "integration" rather appears to be a task of reviewers or philosophers of science but might not be the actual practice in the common lab. Thus, to me it seems to be a very pressing question to ask, how cooperation or shared projects between two research groups with different systems of practice would look like and what their results would be given that they are willing to cooperate. Also, concerning publishing of experimental results it seems interesting to me whether this would require special journals for "integrated pluralist science". Especially because Chang in the end opts for scientists to become pluralists such scenarios should be discussed in more detail. Therefore I conclude that very taunting problems for Chang's active pluralism might not only lie in fostering multiple systems of practice but also in the laboratory practice that might, as a consequence, be demanded of scientists.

Additionally, I believe that the circumstances in which both cooptation and competition is possible need to be discussed more thoroughly and also framed by an assessment of science policy that needs to be applied in order to foster those benefits. For example, both cooptation and competition is only possible, if there are sufficient reasons for even engaging with a certain field of practice. Additionally, cooptation with certain systems of practice might be harmful for track record and scientific reputation. Thus more thorough deliberations on the science policy framing his pluralism need to be provided by Chang.

After having assessed the motivations and benefits of pluralism I will now finish this section by discussing two other arguments for pluralism, which will be important when discussing the work of Kitcher as well as when assessing the case of Lamarckism and Darwinism.

Liberal Democracy – A Pluralist Case?

In the tradition of most philosophers arguing for scientific pluralism, Chang draws an analogy from liberal democracy: this institution gives room for many different opinions; even radical ones can be tolerated if they do not outnumber the moderate ones. Thus, he claims that the

scientific community is also a society and should adopt the principles of good government in a democracy. Also, science is based on dialog – be it written or spoken – what might be an analogy to the voices of the different parties in the parliament. Vice versa he attests an epistemic feature to political pluralism, as many different religions and cultures do co-exist, then this is also true for their beliefs (Chang, 2012, 258f).

Discussing further benefits of liberal democracy Chang expects that those claims are adopted for science: Although a two or more party system might be less efficient than one totalitarian party that does not need to seek consent for its sanctions for others, liberal democracy is more protective. This assumption leads Chang to discuss peer-review and the reliance on good will and good judgment of the peers. Thus, also science should consider the rules of its governance and should be always alert to prevent its turning into an oligarchy or mob-rule (Chang, 2012, 258f). While only listing Chang's arguments at this part of the chapter, I will discuss a critique of the possibilities of plurality and "real" liberal democracy in science in chapter 3.2 and will now turn to the problem of Kuhn loss.

Scientific Revolutions and Kuhn Loss – Prescriptive Pluralism

Chang has great sympathies for Thomas S. Kuhn's theories on scientific revolutions, normal science and paradigm change. Nevertheless he attests two short-comings that his pluralism is able to alleviate. First, Chang claims that the Kuhnian model is not capable of explaining how a new paradigm can arise out of a field of monist normal science. Thus, stating that pluralism is and should be an inherent feature of science would explain why it is even possible for a new paradigm to come up, namely that some actors on the scientific landscape followed alternative goals, asked alternative questions and applied alternative explanations and methods. Second, Chang views his pluralism to be prescriptive against so-called Kuhn loss (Chang, 2012, 224), of which I will give a short definition in the following paragraph.

The phenomenon of Kuhn loss countered the traditional picture of knowledge-accumulation. Although it is an requirement for each scientific revolution that the new paradigm needs to solve a majority of the puzzles its predecessor could solve, some qualitative explanatory power is lost and some important questions are not asked any more, as they are not in the scope or the immediate interests of the new paradigm (Kuhn, 1962, 99f). To cure Kuhn-loss, Chang prescribes pluralism as an antidote, as it both accepts new paradigms, theories and systems of practice but also preserves good workable systems (Chang, 2012, 224).

Of course, when asking what would have happened if scientists would have kept certain paradigms and systems of practice alive and invested intellectual as well as economical goods in their further exploration, this begs the question of what would have happened if fewer scientists would work on the "standard" paradigm. I will discuss this problem in more detail in chapter 4.2 and will now assess Chang's pluralist position in more detail.

Active, Epistemic, Normative Pluralism

Chang describes his position as "active normative epistemic pluralism". Thus, his stance is active, in the sense that he aims at actively cultivating pluralism. In his view, "pluralism is an ideology of sciences aimed at promoting plurality in order to reap its benefits" (Chang, 2012, 269). Thus, active pluralism means, that in monist areas, the active pluralist aims at cultivating multiple systems of practice. This carries also important implications on how we should engage in the history and philosophy of science.

Chang's position is epistemic, because it defines how we should acquire knowledge. Importantly, he does not make any metaphysical statements about the world. He holds that no matter what structure the external world might really have there are always strong arguments for epistemic pluralism (Chang, 2012, 268). It is important to note that Chang's pluralism is not metaphysical, as he does not think that such premises can be supported well enough. Additionally, he also does not make any ontological assumptions about pluralism and also does not believe in any multiple-worlds system (Chang, 2012, 292f).

Chang's position is normative as it demands that if there should be a field of science, which is monist one should go and reform it. Necessarily, this part of his notion requires also to be integrated in a conception of scientific values (Chang, 2012, 269). Thus having identified the three basic principles that underlie his version of scientific pluralism, I will now discuss Chang's notions towards realism, monism and relativism in order to contrast his views with the positions of other pluralist authors in a following chapter.

Realism, Monism, Relativism

Realism

Chang offers a very simple definition of realism, which interprets it as a commitment to engage with external reality. Reality is defined as whatever is not subject to one's own will. Chang's position towards realism is specifically strong, as he "needs" a commitment to realism to strengthen his case for pluralism. Therefore he postulates what he calls "Active

Scientific Realism", which aims at maximizing one's learning from reality by not choosing between contradictory theories, but accepting them as they yield different approaches to maximize one's grip on reality. It is the choice of the individual to generate logical contradictions between different approaches or to permit them to be independent from each other and to embrace the possibility of working with both system and enjoy their different merits (Chang, 2012, 215ff).

Chang distances his view from standard realism as this position would not be compatible with his pluralism. He defines standard scientific realism as the view, that scientists try to discover how the universe really is and that they have been quite successful in doing so. In Chang's opinion it is not possible to infer truth from success, as real-life success is limited, relative and provisional. What Chang also dislikes about the classical realist's stance is the notion, that there exists exclusive truth in a given domain which can be instrumental for refuting any other possible explanation. Thus, combined with the assumption that science does very well in finding "real truths" this position is very hostile towards the cultivation of multiple systems of practice. Importantly, Chang also finds truth not to be a necessary criterion for realism (Chang, 2012, 224). Additionally, Chang aims at re-orienting the debates about scientific realism away from metaphysical assumptions about truth and justified knowledge and towards a call for action to examine what is out there: "From the active realist point of view we should want to have multiple scientific systems of practice because this gives us more angles on reality"(Chang, 2012, 217). Thus, standard realism would also impede and render as useless search for other valid, alternative theories that describe and explain the exterior world.

Conclusively, Chang offers a very strong conception of monism compared to standard definitions, which has important implications on the pluralist practice he proposes. In the following section, I will discuss Chang's position concerning monism.

Monism

When trying to counter monism, Chang proceeds in two steps. On the one hand, he wants to show that not monism, but pluralism should be the "ultimate goal" of science as on the other hand, the aims of science can be served better by cultivating multiple systems of practice. In Chang's opinion, monists can be tolerated in science when two caveats are met: 1) not too many people should be monists to preserve benefits of toleration and interaction; 2) monists need to be tolerant about pluralism (Chang, 2012, 294ff). Conclusively, Chang's rejection of monism is based on his normative assumptions about the superiority of scientific pluralism,

but does not make any metaphysical claims. Thus it would not alter his position, if the exterior world was shown to be metaphysically monist. Lastly, I will discuss Chang's rejection of relativism before proceeding with discussing consequences of his version of scientific pluralism.

Relativism

As the most fundamental difference between pluralism and relativism Chang refers to the fact, that pluralism does not renounce judgment and commitment, as is the case for relativism. His pluralism involves both the willingness and the capability to deal with opposing assumptions and theories in a productive way (Chang, 2012, 261).

Importantly, Chang argues that relativism does not necessarily imply pluralism. In his view, relativism makes no metaphysical stance about whether or not there are or should be multiple alternatives, but just claims, that if there are some, they should be treated equally. Additionally, relativism does not claim that there should be alternatives as Chang's normative pluralism does. Thus, it is far more passive than Chang's pluralism, as it does not search for possible alternatives, but only deals with them, if they arise. Chang's strongest and normative claim is that cultivating a multiplicity of systems of practice is beneficial for science. This leads him to the view that pluralism can actually do more effectively something against absolutism than relativism. Additionally, if relativism is to be interpreted as the position, that theories are chosen randomly or not at all, then both pluralism and monism are equally far away from that position (Chang, 2012, 261ff).

Thus, Chang claims "many things go" instead of "anything goes". This position makes it of course harder for the pluralist than the relativist, because she has to discern, which things go, whereas the relativist could permit every position without deliberations. To counter this argument, Chang claims, that neither monism nor pluralism are released from the responsibility of that kind of judgment. As a good starting point, Chang prescribes to approach any scientific question as one would normally do, but choose not only a winner but also a second or even a third prize. In fact monist judgment is harder than a pluralist's as it is neither clear how the scientific methods for such decisions are defined nor who defines it (Chang, 2012, 262). The last section of this chapter will focus on assessing the consequences of pluralism Chang proposes and leveling critique concerning certain points.

Consequences of Chang's Pluralism

Importantly, Chang's pluralism might seem theoretically very alluring however there are several practical implications. In the following section I will provide a survey on issues and

topics Chang tackles himself as well as on the consequences his campaign would have on history and philosophy of science. In the end, I will close this section with a discussion of problems that immediately arise through Chang's elaborations, but I will especially discuss further problems that would arise through pluralist practice both in chapters 3.2 and 4.2.

Chang gives several responses to the question, whether pluralism is economically affordable, in terms of time, money and talent. On the one hand he argues, that it is certainly not impossible to afford more than one line of inquiry, however, the more pressing question is, how many other lines of inquiry should be afforded. On the other hand, he states that exploratory research is often very cheap, and thus it would only cost some salaries and a tiny bit of academic freedom to provide to some unorthodox thinkers (Chang, 2012, 266ff).

Additionally, Chang provides another argument by claiming, that the history of science shows, that pushing scientists only into one direction of science might not be as fruitful as hoped and might stall scientific progress. As an example he takes the over-commitment of physics with string-theory, which has not yielded that much exciting insights as one might have hoped for (Chang, 2012, 267). Another example would be the current state of pharmaceutical research, which mostly aims at identifying new generica of substances that are already in use, instead of following entirely new lines of research (e.g. Spellberg et al., 2008). Thus Chang requires reconsideration of whether money is really most efficiently spent on different lines of work. Therefore he concludes that one should not take for granted that monism is affordable. Additionally, Chang claims that it is much more cost-intensive to resurrect a once discarded theory if it appears that it might have had its justification than having kept it around all the time (Chang, 2012, 267f)

Finally, Chang argues, that monism might be the cause, why too few people get interested in science. He identifies this as the reason for too little funding and too few young, innovative scientists. Thus he claims that more debate and more pluralism will get more people inducted into science (Chang, 2012, 268). After having discussed the consequences on scientific practice, Chang proceeds in assessing the duties and responsibilities of the history and the philosophy of science.

In Chang's view, pluralism should become a doctrine about knowledge-building and not just knowledge-evaluation. He believes that history, especially pluralist historiography, might play an important role in the re-evaluation of scientific discovery and as a corrective concerning the common views on the history of science. Also, it will help the history of science to get

away from triumphalism, namely from exclusively writing the winner's history. Chang's pluralism contains three directives for historiography: 1) re-visiting triumphalist history and paying close attention to the losing side; 2) turn away from the focus on closure and consensus-formation; 3) establish plurality as a normal and to-be-expected feature of science (Chang, 2012, 286).

According to Chang, pluralism has also great potential in reshaping philosophical theories on science. For example, the discourse on theory-choice is strongly shaped through monist assumptions, obscuring the possibility of multiple theories even in a state of Kuhnian normal science. Also it needs to be reconsidered, why moments of victory are privileged to those of competition (Chang, 2012, 288f).

Chang sees it as the duty of an integrated history and philosophy of science to proliferate a multiplicity of scientific systems in order to complement monistic orthodoxy. Historians and philosophers of science should see themselves as guardians of an endangered scientific theory species. As a next step, systems of knowledge, that were cut off unjustifiably, should be revivified. In general, the aim of active, normative, epistemic pluralism is to improve science by cultivating multiple systems of knowledge (Chang, 2012, 289f).

In the last section of this chapter I will level two lines of critique against Chang′s prescriptive scientific practice. First I want to question, why Chang thinks that establishing pluralism would be very cheap. This certainly cannot imply "very cheap" for the individual, as neither the instruments nor the day-to-day supplies are usually affordable. In fact, Chang argues, that through a negligible amount of encouragement, amateurs and enthusiasts would invest their own resources in certain lines of inquiry. But even if researchers like Priestly, Dalton, Jenner, Fleming, Tesla and the young Einstein could provide for themselves I seriously doubt that the cost-intensiveness of cutting-edge science could allow this nowadays.

Additionally, the ongoing specialization of the fields and the increasing incapability of researchers to understand the theoretical background of only slightly distant research fields is a well-recognized fact. This leads me to uttering my concern that it might simply not be possible for amateurs and enthusiasts to try to answer questions they would like to see answered. Nevertheless, Chang makes a good point when he claims that at least on the theoretical field, amateur self-funded research could yield important and exciting pluralist possibilities (Chang, 2012, 267f).

If Chang means that fostering scientific pluralism is "very cheap" for institutions there arise several problems: Fostering entirely new lines of research is not "very cheap" for public institutions. Doing plurality exploring research might be affordable for heavily funded private organizations, but those are usually strongly focused on some certain outcome and might have neither economical nor intellectual interest in fostering complementary science. Therefore it seems that institutionalizing pluralist science would be the only way to establish it.

Importantly, I believe that the hypothesis, that public debate induced through pluralist science will cause immediate interest in science and will lead people to becoming scientists needs to be supported with some historical evidence in order to be worthy of consideration. Thus, it would be interesting to research interest in science in bygone centuries, where scientific debate was much fueled by a controversy of public interest. At least for eras where university subscription numbers can be looked up this would for example be a good indicator for the thesis, that debate fuels interest of the public into science. Also for very recent discussions, such as on the benefits and harms of Genetically Modified Organisms (GMOs) it would be interesting to investigate whether it really is a common measure of people to become scientists themselves in order to help resolve the debate. One could also try to make the same observation for creationists and ask whether they get encouraged to acquire the professional requirements for entering the scientific discourse.

2.2 Discussing Plurality

In the following I will provide an account of two other positions favoring pluralism discussed in Dupré's ***The Disorder of Things*** and Kellert, Longino and Water's ***Scientific Pluralism*** and will discuss possible problems that arise when embracing their respective notions. After having sketched the outlines of those stances I will discuss where consent as well as dissent between those positions is located. Most importantly, I will discuss their positions towards monism, relativism and realism.

Outline: Dupré – Pluralist Stance

John Dupré – The Disorder of Things

In his monograph Dupré advocates two theses: (1) Science can never constitute a single, unified project. (2) There is an extreme diversity of the contents of the world. Thesis (2) shows the inevitability of theses (1). Thus, his pluralistic assumptions are metaphysical and he claims to describe an intrinsic feature of the world, namely *the disorder of things*. The disorder of things means a rejection of the notion, that the world is deterministic, fully law-

governed and potentially intelligible. Thus, the disorder and disunity of science follows from the disorder of things (Dupré, 1993, 1).

It is Dupré's opinion that three scientific doctrines – essentialism, reductionism and determinism – obstruct the awareness of the metaphysical character of the exterior nature. Thus, he tries to reject those three stances in his work, to provide a basis for his pluralist positions. In the following I will turn to sketch his notion of metaphysical pluralism and the disorder of things and finally assess his notions concerning realism, monism and relativism.

For arguing his metaphysical pluralist position, Dupré discusses the notions of a unified science and a well-ordered external world. In contrast to those positions, Dupré argues that any motivation for a unified science becomes questionable and thus advocates scientific disunity.

First he discusses the "Unity of Scientism". Scientism he coins as the process, that the attribute "scientific" became an epistemic, honorable predicate and thus gained "pseudoepistemic power" merely through the fact that certain institutions claim epistemic authority, thus generating a sociological unity. As an example, Dupré states that mathematics has exaggerated prestige amongst the sciences. This is partly due to the fact, that people believing in determinism and an ultimately orderly universe hope that mathematics will be the language to capture the metaphysical structure of it. Importantly, Dupré argues, the notion of sociological unity needs to provide a strong criterion of demarcation of sciences from pseudosciences, as else "questionable candidates could sneak in under the cloak of sociological unity" (Dupré, 1993, 224). In the same manner Dupré discusses and refutes several approaches for a unity of science, as he tries to argue, that both theoretical and methodological approaches towards unification of science fail and that also sociological explanation cannot make a strong point for scientific unity.

Rather than advocating a unifying concept for all sciences, Dupré argues, that science should be seen as a Wittgensteinian "family resemblance concept", meaning that there are many features that are true for some but not all sciences. Thus, although it is not easy to discern what makes all sciences be together in an enterprise, it seems intrinsically right that they are. To give an example, the role of theories, empirical evidence and institutional authority varies greatly amongst different sciences (Dupré, 1993, 242). Also, Dupré attests the impossibility of maintaining a demarcation criterion and suggests to replace descriptive and apologetic epistemology with an epistemology of virtues including "sensitivity to empirical

fact, plausible background assumptions, coherence with other things we know, exposure to criticism from the widest variety of sources" (Dupré, 1993, 243). This approach, he argues, has the capacity to capture a rich variety of projects of inquiry, without conceding Feyerabend's programmatic scientific relativism of anything goes. With this conception at hand, good science can be distinguished from bad science, but demarcating science from non-science will be rendered impossible. Thus, while leading to a plurality of science, Dupré subsumes that his conception embraces a unity of knowledge (Dupré, 1993, 243).

Importantly, what I find problematic about Dupré's argumentation are the ambiguities between an empirically argued but metaphysically motivated form of scientific pluralism. If the evidence he provides for the refutation of essentialism, reductionism and determinism is meant to be decisive material, then it is necessarily not sufficient to draw examples from biology to conclude about all sciences. Additionally, it is not clear, whether essentialism, reductionism and determinism are positions that are essential for arguing monism or different forms of monism. Also, his claim that methodological, theoretical or sociologically explained unity is impossible is argued from an as-of-today shaped perspective and might not be sufficient to argue metaphysically.

Considering his argumentation it is not clear to me, in which way his empirical data and his metaphysical stances can be interrelated and thus I suspect, that he treats his evidence similarly to Chang, who claims, that individual refutations of historical claims he makes cannot harm his pluralist position, as it is basically independent of historical corroboration. Nevertheless, I find it important that Dupré opens the field for engaging with biology for arguing one's philosophical stances, as I also believe that this field will prove most fruitful for such endeavors. Concluding this section, I will now turn to discuss Dupré's stances towards realism, monism and relativism.

Dupré believes that the acceptance or refusal of realism should have no influence on one's ontological style or metaphysical beliefs. Nevertheless, he holds that his pluralism and his postulated disorder of things is no obstruction to the reality of things, as even though they can be classified into various overlapping kinds of things this does not interfere with their reality. From this it follows, that none of his positions force the abandoning of realism. Thus, Dupré's realism is promiscuous: scientific kinds are not illusory or unreal, but promiscuous in respect to who is applying the classification (Dupré, 1993, 262).

Importantly, Dupré gives no specific account of reasons for rejecting monism, but as he launches an assault against essentialism, reductionism and determinism it becomes quite clear that he does reject monism. Also, as his thesis is metaphysical, by assuming general scientific pluralism, there is no room for monism in his conception. Importantly, reductionism is a very important tenet of monism and scientific unification, thus his attitude towards monism seems quite clear.

When discussing whether his position is relativistic, Dupré's answer is two-folded: on the one hand, he states that the theories the sciences come up with are relative to the purposes for which they are intended. Thus, in respect to these considerations, Dupré's position is relativistic. Importantly, on the other hand, Dupré rejects the notion that any scientific account could be true, or that all scientific theories have equal epistemic credentials and value. Additionally, Dupré's position maintains the possibility of normative statements, given by the postulation of his epistemology of virtues: there are plausible and incredible scientific practices that might range from extremely valuable to entirely pernicious (Dupré, 1993, 261f). Finally, a third position, the "pluralist stance" of Kellert and Longino and Waters will be introduced before contrasting all discussed positions.

Kellert, Longino, Waters – The Pluralist Stance

When discussing the pluralist stance argued by Kellert, Longino and Waters I will draw from two sources to illustrate this position: First, I will sketch their "pluralist stance manifesto" they advocate in the introduction of their co-edited volume ***Scientific Pluralism***. Second, I will on the other hand take under account Longino's 2002's monograph ***The Fate of Knowledge***, which picturesquely illustrates her pluralist position.

Kellert, Longino and Waters state that the origin of their conception of pluralist thinking lies in the fact, that researchers of various scientific areas find the explanation and investigation of a natural phenomenon with one single theory or one single approach increasingly taunting. The authors believe that the world might be simply too complicated as well as too underdetermined and believe that our cognitive interests might be too diverse for monist interpretations. This leads them concerning particular issues and the philosophy of science in general to embracing methodological as well as explanatory pluralism. The justification for adopting pluralist views lies for the authors in empirical studies of particular scientific cases.

In principal, this means, that pluralism is to be accepted, whenever two or more theories, methods or explanations concerning one phenomenon cannot be reduced to a monist

account. Whereas monism would interpret the multiplicity of approaches or theories as a deficiency, a pluralist accepts plurality as an inherent feature of science. Thus, it is the core feature of the pluralism advanced by Kellert, Longino and Waters that the premise that the world or parts of the world can be fully described by a monist account should be treated as an open and empirical question and should be examined for each case exclusively. The authors state, that the denial of pluralism is metaphysical prejudice and leads philosophers to three different kinds of errors: 1) to minimize or overlook important scientific approaches; 2) to dismiss from consideration legitimate scientific approaches that seem to lie outside the mainstream; 3) to exaggerate the explanatory importance of scientific approaches that are mainstream (Kellert et al., 2006, xii).

Kellert, Longino and Waters try to place their notion of pluralism somewhere between "radical" and "modest" forms of pluralism, as in their understanding "radical" forms are often not distinguishable from relativism and "modest" forms can be reduced to monism. Their so-called pluralist stance does not make any metaphysical assumptions and denies that there are any a-priori reasons for accepting monism. They characterize their stance "as a commitment to avoid reliance on monist assumptions in interpretation or evaluation coupled with an openness to the ineliminiability of multiplicity in some scientific contexts" (Kellert et al., 2006, xiii). They believe that there is sufficient evidence for the proposition, that there are several factors which determine phenomena in the world which cannot be subsumed under a single theory. Thus, multiple accounts produced by a multiplicity of approaches will yield a more fully rounded picture of the world.

A pluralist defending the pluralist stance is open to cases that both show to be reducible to a monist explanation and to such cases where this is not the case. In the opinion of Kellert, Longino and Waters there are certain constraints that limit the number of possible explanations. Nevertheless, the pluralist stance does not worry about tolerating incoherence, which is often leveled as an argument against stronger forms of pluralism. The authors hold, that deciding between two approaches that are not intertransable but both produce knowledge will cause a loss of knowledge. Also pluralism preserves explanatory flexibility. The authors propose an evidence-based form of scientific pluralism, which initially considers that the world might not yield purely monist explanations. To support this claim they provide within their book empirical evidence from other contributors, which corroborates this proposition. Also, they believe, that accepting pluralism is more faithful to the scientific situation than blindly claiming monism (Kellert et al., 2006, xiiif).

The authors believe that pluralist thinking shapes science in several different ways. On the one hand, it will alter the way, scientists deal with explanations that seem to be replaced by a "better" one. As argued by Kellert, Longino and Waters the acceptance of the pluralist stance will lead to nourishing multiple theories and will reject discarding theories that produce knowledge in the course of scientific development. The pluralist stance also includes the demand to revisit the classical concept of the counterexample or *experimentum crucis* and adjust it to pluralist thinking (Kellert et al., 2006, xxivff).

To further illustrate a detailed conception of pluralism of Longino, I will close the section on the pluralist stance by a discussion of her monography ***The Fate of Knowledge*** and subsequently criticizing some tenets of this position.

For Longino it is not clear, how investigative strategies might be evaluated. If incompatible theories both produce usable results, she does not see the need to discard one. Also, when accepting strong forms of pluralism, not only the concept of the objectivity of science gets destabilized but also the metaphysical unity of the world needs to be revisited, as when talking about incommensurable equally good theories, a way needs to be found, that avoids talking about multiple worlds. It is Longino's notion that accepting multiple worlds – even in a very moderate, Kuhnian sense, who illustrated his notion with the statement that scientists holding incommensurable and incompatible theories occupy other worlds – leads to the fragmentation of scientific worlds (Longino, 2002, 188f). Notably, pluralism envisioned by theoretical pluralists is an assumption about one empirical world, not many.

Similar to what is stated in the pluralist stance, Longino stresses that pluralism needs to be seen as an empirical claim, which finds its support in contemporary science and not as a metaphysical a-priori statement. From these assumptions there might arise a problem of incommensurability when pluralism is countered by monism, thus Longino suggests that even if plurality is eliminable, epistemology should not presuppose but test it. Therefore, Longino provides guidelines for accommodating with pluralism, which aim at impeding presupposing neither pluralism nor monism.

1. "The plurality of representation in the sciences may be a function of how the world is or of human intellectual equipment for and interests in understanding the world (or of both). Our epistemology cannot dictate which. Thus:

2. A satisfactory epistemology should be open to theoretical plurality or theoretical unity being the final result of inquiry, or what amounts to the same thing:

3. The issue of theoretical pluralism ought not be decided by one's choice of epistemology

4. A suitable humility requires a modest epistemology. An epistemology – as a theory of human knowledge – does not have to promise complete knowledge (or trade in other absolutes, like certainty) but ought rather to give sense to the distinctions and normative judgments that are a part of epistemic discourse." (Longino, 2002, 95)

Another very important notion Longino discusses is black-boxing: After the scientific society settles its debates on a certain matter, it is subject to community consensus. It starts to be seen as inevitable and an expression of nature, thus, it gets black-boxed. The theory becomes itself the basis for further theories and origin and justification do not matter any more (Latour, 1987, 2f). Although plurality is generally expected as a feature of ongoing debates, it nevertheless should also open debates about "stabilized" knowledge, as through black-boxing pluralism gets eliminated from the field in question (Longino 2002). Longino states, that pluralist approaches to black-boxed knowledge offer a more adequate reading of such cases (Longino, 2002, 189). After having discussed the premises of the pluralist stance in detail, I will now offer some critique on this position and then turn to assessing their notions concerning realism, monism and relativism.

As Kellert's, Longino's and Waters' stance is very cautiously argued, it is hard engaging into critique except as to criticize its tentativeness. This is what e.g. Chang does and what I will sketch in the following section. Still I believe that there arises a certain problem with their empirical motivation.

Of course, that the existence of pluralism or monism is treated as an empirical question, what has to be discerned for every scientific field and even for each scientific question separately, is somehow problematic. Thus, the issue needs to be put forward, how the authors imagine the possibility of a final decision on whether the world is pluralistic or monistic and what would characterize the corroborating data that allows making such a decision – at least for one field. As they seem to assign themselves to be the referees, they need to know, when a goal was scored and when they have to blow the final whistle. Unfortunately, their argumentation does not yield any guidelines concerning such issues, and it remains in question, whether they could provide examples, where they believe that a phenomenon can be described in a monist way. In the following, I will give a short account on the notions of the pluralist stance on realism, monism and relativism.

The pluralist stance argued by Kellert, Longino and Waters suggests a "tempered realism". This means, that our scientific data – be it now really in touch with the exterior world or not – should not be taken to make metaphysical assumptions about nature. The pluralist stance argues, that it might not be the case, that science might ever provide reliable answers to big metaphysic questions (Kellert et al., 2006, xxiv) Thus, as their pluralism is empirically motivated, the authors of the pluralist stance argue that both monist or pluralist interpretations might give an account of external reality, but no data provided by either of the approaches should lead us to feel entitled to answer metaphysical questions.

The authors characterize monists as researchers and philosophers of science who accept plurality as a temporary state of science but believe that through the advancement of science, all notions of plurality will be cleared out of scientific inquiry. In contrast to this view, Kellert, Longino and Waters hold, that it might be the case that in certain areas of science theories and explanations will never be reduced down to a single monist theory. Thus scientific research and produced data should not be evaluated regarding their potential of realizing the goals of scientific monism (Kellert et al., 2006, xi).

Kellert, Longino and Waters are very clear about the relation of their "pluralist stance" and relativism. They argue that their viewpoint does not lie anywhere near Feyerabend's "anything goes". The supporters of the pluralist stance, they claim, are able to discern and refute poor research, although they do not state by which methods. Additionally, through treating the assumption that there can be a single, monist account of the exterior world as an open question, a supporter of the pluralist stance can normatively assess approaches, that aim to promote themselves by demolishing opposing approaches (Kellert et al., 2006, xiii). After having discussed several positions towards scientific pluralism I will now turn to discussing the reception of each individual notion by the other authors mentioned.

Reception of the Different Positions

As Dupré's monograph was the first to be published both the authors of the pluralist stance, as wells as Chang have criticized his position. On the one hand, Kellert, Longino and Waters attest Dupré's metaphysical pluralism to be one of the most "radical" kinds and thus find it to be in dangerous vicinity to relativism. Countering Duprés´s views they argue, that they believe that "there are constraints, that limit the variety of acceptable classificatory or explanatory schemes" (Kellert et al., 2006, xiii). On the other hand, the authors interpret his case studies as leaving the question open, whether there are such constraints or not (Kellert et al., 2006, xiii).

While Kellert, Longino and Waters reject Dupré's position because of its "radicality" and tendencies of relativism, they do not have a specific critique on the level Dupré places his arguments. As the authors emphasize, they treat the question whether there exists pluralism in certain fields of science empirically, they might not accept Dupré's metaphysical assumptions on the existence of pluralism. Also, they would not regard case-studies in the field of biology as an argument for assuming global pluralism in every field of research.

In contrast, Chang gives a specific critique on the metaphysical and ontological basis of Dupré's arguments. Making such statement about the exterior world is as unverifiable as making such assumptions concerning monism. Also, Chang refrains from all statements about what the exterior world consists of (Chang, 2012, 292f). Additionally, one might argue, that Dupré's elaborations do miss Chang's emphasis, as he wants to stress a commitment to active pluralism and cultivation of many pluralist systems and not to make general attestation of the structure of the world. Importantly, Dupré states at the end of his considerations, that his position is open and tolerant to the possibility of occurance of some possible candidates for real essences, in such a case, Chang would claim that one should make an effort to look out for plurality. Importantly, Chang does fully embrace Dupré`s refutations of reductionism as he levels three points that are quite similar to Dupré's notions: 1) reductionism does not seem to have an end in the near future, as every proposed "atomic" particle was shown to be further dividable; 2) things do not get simpler when going deeper; 3) Wholes can be simpler as their parts and much more easy to understand (Chang, 2012, 235).

Chang provides a thorough discussion of the pluralist stance. On the one hand, Chang believes that the pluralist stance is way too passive and that pluralism should consciously go beyond interpretation to actively promoting the advancement of science (Chang, 2012, 292). On the other hand, he holds that the pluralist stance will eventually boil down to his pluralism: as to empirically assess whether in a given scientific field there are multiple incommensurable theories requires the cultivation of such, this practice is equivalent to Chang's active pluralist agenda (Chang, 2012, 292).

Also, Chang criticizes the experimental character of the pluralist stance's approach: on the one hand, it is hard to believe that a monist would engage in testing out pluralism, but rather people who are already prone to pluralism. On the other hand, it does not seem possible to decide, when the experiment is finished, meaning, when pluralism and monism have been both tried out, that anyone would go back to monism, as by the time the experiment had

advanced to some point, everybody will be promiscuous concerning monism and pluralism in Chang's opinion (Chang, 2012, 293).

2.3 Commitment to Action – A New Classification?

In the last two chapters, I discussed three different approaches towards pluralism and their notions concerning monism, realism and relativism. The discussion makes it evident, that the individual stances of Chang, Dupré and Kellert, Longino & Waters vary substantially in their commitments and implications. Therefore the question arises, whether the classification into radical and modest forms of pluralism would give a good account on these positions.

First, the definition of "radical" and "modest" seems problematic. As it was not discussed before because no pluralist position I reviewed falls into this classification, I will briefly sketch the characteristics of modest pluralism, which are twofold: Either a modest pluralist claims, that there are different theories for different phenomena of the exterior world however, by some method or the other, those theories are intrinsically translatable. Another possibility for being a modest pluralist is accepting the current plurality of science, without being enthusiastic about it but believing that the advancement of science will lead to unifying theories. Concerning radical pluralism, Kellert, Longino and Waters discern antirealist and realist positions. Anti-realists are constructivists that claim that human ingenuity is the only constraint for possible theories, whereas realists such as Duprè claim that nature offers indefinite ways of classification (Kellert et al., 2006, xiii).

Kellert, Longino and Waters state, that radical forms of pluralism are not distinguishable from relativism. Importantly, they claim that the most important feature that distinguishes their position from relativism is the fact, that they do not believe that there are unlimited "ways of classification" but that there are some constraints. First, "ways of classification" needs to be expanded to a more general term, as this critique was mainly leveled against Dupré's notion of pluralism. Thus, generalizing their claim would mean, that they belief that there are constraints to possible accounts of the world. Considering this, it is important to determine if this is a metaphysical or a normative statement. If the latter is the case, then Dupré would also agree on this as he clearly states that he wholly rejects the notion that all scientific beliefs are of equal worth. Thus, he is not of the opinion, that when accepting plurality, one becomes unable to judge, accept and dismiss certain theories. Now, if Kellert, Longino and Water`s statement is interpreted metaphysically, then it loses its strength and also becomes quite implausible: Claiming, that there is some constraint to the plurality of

theories, meaning that there is a final number of plausible theories is not verifiable and does not fit into their general account of pluralism.

These considerations show, that attesting radicalness to Dupré's position because of relativist inclinations is not possible. Another reason, why Dupré's position is rendered as radical might be that he makes a metaphysical statement about plurality whereas the pluralist stance is much more tentative and only empirically motivated. Thus one could question whether it is a correct attestation to claim that Dupré is "more" of a pluralist than Kellert, Longino and Waters. If this is the case the attribute "radical" completely fails to describe this relation. Also, the reason why Dupré seems to be more of a pluralist lies within the nature of their claims: Dupré's is metaphysical although based on evidence, whereas the pluralist stance only holds that monism should be no a-priori metaphysical assumption for every respective scientific field. In this case, attesting radicalness for arguing on a different level than they are also seems to be not the optimal practice for classifying pluralism.

Enter Chang. Considering Chang's pluralist position even further shows that the dichotomy radical – modest yields no good way of classification. Similar to Kellert, Longino and Waters he does not make any metaphysical statement about the exterior world. Naturally, he is no modest pluralist as he neither claims translatability of different systems of practice nor does he believe that in the future incommensurable theories will be reduced to one single account. As the pluralist stance is placed by its author in the middle of the radical – modest –scale, one might ask whether this would also be a good place for Chang's pluralism. Notably, Kellert, Longino and Waters identify the most intrinsic characteristic of their pluralism as their empirical motivation. This empirical motivation includes accepting both monist and pluralist cases. Chang's pluralism certainly does not accept monist cases, but its commitment to action would demand planting next to each monist flower at least three other ones. Also, empirical motivation is no core feature of his stance. Although deliberations on his pluralism are preceded by a case study concerning the Chemical Revolution and the potential benefits of keeping phlogiston theory around, Chang himself states, that the case-work is not necessary for his pluralist position (Chang, 2012, 254; Chang, 2015, 97). Thus, Chang wants to motivate people to follow his example and perform empirical case-studies in order to cultivate pluralism, but does not require empirical evidence as prerequisite of scientific pluralism.

These considerations seem to suggest moving Chang's position nearer to Dupré on the radical/modest scale. Thus, one needs to ask about the nature of Chang′s radicalness. A starting point would be his normativity and activity. Concerning this, Chang's position is

unique in respect to both Dupré and Kellert, Longino and Waters. Whereas the disorder of things says "pluralism is" and the pluralist stance says "pluralism might be", Chang's position states "pluralism is good". Thus, for Chang it is of no interest whether monism might metaphysically be the case, he argues that the benefits of pluralism in every field of science outweigh the benefits of having "the" true monist theory. This means, that Chang is radical in the sense that he is inclined to disregard empirical evidence for monism because of actually pragmatic reasons, as assuming pluralism even if there might be none is more useful. This leads to the conclusion, that both Chang and Dupré need to be placed on the radical end of the scale.

Having seated Chang's position next to Dupré's ***Disorder of Things*** it becomes evident that actually the most salient points of their notions are disregarded when attributing both with the same core denominator "radical". Chang does not accept any metaphysical claims, as he clearly states when rejecting Feyerabend's and Dupré's assumptions. Thus, if pluralism needs to be classified, a more sensitive scheme needs to be applied. Therefore I will in the following discuss possible classifications concerning individual stances towards realism, monism and relativism and will close this chapter by proposing a new way of classification, which focuses on commitment to action.

Realism

The discussions of the last chapters tried to assess Dupré's, Chang's and Kellert's, Longino's and Water's position considering three features. First I showed, that all positions maintain realism, although with certain differences: while Dupré and the pluralist stance are somewhat egalitarian towards realism, Chang counters standard realism with active realism which is instrumental for him to state a motivation for scientific pluralism. Thus, as two of the three positions do not regard realism as salient to their positions, classification through commitment to realism seems pointless. Additionally, as Chang introduced a new form of realism, it seems problematic to place him on the same scale as the other two positions (Fig. 2).

My scheme represents the several conceptions of realism of the different authors as follows: I understand realism as the position that scientific theories are providing a literally true account of the exterior world and that there substances in the world to which our theoretical accounts refer. Due to the fact, that none of the authors engages in a discussion about the progress of science, I will omit claims realists make about it. The endpoints of the scheme represent either the acceptance of the definition I provided (‟realism″) or the refusal of its propositions (‟anti-realism″). As both Dupré and Kellert, Longino and Waters embrace

realism in a sense that they state that pluralism does not deny realism and especially because they offer certain restrictions to its concept (e.g. "tempered realism") I placed them in the middle of the scale, as they seem to have certain problems with accepting standard classifications. To my interpretation, Chang fully embraces the concept of realism however, he adds an active component to it as he states that it is not sufficient to make contact with the world with whatever theoretical account we happen to have, but that it is most vital to make as many contacts as possible with as many theoretical accounts as possible. Thus, I placed his account even beyond the endpoint of the scale.

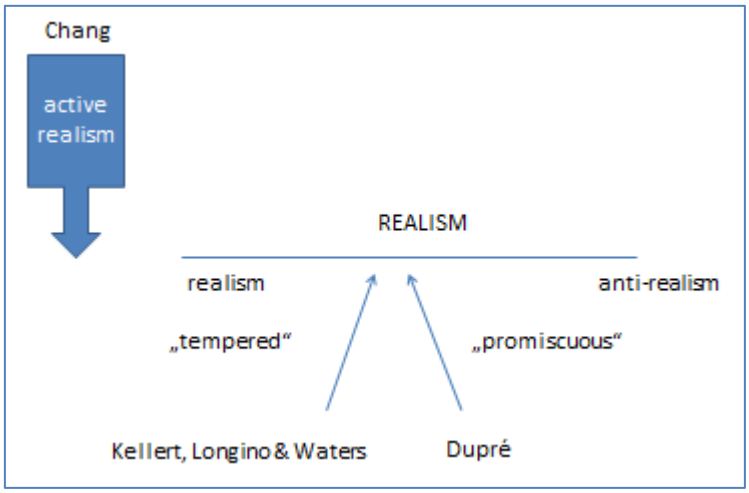

Figure 2 stances towards realism

Monism

Before discussing the various positions towards monism, I will provide my definition of it, which I adopted from the authors of the pluralist stance (Kellert et al., 2006, x). Thus, monism is built on five tenets:

"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 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; To this position Chang adds for 4) that monists also usually claim that there is one best method of inquiry for each subject-matter (Chang, 2012, 260).

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)

While Kellert, Longino & Waters accept cases in which monism seems to be apparent, Dupré and Chang fully reject monism. Again, placing these two positions next to each other seems wrong, as both positions offer completely different motivations for the rejection of monism: While Dupré states that monism is metaphysically impossible Chang has normative reasons for rejecting monism (Fig.3). Thus, the endpoints of the proposed scale represent the acceptance or rejection of possible instances of monism. As the pluralist stance is open for cases that can be fully captured by monist accounts, they are situated at this endpoint. Contrarily, Dupré and Chang refuse monism, however out of fundamentally different reasons. Therefore, I discern two different levels. On the first level, that assesses the acceptability of monism, both positions take the same stance, however, one level beneath lies the level of the motivations, where Chang and Dupré vary substantially: whereas the refusal of monism is in Dupré's case built on a metaphysical claim about the structure of the world, Chang's refusal is motivated by his assumption, that a plurality of accounts is normatively superior to monism.

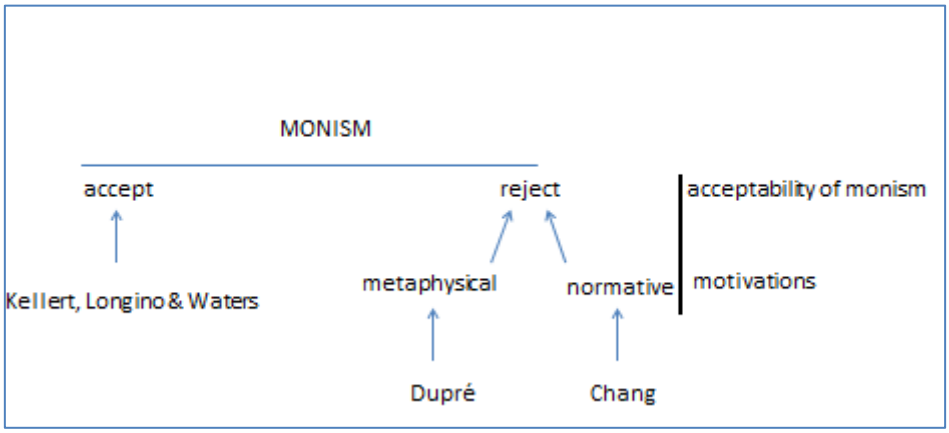

Figure 3 stances towards monism

Relativism

Assessing the various stances concerning their inclination towards relativism shows, that all three positions rejected relativism in the sense that pluralists are unable to judge and discern theories. I do broadly define relativism as a position that holds that scientific knowledge is context dependent and that there are no objective measures to judge between different explanatory ways. Whereas Chang and Kellert, Longino and Waters clearly reject the slightest bit of relativism, Dupré states that he fully embraces the fact that all scientific accounts are

relative to the purpose they are made for. Thus, while the authors of the pluralist stance seem to believe that it is possible to discern stances of pluralism by their respective commitment to relativism, this seems to be not practicable or at least disrespects the reflective assessment of the authors towards their own work (Fig. 4). Thus, while the end points of my scale define full acceptance or full rejection of relativist propositions, Dupré can be placed in the middle, as he tries to preserve the possibility of judgment criteria while he believes that there are endless ways of classifications. Conclusively, both Chang and the authors of the pluralist stance can be placed at the end of the scale, as the fully reject the premises of relativism.

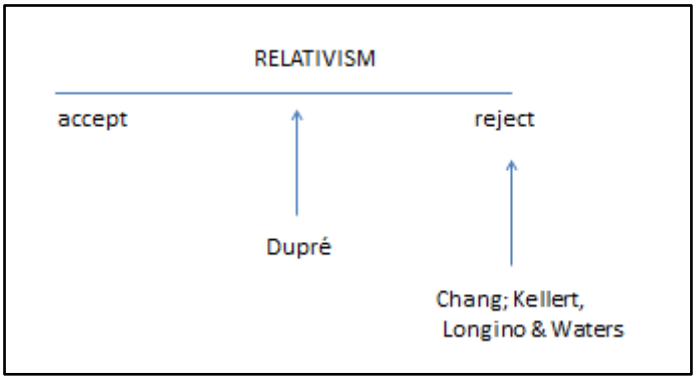

Figure 4 stances towards relativism

Pluralist Action

After having provided several alternative ways of possible classification and having discussed the problems arising with them, I want to introduce a new model that is mainly inspired by Chang's monograph. What seems concerning Chang's active, normative, epistemic pluralism radically new to me is its commitment to action. This is symptomatic in his rejection of standard realism and his concept of complementary science. As I have discussed Chang's commitment to action and his prescriptions for pluralist practice in the previous chapter I will not go into detail about this but rather try to investigate how the "action-potential" of the pluralist stance and the disorder of things can be characterized.

First I will discuss the "pluralist stance". Importantly, Chang tries to show, that this position actually boils down to his pluralism because: 1) the pluralist stance demands empirical assessment of cases to discern pluralism or monism; 2) to assess whether the respective case is a case of pluralism or not, multiple theories/methods/explanations have to be cultivated to be able to judge; 3) When pluralism is already established it makes no sense of removing it again from the scientific landscape. This means, that Chang is of the opinion, that Kellert, Longino and Waters will nolens-volens engage in his commitment to action and as a result accept his normative view. To illustrate his notion he draws an analogy from

testing out both promiscuity and monogamy. Discussing this he claims, that once promiscuous, *once having stepped so far*, there will be no going back (Chang, 2012, 292).

This argument mostly relies on Chang′s judgment concerning monism and pluralism. It reminds of the "Concorde Fallacy", which exemplifies the economical concept of "sunk costs", the assumption, that already spent resources have to be taken into account when deciding whether to stop or continue a certain project. Thus, using a fallacy as an argument why pluralism will be maintained in any case is not convincing. Most importantly, the problem of Chang's thesis lies in premise 3). There is no reason, why people testing out both pluralism and monism will feel the need to adopting Chang's normative statement. It seems equally possible, that experimenters being confronted with the plurality they tried to cultivate themselves want to flee back into comfortable monism. Thus Chang′s commitment to pluralism somehow blindfolds him for other possible outcomes.

The considerations of the last paragraph lead me to suggesting a partial adoption of Chang's view on the pluralist stance, which will lead to a criterion of differentiation concerning the degree of activity both positions imply: I accept premises 1) and 2) for Kellert′s, Longino′s and Waters′ pluralist stance, as they capture quite well what their program implies. Further I argue, that after having performed requirements 1) and 2) the philosopher having adopted the pluralist stance will start assessing the cultivated pluralism to answer her empirical question: is this field pluralist or not? Is there any support for keeping any of the cultivated systems of practice or do they seem by way inferior to the system that was already there? Of course, it is another question how the follower of the pluralist stance will do that. As mentioned above, this is a short-coming of Kellert's, Longino's and Water's pluralism manifesto and will not be answered in this thesis.

Thus having discerned different degrees of pluralist action and normativity I will assess Dupré's metaphysical position on this scale. Dupré offers examples based on taxonomy, genetics and ecology for his notion that science is intrinsically disunified and that the world and things are metaphysically and ontologically disordered. Importantly, his work does not imply any demand for neither testing the possibility of pluralism nor actively engaging in cultivating pluralism. Dupré has shown that the world is disordered, period. He neither beliefs that it remains necessary to treat the possibility of plurality as an open question, nor does he utter a normative statement of the kind that there should be more pluralism. Thus, I classify his pluralism as passive and observational and claiming closure.

This leads me to the following conclusion: discerning different pluralist positions by assessing their degree of activity, provides a scheme of classification that not only helps to give a good account of the three positions discussed, but also shows a way to demarcating without having to refer to alleged radicalness or modesty (Fig.5). In this scheme, the endpoints of a classification based on a commitment to action represent passive observing of pluralism and an active inclination to cultivate pluralism wherever possible.

It is a working hypothesis, that metaphysical pluralist positions will generally be situated near Dupré's position whereas solely epistemic position will be placed somewhere between Chang and the pluralist stance. Thus, this scheme is not only able to demarcate epistemic positions, but also gives an account on the degree of pluralist practice each individual stance implies.

Of course, it will be both interesting and necessary, to apply other pluralist positions to this scheme, in order to test it for its applicability except of these three examples and also to improve and sharpen it to give even better accounts of the various forms of pluralism. I am aware, that this model is nowhere near a perfect way to discern ways of classifying pluralism. At any rate I believe that it serves its purpose better than the scheme of radical and modest pluralism, introduced by Kellert, Longino and Waters.

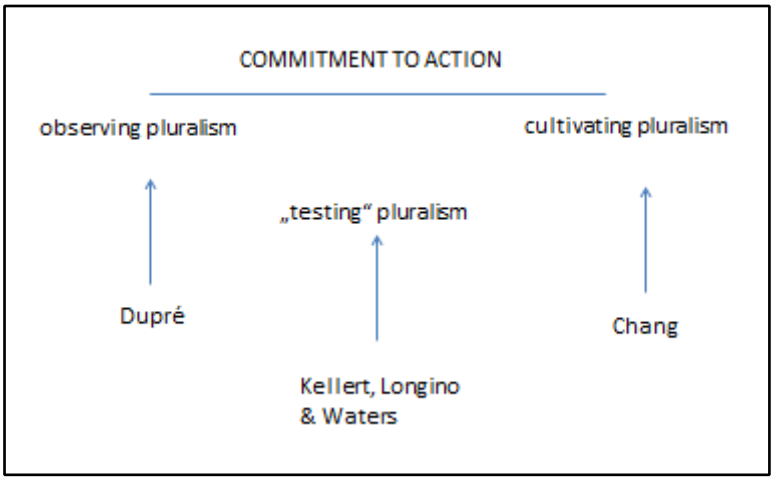

Figure 5 a new classificatory scheme

3 Pluralism – A Call for Action? – Critique

This chapter will focus on critique countered against Chang's work on scientific pluralism and the history of H_2O. This discussion will proceed in two steps: first I will give a survey on especially two publications leveled against Chang's pluralism, which criticize Chang's historical work on the history of phlogiston, Priestley and Lavoisier. In this vein, the definition of a "revolution" will be discussed and the term of the "experimenters' regress" will be introduced, what both will become important when discussing differences of Darwinism and Lamarckism. Especially, sociological explanations of theory choice will become important.

Having discussed the historical shortcomings of Chang's account I will proceed on questioning the actual possibilities of a pluralist's call for action. It is Chang's opinion that resources could easily be spared to foster the blooming of multiple approaches and theories within science. This notion will be contrasted by Kitcher's diligent work on how much plurality is possible and beneficial in a scientific community. The awareness gained on these topics will be applied in the following chapter when discussing the question, whether Lamarckism should have lived throughout the centuries.

3.1 The Historical Debate: Chang – Kusch – Klein

Chang's monograph recently faced critique from two authors, in two different ways: Ursula Klein tried to prove the point, that the historical episode Chang describes was no era of scientific revolution but rather a re-sharpening of theory and method (Klein, 2015, 80). This is not only important critique on Chang's work but will also prove salient when discussing the transition from Lamarckism to Darwinism to assess whether the term "revolution" is appropriate in this case in the following chapter, or based on what criteria it is possible to argue, that a revolution took place. Also it will prove important when discussing, whether scientific pluralism should especially be introduced in times of scientific revolutions to preserve theories that would else be subjected to "Kuhn loss". This will become important in a different section of chapter 4.

On the other hand, Martin Kusch argues, that Chang's historical case is not supported by the history of science and that there actually were good reasons for adopting Lavoisierian chemistry, thus weakening Chang's case for plurality (Kusch, 2015, 69). Also stressing the importance of a broader concept of explanations, he introduces the notion of the experimenters' regress, which I will discuss in more detail in order to assess in the next chapter whether the disagreement between Darwin and Lamarck could also be characterizes

as such. In the following I will discuss the main points of Kusch's and Klein's arguments and will also provide Chang's response to their articles as well as my own position concerning their conflicts.

Was There a Revolution?

Ursula Klein argues, that what is called a "revolution" by Chang – in the tradition of Kuhn and almost all western text books – was none, as Lavoisier shared ontologies, types of causal explanation and paradigmatic types of empirical methods with the phlogistonists. Klein defines scientific revolutions as radical changes in analogy to social or political revolutions (Klein, 2015, 80). Also she tries to show, that Lavoisier's work did not affect all fields of chemistry, but was rather a change within a certain sub-discipline, thus also not fulfilling requirements for a revolution. This leads her to the conclusion, that phlogistic and anti-phlogistic theories were not incommensurable. The reason, why Lavoisierian chemistry superseded phlogiston theory was the fact that it made teaching and communication easier (Klein, 2015, 81). In the following, I will provide a more detailed account on the arguments Klein provides.

In her approach, Klein first asks which substances were included and excluded in Lavoisier's theories. Importantly, whereas early modern chemistry switched its ontological understanding from a hierarchy of substances – as also found in Plato and Aristoteles as well as all the presocratics – to a very "flat" ontology, which led to shifting the focus of chemists from working with "natural substances" to working with only a restricted number of substances such as metals, acids, alkalis, "earths", alloys and salts), Lavoisierian chemistry did not bring such a change but rather maintained most of the areas of interest. Klein argues that the chemical table Geoffroy constricted in 1718, which included phlogiston, marks a peak in the revolution from pre-modern, aristotelian-influenced chemistry, as the explanandum had radically changed. Also, the ontological status of the substances had significantly changed from the Aristotelian systems, the hierarchy was flattened and components had the same status as compounds (Klein, 2015, 82).

In the same vein Klein argues, that Lavoisier's studies were built on existing phlogistonist theories. Also, she states, the meaning of phlogiston and Lavoisier's "caloric" overlapped, as both were claimed to interact with other substances and to be the causes of the increase of the temperature of flames. Also, the methods for studying phlogiston and caloric were partially the same. According to Klein's account, it was not Lavoisier, but the phlogistonists who first introduced quantitative methods however he made them only more

precise. Thus, Klein denies methodological incommensurability between phlogistonists and Lavoisierians, which would also constitute a revolution (Klein, 2015, 84).

It is Klein's thesis, that anti-phlogistonist science would not have been that successful, if the notions of phlogistonists had not been that diverse as they were at the end of the 18th century. If phlogistonists could have produced a unified nomenclature based on their alternative understanding, Klein claims that they could have successfully competed with the Lavoisierians (Klein, 2015, 89).

Concerning Klein's definition of scientific revolutions, Chang argues, that this definition is of course possible, but not as the only possible one. Thus, he believes that relativizing Lavoisier's revolution is appropriate, as dependent on what is understood as a revolution, the chemical revolution is or is not to be perceived as one. Further, Chang agrees that Lavoisier did not restructure chemical ontology, but did create methodological incommensurability. Importantly, Chang does not believe that ontology is the only important aspect of scientific change (Chang, 2015, 92). Also concerning ontology, Chang claims that there were slight changes: on the one hand, assumptions about the elements and compounds differed; on the other hand, phlogiston was often regarded as "ontological principle". Importantly, this was clearly only a feature of phlogistonist theory and not Lavoisierian. Also Lavoisier modified the problem-field and the judgment-criteria of chemistry, which made impartial judgment between those two systems impossible (Chang, 2015, 92).

Additionally, Klein's claim, that Lavoisier's chemistry did not change the subjects of interest much, is only partly true, as Chang gave an account of the division of the problem-field during the chemical revolution, which showed, that although there were some subject-areas that both parties found interesting (understanding of combustion, calcination/reduction, respiration; theory of acids, constitutions of various substances) there were a lot of problems that were only considered important by phlogistonists (explaining properties of compounds in terms of properties of ingredients; mineralogy & geology; metereology; nutrition & ecology) or by Lavoisierians (theory of heat and changes of state; chemistry of salts) (Chang, 2012, 20). Importantly, Klein never commented on this account. Naturally, it depends on the extent of non-shared problem-fields Klein requires for attesting a chemical revolution, but on the other hand, if both theories would not share any problem-field then they could hardly be in disagreement at all but they would both be parts of different disciplines.

Also, extending Chang's argument, Perrin offers a good account of a scientific revolution, when he states … "a scientific revolution may often be detected not only by the depth of the conceptual shift it entails, but by the drama of the accompanying debate" (Perrin, 1988, 81). Given Chang's reconstruction of the chemical revolution there was evidently enough drama to constitute a revolution. Another argument for accepting the characterization of a revolution comes from Holmes, who develops the view, that not only Lavoisier's but also Priestley's program was radically different to all phlogistonist theories before his, thus one could also characterize the chemical revolution as a fight of two radically new research programs (Holmes, 2000, 737). In this vein, ontological assumptions about phlogiston were also substantially different from the old French phlogistonists to Priestly, thus strengthening the case of a revolution also in Klein's account.

Importantly, I agree witch Chang, that there are several ways of characterizing a revolution and that the Chemical Revolution fits quite some, if not all. Still, Klein's qualifiers for a revolutionary status will prove very important when I will assess, whether there was a revolution concerning Lamarck's and Darwin's accounts on evolution. Also, I believe that "Kuhn loss" could serve as a good criterion for characterizing scientific revolutions.

Additionally, even accepting that the chemical revolution does not attain revolutionary status as not comparable to radical social or political revolutions, does not weaken Chang's case much. As he argues that phlogiston was prematurely if not unjustly rejected and the rejection of phlogistonist theories was evidently the case, even if the conceptions of the Lavoisierians were ontologically similar to the phlogistonists his argument remains intact.

Conclusively, conceding that the transition from phlogistonism to Lavoisierian chemistry does not fulfill all criteria for a scientific revolution does not weaken Chang's point. Importantly, what is salient for his arguments is that something was lost through this transition. Thus, if Chang's case-work on what was lost remains convincing, this historical debate is no threat to the evidence provided to support his pluralist position.

Sociological Explanations of the Chemical Revolution

One of the main points of Martin Kusch's critique is that Chang does take too much the side of the phlogistonists and thus adopting their "actors' sociology" disregards the reasons for rejecting phlogiston and accepting the oxygenists' viewpoint. Thus, Kusch states, that if it is possible to attest rationality to researchers, who converted from phlogiston theory to Lavoisier's postulates, then Chang's case is immediately very weak. Also, when accepting

sociological explanations of theory choice diminishes Chang's case, as he could not argue any more, that it was rational for phlogistonists to maintain their stance. Thus, Kusch aims at proving that Chang's account of the chemical revolution is incomplete, as he omits sociological explanations in his reconstruction.

In the following, I will refer to these two kinds of explanations as "narrow" and "broad". I intend to stick with this very tentative description, as I think that categories such as "rational" and "irrational" miss the point. Also, I do not think that calling them "scientific" and "sociological" reasons is a good approximation, as what is regarded as "scientific" is again defined by one's individual standards and criteria for rationality. Thus, I call reasons "narrow", if they mainly focus on experimental results and theoretical considerations, whereas I call reasons "broad" if they involve issues of trust as well as other external social or historical circumstances.

It is Kusch's position, that Chang adopts the phlogistonist's view on moral and intellectual superiority in contrast to the Lavoisierians. This is what renders him as a partial judge. Additionally, as phlogistonist positions at the edge of the 18th century were very diverse, it is incorrect that Chang reduces the debates down to the conflict between two systems, as phlogistonists should not be unified to one system of practice (Kusch, 2015, 74). Also concerning this problem, there was some radical novelty in Priestley's phlogiston theory compared to the other phlogistonists. In addition, Chang ignores the reasons of contemporary phlogistonists that "converted" to Lavoisier's chemistry. In this vein, Kusch emphasizes that there actually were convincing reasons for discarding phlogiston, as Chang construed relevant reasons too narrowly and did not take broader explanations into account .

Kusch draws some special attention to the disagreement Priestley and Lavoisier actually had: they did not only disagree about the interpretation of experiments, but did also disagree about what result a competently performed experiment should yield. This is an example for what is called "experimenters' regress", a case, where the features of a competently performed experiment and the theoretical account of a phenomenon are under discussion (Kusch, 2015, 71). It is Kusch's notion that Chang underestimates the experimenter's regress, as he pays no attention to the fact that they not only disagreed about the correct interpretation of experiments but also about the correct theory and on how experimental data is competently produced (Golinski, 1995, 78). Thus, such "broader" reasons

are to be taken under account, as they are closely intertwined with the „narrow" reasons for theory choice.

At this point I will discuss the definition of the experimenters' regress in more detail: When a novel phenomenon is detected, the successful outcome of the crucial experiment is in question as it could be viewed as the detection – and a possible paradigmatic change – or the non-detection – and the maintenance of the current paradigm – of the phenomenon. Also, researchers need to agree, that the crucial experiments detecting a certain phenomenon were competently performed. Thus, incredibility is also a partially social product. Also, the interpretation of results by other scientists depends on their charity invested. In science, "lack of charity in interpretation implies a defense of the status quo and a license to expel anomalous findings from the body of scientific knowledge" (Collins, 1981, 53).

The experimenter's regress is what characterizes contemporary cutting-edge science, as there is no criterion for experiments, other than their outcome, that indicates whether difficulties have been overcome. Thus it is often impossible to say objectively whether a given empirical finding is confirming or disconfirming evidence. The reason for this is that we take an instrument to be good if it produces good evidence and vice versa, good evidence needs to be produced by a good instrument. Thus, Collins argues, that scientists turn in such cases to criteria, that are used when making common-sense judgment in day-to-day life, such as criteria like reputation, affiliation, track record, personal ties…(Gelfert, 2011, 149).

In this vein, Kusch gives an account on why people in the 1780s and 1790s had good reasons to convert to Lavoisierian theories and states and argues that they were not only the victims of a "ruthless campaign" launched by Lavoisier. These reasons were rather indirect but intertwined with experimental data and proximate theoretical considerations. These indirect reasons were for example track records, social indicators of reliability, institutional status, standing in the profession and plausibility of vision for the field of chemistry as a whole – simply put, the classical factors in an experimenters' regress for the adoption of one position. Importantly, it needs to be kept in mind that trust in testimony of fellow scientists is what mostly makes out scientific knowledge, as only a very small part of knowledge is generated by one's own experiments. Trust is defined by track record and status indicators: success, institution, publishing, prizes, citations, collaborations, intellectual debates the researchers is involved in…(Kusch, 2015, 76). In times of major scientific debates, trust is broken down and reassembled when "closure" was achieved (Kusch, 2015, 76).

It is another argument that researchers such as Humphrey Davy, which were characterized as fence-sitters by Chang's account did not adopt phlogistonism for scientific reasons, but rather because of politics. This includes for example, that in times of the Napoleon Wars, "French theories" were likely to be rejected by British scientists, which again proves, that Chang omits important sociological factors in his narrative. Also, Chang calls some scientists fence-sitters that lived in countries where the translation of Lavoisier's treatises were not available at the time of Chang's account. Of course, such a characterization does them wrong, as the reception of oxygenist theories was delayed in those countries. In addition, even taking these scientists into account can in no case be taken as evidence for plurality, as researchers, that did not belong to the core-set of investigators and did not at all engage in the on-going debates can hardly be taken as evidence for plurality (Kusch, 2015, 75).

Concerning the experimenter's regress, Chang responds, that it was not important concerning the decision-making between phlogiston and oxygenist theory. Also he holds the opinion that all sociological reasons that might have been present at that time, were uninteresting. Thus he concedes that in the beginning there might have been disagreements about the observed phenomena, but that those did not last for a long time (Chang, 2015, 95).

Additionally, Chang disagrees about what he identifies of Kusch's notion as good reasons, namely generally favoring monism. Although it is clear why Chang finds this characterization objectionable, it is nevertheless supported by the history of science and also by the current state of science and our general aims when formulating explanations: They are favoring monism and simplicity. Thus, Chang can make his normative pluralist statement and say that they should not be like that, but that does not disqualify them as good explanations of present and past.

Also, Chang holds, that the attestation of the higher trustworthiness of the Lavoisierians against the phlogistonists needs to be proven. For example, he believes that there cannot be a scenario – if starting with external historical explanations – where an englishman would prefer a "French" theory to an "English" one (Chang, 2015, 96).

Given their debate, it might be the case, that Chang and Kusch have their own experimenter's regress – or the version of it placed on the meta-level - , as they cannot decide on the instrument to describe the chemical revolutions – "narrow" or "broad" explanations of rational theory-choice. Thus, if „narrow" reasons are the only ones Chang holds important then it becomes clear why they cannot agree on one explanation. Also, they might disagree what a

successful outcome of such a historical investigation into the chemical revolution is. For Chang it is clearly the attestation of plurality and pointing out the benefits of it. Yet, if this is not shared by Kusch, then it is clear why they disagree. Considering both positions, I agree with Kusch on the importance of "broad" explanations in addition to "narrow" ones to reconstruct the rationality of choices concerning certain decisions. Also, especially the notion, that those two kinds of reasons are closely intertwined is an observation that seems also to hold true for my investigations concerning Lamarck and Darwin, what will be shown in chapter 4.

In summary, the embrace or rejection of sociological explanations makes it in several ways impossible to settle the case between Kusch and Chang: For example, the reconstruction of the justification of embracing Lavoisierian theories seems to be dependent on accepting "broader reasons". Also, as Kusch requires Chang to provide an account, whether the relevant actors of later centuries would have regarded phlogistonism as worth of further development, this is again dependent whether the story is framed by broad or narrow explanations only. This again shows, that there might be something similar to an experimenters' regress on the meta-level between Chang and Kusch. Having discussed problems arising witch Chang's account based on the history of science I will now turn to criticism based on considerations on democratically organized science.

3.2 The Philosophical Debate: Kitcher

In the following, I am going to provide a critique of Chang's claim that the cultivation of scientific pluralism is affordable. As discussed in chapter 2, it is his opinion, that pluralism would not only be beneficial for science, but also that science has the capacities to maintain lines of inquiry on several different incommensurable theories, which all will lead to important results. For example, he states for the phlogiston theory: "If that system would have developed further, who knows what kind of interesting organic classifications, even structural theories, it might have led to in the end? And who knows what kind of new syntheses it might have stimulated by plan or by accident?" (Chang, 2012, 211) Thus, Chang believes that keeping various systems of knowledge alive – first through conservationist pluralism, later maybe even through science policy – will lead to a cumulative growth of knowledge beyond what is straightforwardly possible for each single system of practice. For example, Chang believes, that the full potential of late 18th-century science was not seized, as the phlogistonist's chemistry was abandoned too early. Additionally to abstract hypothesizing,

Chang believes that his historical case-work shows, that periods of plurality concerning electrochemistry and atomic theory were very productive (Chang, 2012, 213).

Importantly, one of the criteria Chang provides for favoring pluralism is the analogy drawn from the pluralism of a democratic society. Thus, it needs to be asked, whether this analogy should really serve as an argument for pluralism and whether supporting pluralism should be a feature of a democratic society that might support political pluralism of opinions, ethnics and religions.

In the light of this, the combination of pluralism and democratic societies will be critically assessed following two major works of Kitcher, namely ***Science in a Democratic Society*** (2011) and ***The Advancement of Science*** (1993), in which Kitcher deals with the ideals of scientific inquiry and their shaping by public opinion. In the following, I will provide a discussion of Kitcher′s account of public engagement with science and values that govern scientific practice. These aspects are important when resolving debates. Additionally, I will discuss his critical assessment of when plurality within the sciences is beneficial and when it is not. Thus, Chang's pluralism will be contrasted by Kitcher's notion of "well-ordered science".

What Kitcher defines as well-ordered science is a state of science, where "the specification of the problems to be pursued would be endorsed by an ideal conversation, embodying all human points of view, under conditions of mutual engagement″ (Kitcher, 2011, 106). Thus, scientific significance is a feature of those problems, which are held important under the conditions of well-ordered science.

Before discussing the policies of well-ordered science, Kitcher gives an account on the history of the democratization of science, which leads him to the characterization of the current problems of representation of science to the public. He characterizes it as a feature of the 20th century and a legacy of the Enlightenment period that politicians saw great merits in investing in science and citizens increasingly regarded scientists as authorities. Importantly, at the end of the 20th century, the public became more and more suspicious about science and so-called "expert consensus", as also within science there is more and more disagreement about issues that also seem important to the public, such as evolutionary theory, genetically modified organisms and nuclear energy. Additionally, these fields of science also become more and more politicized thus also causing some substantial distrust of the public. But also on the

scientific level, trust in the “knowledge” and “truth” of science becomes increasingly challenged (Kitcher, 2011, 15ff).

The reasons for this dissent lie partly in the fact that many people disagree with the current division of epistemic labor. Epistemic labor characterizes the designation of each topic area to an expert community except for one where epistemic equality holds, letting people identify their own aspirations and values. Thus the public either believes that the wrong experts were assigned or that the criteria for assigning the experts were wrongly set. Also, Kitcher argues, decisions about the division of epistemic labor are often not integrated with values that are central to democracy (Kitcher, 2011, 20ff).

What also relativizes the concept of scientific authority is, that the notion of the value-freedom of science has long been challenged by historians, philosophers and sociologists of science: already when deciding which problems to tackle and which question to answer the values of the individual scientist, the research group and the institution in which she works have great influence, although this is limited by the current potential of the field and one's own talent and financial possibilities. Additionally, Kitcher claims, that not only the context of discovery, but also the context of justification - which was historically mostly characterized as pure and value-free - is value-laden as no scientist can judge the adequacy of evidence without taking into account what outcome this would have on their own science and presuppositions. Also, the assessment of the probability of a hypothesis after experiments were conducted is largely dependent on the prior probability of that hypothesis - i.e. the assessment before any information was gathered -, thus, there can be no talk of the value-freedom of the sciences (Kitcher, 2011, 31ff)

Thus, the values at hand to each scientist are crucial in times of scientific debate. Crudely, Kitcher discerns three different sets of values: (1) The broad scheme of values defines the commitments around which human lives are organized. (2) The cognitive scheme of values gives an account of a person’s ideal and characterization of important knowledge. (3) The probative scheme of values defines the specific questions that are regarded as crucial. Kitcher characterizes periods in which two opposing scientific theories are competing by the feature, that proponents of each side “adopts probative schemes of values, according to which their favored approach resolved more of the important problems than its rival” (Kitcher, 2011, 60). Success on one side then leads the rivals to modify their notions of critical values and at a certain point the problem solving capability of one research program does convincingly force the other out of the field or has everybody converted. This phenomenon is a characteristic for

revolutions like the Copernican, the Lavoisierian and the Darwinian. As cognitive values where thence also reshaped, the only reason for sticking to the theory „at loss“ was the allegiance to a set of broad scheme of values with which the losing theory could be identified (Kitcher, 2011, 59ff).

. For Kitcher, the broad scheme of values is a demarcation criterion to discern which positions have a right to enter scientific debates. To provide an example, the reason why the broad scheme of values of creationists can be dismissed according to Kitcher is because an error lies in the character of the particular broad scheme of values introduced. This means, that anti-Darwinians′ claims about the existence of a supernatural being are almost certainly false, what cannot and should not be part of the conversation, as this violates the cognitive conditions of mutual engagement. Thus creationists cannot fulfill the requirements for an ideal conversation, as they are not able to endorse the values in question (Kitcher, 2011, 60).

Despite problems arising for theory choice at times of scientific debates, there are limits to tolerance of differentiating scientific systems. For the rejection of a certain system, Kitcher provides two criteria: 1) The cognitive system is poorly designed that it cannot promote and cognitive goals; 2) The goals of the cognitive system are not cognitive at all.

Thus, in Kitcher′s account there are definitely limits concerning „who sits at the table“ and thus he draws the demarcation line at pseudoscience. Contrarily to other philosophers of science, he does not define pseudoscience through implausible logics but draws his argument from psychological dispositions: If the situation can be characterized by the aforementioned tenant 2) and the subjects still state to have cognitive goals, then they are deceiving themselves, thus pseudoscience is a psychological process. In other words, psychology configures and biases their lines of inquiry, and thus, for example creationists, are easy to chase away from „the table“ (Kitcher, 1993, 195f). Thus, Kitcher strictly opposes Chang′s view on whether pseudoscience should be integrated, as he attests their epistemic fallacy to be too big to be able to contribute anything worthwhile to science. Lastly it should be said that it is worth noting that such decisions are also framed by the ascriptions of authority to the protagonists and their institutions.

Kitcher argues, that a practice directly inferred from democracy would lead to awarding scientific significance through a majority vote to a certain research program or problem-field: each member of the population would vote for the scientific investigations she would like to see put forward. Many critics of this hypothetical practice do of course state, that - given their

presuppositions about the education and reflectivity about society - the „winning“ projects would be mostly such, that promise short-term practical goals, whereas research programs that are aiming for long-term scientific goals would be disfavored. In spite of that, the radical alternative to that model is that a group of experts has an all-deciding authority on the lines of inquiry science should pursue (Kitcher, 2011, 112).

For efficient practice of public engagement in science, Kitcher gives several instructions: on the one hand, voting should never be initiated, before discussion has taken place. Differently and more idealized spoken, voting should occur in the settings of well-ordered science. This process includes tutoring by experts, dialogue with other tutored individuals and assessment of future consequences. There are three different possible outcomes of this procedure: 1) All agree on one plan and thus console on a way of dividing the support and resources among various lines of investigation; 2) Several plans are rendered by every individual as acceptable but with different rankings and the decision is made by majority vote; 3) No plan is acceptably for every individual and the choice is made through majority vote (Kitcher, 2011, 113ff).

What Kitcher proposes as a main benefit of well-ordered science is its fair-share principle: a problem is not considered as solved, if the western world is free of it, but only, if it was solved also in all the remaining parts of the world too. Thus, well-ordered science considers global health. Additionally, it aims at the idea of full representation, as it holds that all human perspectives necessarily must have an impact on any adequate simulation of an ideal discussion. This of course gets a little bit more problematic when trying to include the needs of plants or animals into this ideal discussion (Kitcher, 2011, 122). After having framed Kitchers consideration on the possibilities of a liberal democracy applied on science policy, I will now discuss his assessments of the possibilities of pluralist research in science.

Kitcher provides a formula that captures the benefits of applying various research strategies for aiming at one goal

$$\Pr(\text{success} \mid \langle n_1, n_2, \ldots, n_r \rangle) = \Pr(S_1 \mid n_1) + \Pr(S_2 \mid n_2) + \ldots + \Pr(S_r \mid n_r)$$

Importantly, when one strategy (S_1) seems to be more promising than all the others (S_i) then all available researches (m) should approach investigation S_1 and no other approach should be tried out by a portion of all available researchers (m-n):

$$\Pr(S_1 \mid m) > \Pr(S_1 \mid m\text{-}n) + \Pr(S_i \mid n)$$

On the other hand, when the addition of one more scientists to the mainstream approach is thought to bring non-substantial progress and another approach seems promising, then more than one approach should be followed:

$$Pr(S_1|m) < Pr(S_1|m-1) + Pr(S_i|1)$$

This formula can of course be extended for S_n (Kitcher, 2011, 194).

One reason for Kitcher to pursue the approach that is considered much less promising is the probability of making a substantial discoveries because of being the only one who follows approach M* and not M, as the relative probability of each individual scientist pursuing M to make the important discovery is rather small (Pr(M|N)/N). As an example he provides the case of Watson and Crick, who did not engage in traditional analysis of X-ray crystallography but tried speculative model building (Kitcher, 2011, 196f). It is important to note that this explanation for favoring a plurality of approaches does not hold any normative position, that pluralism is good, but just gives an account of scientist′s ambitions.

Concerning controversy in the sciences, Kitcher states as a rule of thumb, that diversity of opinion, judgment of significance and standards of certification are welcome as long as a problem is yet to be solved. Interestingly, Kitcher distinguishes diversity from dissent: the former applies for disagreements before a problem was solved, whereas the latter applies for cases after the matter was settled. Thus it is Kitcher's notion that when a problem is resolved, diversity is wasteful. Thus, maintenance of diversity is one task for well-ordered science, as long as the debates are not settled (Kitcher, 2011, 218f). Also, Kitcher argues that there are also problems that are emerging from rivalry within the emerging diversity: it is not clear, how other desirable features of science, such as collaboration, are affected by over-competitive scientists that e.g. refrain from sharing their results (Kitcher, 2011, 219).

When discussing how to determine whether one theory is „better" and more „progressive", Kitcher introduces the view of robust realism: there is an objective order of dependency within nature. Recognizing these dependencies and deepening one's understanding of them can be called a progressive step. Another way to describe progress is to assess whether the new theory enhances understanding of the explanandum, thus not making a statement about the truth or falsity of the explanans. Kitcher extrapolates several different processes that are at work when improving explanations: 1) the introduction of correct

schemata; 2) the elimination of incorrect schemata; 3) the generalization of schemata; 4) explanatory extension. Thus he describes the explanatory process as following:

(EP) P_2 is explanatory progressive with respect to P_1 just in case the explanatory schemata of P_2 agree with the explanatory schemata of P_1 except in one or more cases of one or more of the following kinds:

(a) "P_2 contains a correct schema that does not occur in P_1

(b) P_1 contains an incorrect schema that does not occur in P_2

(c) P_2 contains a more complete version of a schema that occurs in P_1

(d) P_2 contains a schema that correctly extends a schema of P_1" (Kitcher, 1993, 111)

Compared to Chang, Kitcher has a radically different opinion on Kuhn loss and the necessity of avoiding it. Admitting, that there was explanatory potential that was lost by moving from P_1 (e.g. Aristotle or Priestley) to P_2 (e.g. Newton or Lavoisier), while P_2 had other significant explanatory potential P_1 did not have, he states, that from the (EP) position, both theories cannot be ranked in terms of progressiveness. Nevertheless he argues that the questions abandoned were intractable at that time and only through solving the tractable questions posed by P_2 it was possible to solve the problems of P_1 (Kitcher, 1993, 111). Thus, in Kitchers account, when it can be shown, that the lost questions were recaptured after a time, there is no problem with temporary Kuhn loss and it is actually a necessity in order to overcome it. This position could be suspected of having a minor inclination towards the model of knowledge accumulation, although the accumulation process is characterized to be more indirect.

Kitcher discusses extensively the benefits of „hedging one's bets": They are desirable if the situation is unclear and no decision on a scientific matter can be made. Kitcher argues, that it is viable, that opposing points of view are fostered whose objections help to refine the ultimately successful position. He demarcates „epistemically pure" from „epistemically sullied" scientific communities. Epistemically pure communities show cognitive uniformity, as they are aiming for purely epistemic ends and immediately favor the theory that seems to have the highest truth value. An epistemically sullied community is also influenced by socially pressured and unscientific types of motivation and thus will produce an epistemologically heterogeneous community. It is important to bare in mind that epistemically sullied communities seem to do very well, eventually, as the example of Watson & Crick canvassed above shows (Kitcher, 1993, 308ff).

Additionally, Kitcher shows that the epistemically sullied community also does better in achieving a stable optimal division of cognitive labor as long as a scientific debate is not settled. One reason for this is that members of the community see their chances in trying out methods whose intrinsic prospects are relatively low, but might be advantageous in the sense that they could render them first to make the discovery. Also, local autocracy can help fostering multiple approaches (Kitcher, 1993, 309ff).

Importantly, Kitcher concedes that training, rationality, personal loyalties and intellectual affiliations might shape theory choice and research in important ways. Crudely he divides researchers in traditionalists, rebels and neophytes who all have differently assigned benefits of following certain lines of research and theory choice. Kitcher articulates an interesting case, when theory T1 and T2 have together a probability of being true that is less than one. In this case, he questions how a society would respond and how resources can be distributed in order to find T3, T4, T5… . Individuals exploring new options are especially under pressure as they do not only have to hope that T1 and T2 are proven to be wrong, but also that some accepted theory emerges from their investigations. Then again, the probability of them identifying this theory has to be computed as a function of how many researches search for new theories (Kitcher, 1993, 367ff).

When asking which consensus forming mechanisms would be best for a scientific society, Kitcher demarcates three different kinds of considerations that affect costs and benefits of consensus-forming mechanisms: 1) the security of the consensus is defined by how many scientists manage to replicate the experiments and reach the consensus decision; 2) there are losses when research agents focus on the reforming of consensus practice and do not follow their own research goals, that might have been beneficial for the research community; 3) reaching consensus by a majority but not all members of the group will exclude those members, that deny the consensus has been reached (Kitcher, 1993, 382ff).

Conclusively, there are several notions of Kitcher's work, which can be leveled against Chang's unconfined conception of active pluralism. Importantly, Kitcher grants pluralism especially for those periods, where scientific debates are not settled, as there are clear benefits if not all researchers stick to the same research approach and theoretical framework. On the contrary, pursuing alternative theories and approaches when debates are settled are characterized as unproductive dissent. In spite of that, Kitcher's approach still provides an antidote for Kuhn loss, as he claims, that questions, that were important to the probative set of values of the losing site, can be reanimated, when the progress of the winning paradigm has reached certain stages. Thus

introducing democratic values to science does not automatically lead to Chang's normative pluralism.

Also, contrarily to Chang, Kitcher argues that there are certain limits of accepting theories. The broad scheme of values offers a criterion for such demarcation. Thus, if Chang argues, that even letting sit creationism at the table, as fuelling the debate might generate knowledge important for the well-accepted sciences, one needs to pose the question, how much it would stall science if a respective amount of scientists would engage in active debate with creationists as they could not perform their assigned research aims. Of course, this argument could be expanded to several cases of pluralism. Additionally, characterizing pseudoscientists as self-deceiving provides another strong argument for not considering all positions as worth of serious debate.

In summary, I would argue that through Kitcher's deliberations it is possible to turn Chang's pluralism into a more economical approach. For instance, it gives good reasons why to omit pseudoscience in pluralist considerations, what would prevent the rise of scientific debates that are intellectually uninteresting and thus might really paralyze science. Still I believe that the prognosis, that important questions of losing theories might come back eventually as a result of the progress of the winning account might not console Chang's demand for normative pluralism. Thus, he would for example claim that assessing whether $Pr(S_1|m)$ is larger or smaller than $Pr(S_1|m\text{-}n) + Pr(S_i|n)$ should be also practiced when a debate seems to be settled. Thus it might be salient to consider the extent n should have to limit stalling science were not necessary but still maintaining all possible benefits of pluralism.

4 Lamarck/Darwin Revisited

In this chapter I intend to sketch a historical case study to test whether there were other situations, similar to the Chemical Revolution, which can be used to argue in favor of scientific pluralism. If the historical data corroborates this claim, one could argue that plurality within the sciences should have been maintained and multiple theoretical accounts should have been fostered. Despite fully according with Chang that the Chemical Revolution provided such an example, I believe that there are several historical episodes, where a reconstruction from a pluralist perspective could be enlightening.

This study will focus on the immediate success of Darwinism over all other theories of species ontologies, such as Lamarck's. First an account of the historical circumstances when Lamarck published his views on the gradual transformation of plants and animals will be given. Then I will proceed to the time point when Darwinism entered the battle and the differences between both theories will be discussed as well as the reasons for the rejection of one and the success of the other. Having canvassed the reasons for abandoning Lamarckism an account on how epigenetics brought back Lamarck into state-of-the-art molecular biology will be provided. In a concluding section I will assess whether Lamarckian or Darwinian theories contributed to a scientific revolution in biology and whether their discrepancies can be characterized as an experimenter's regress. When doing so, I will also include positions held before both Lamarck and Darwin formulated their evolutionary theories.

These elaborations will reveal a duality between the good reasons for rejecting the thesis of Lamarck especially in favor of Darwin's elaborated axioms of evolution and the fact that in the end and in the light of cutting-edge molecular biology, some notions of Lamarckian evolutionary theory seem to have their justification. In this vein, it will be discussed whether the presented account makes a case for pluralism. This will be done by asking whether if Lamarckian evolutionary theory would have been kept alive, certain phenomena would have been discovered and understood earlier and certain scientific fields could have developed farther than they have at the presence. These deliberations will be contrasted by the restrictions of a scientific society to foster plural approaches. These considerations are imbued by the works of Kitcher on the ***Advancement of Science*** as well as ***Science in a Democratic Society***, which I sketched in chapter 3.2.

My choice of this historical case-study is based on two reasons: On the one hand state of the art biology stimulated most contemporary pluralist thinking (e.g. gene-centered biology, systems biology, behavior…). On the other hand, the controversies of Neo-Lamarckians and

Neo-Darwinians are mostly shaped by the debate concerning nature vs. nurture – which cannot be fully captured by a specific branch in biology but draws its evidence from many, possible not reconcilable sub-disciplines such as genetics, biochemistry, physiology, neurology as well as even psychology (Longino, 2002, 176ff) and thus is pluralist.

4.1 1809-2010 – The Big Picture

This historical case study will be divided into three parts: first and most extensively, I will provide an account on the historical circumstances when Lamarck's published his ***Philosophie Zoologique*** in 1809 and of the reasons for the immediate rejection by the scholars in his scientific macro- and microenvironment. Those will be divided into "narrow", reasons and "broader" reasons. This examination will show that also in the case of Lamarck, narrow and broad reasons for rejecting his theory were closely intertwined, as Kusch also argued in the case of Priestley and Lavoisier (Kusch, 2015, 76). At the end of this section, after having elaborated a vast amount of reasons for the dismissal of Lamarck by his contemporaries, I will discuss whether his project should be regarded as a failure in comparison to Darwin or as an individual, independent approach.

Second, I will try to reconstruct the almost immediate success of Darwinism as well as the debates between Neo-Darwinism and Neo-Lamarckism until the 1940s, when the case seemed again to be settled in favor of a modern synthesis of Darwinism. In this vein, I will also try to determine, which circumstances granted Darwin's success in contrast to Lamarck.

Third, I will give a short survey on phenomena in molecular biology that seem to suggest, that there are Lamarckian mechanisms in addition to Darwinian ones that guide heredity and evolution. Additionally, I will discuss both Neo-Darwinian as well as Neo-Lamarckian interpretations of those.

These accounts will lead me to discussing several problems, concerning the nature of the often constructed dichotomy Lamarckism / Darwinism. Additionally, I will contrast both positions with their scientific predecessors. In this vein, I will try to assess two different problems: First, I will try to answer the question, whether one can speak of a scientific revolution, a change of paradigm, when looking on the transition from Lamarckism to Darwinism. These considerations will be important in the next chapter, where I will try to assess the proposal, that under the extraordinary circumstances of scientific revolutions, Chang's scientific pluralism could be applied.

Second, I will try to test, whether the discrepancies between Lamarck and Darwin can be characterized as an experimenter's regress, similar to Kusch's reconstruction of the debate between phlogistonists and Lavoisierians.

Lamarck and his *Philosophie Zoologique*

Lamarck's evolutionary theory was first outlined briefly in an introductory lecture he gave on zoology in 1800. In the years before, he based his talk on the writings of two of his colleagues, as he was assigned from botany to a new research area – invertebrates - in 1793 of which he had little expertise. In 1800 he completely rewrote his course, which helps to pin down the time point, when Lamarck initially started to believe in transmutation of organisms (Burkhardt, 1972, 414). His thesis was further developed in his 1802's ***Recherches sur l'organisation des corps vivans*** and fully discussed in his 1809's ***Philosophie zoologique, ou, Exposition des considérations relative à l'histoire naturelle des animaux*** [5].

In my elaborations I will not go into detail on the structure of his work nor provide a detailed account of his explanations and examples discussed but rather will briefly introduce them wherever needed. Thus, I present the Lamarckian evolutionary theory as mainly based on two theorems and add a few considerations on certain principles Lamarck held important:

1) The use of organs promotes their development; the disuse of organs leads to their reduction

2) Acquired characters are inherited.

Despite these two general laws, it is important to add, that Lamarck's theory was mainly shaped by his belief that life would strive towards ever greater complexity. Additionally, the further development or reduction of organs was in his account executed through the canalization of certain "fluides incontenables", where "incontenable" might most accurately be translated with "subtle". His conception of such "fluides" was naturally deeply shaped by his adherence to phlogistonist principles (Pichot in Lamarck, 1994, 22f).

Despite their provocative potential, none of Lamarck's works made a substantial impression on his contemporaries. Nevertheless, it is not clear, whether this was dependent on the insufficiencies in Lamarck's work or on hostile views against Lamarck that dominated the French scientific society at the end of the first decade of the 19th century. Thus, the following

[5] In the following, this monography will be referred to as ***Philosophie Zoologique***

accounts will show that both "narrow" and "broad" reasons for the rejection of his theories were deeply interwoven when considering Lamarck's case.

"Narrow" Reasons for Dismissing Lamarck

Richard Burkhardt states, that there are three different distinctions to be made, when engaging with Lamarck's scientific conceptions: 1) his broad foundations of thought vs. his immediate reasons for believing evolution, 2) the set of specific problems that made him arrive at a conception of evolution vs. the broader set of problems that came up when embracing evolutionary thoughts and 3) the belief that evolution takes place vs. the particular explanations he offered (Burkhardt, 1972, 413).

Burkhardt makes an interesting point when he claims that Lamarck was a scientist of a bygone century, and thus not perceiving the "needs of science" in the same way, as his younger contemporaries did. Importantly, his biggest success was his ***Flore française: Ou Descriptions Succinctes De Toutes Les Plantes Qui croissent naturellement En France ; Disposée selon une nouvelle méthode d'Analyse, et à laquelle on a joint la citation de leurs vertus les moins équivoques en Médicine, et de leur utilité dans les Arts***[6], which was published in 1779. Simultaneously, Burkhardt attests to Lamarck's writing an insensitivity to the difficulties, possible readers could have with his text and thus he made no effort to alleviate these difficulties. This might partly be explained by the fact, that at this time in life – 1809 – Lamarck was already of the conviction that all of his works are prone to be poorly received. This shows that not only the perception the scientific society of France had of Lamarck, but also his perception of these relations played a great part in explaining why the ***Philosophie Zoologique*** was that ill-received. Additionally, his perception of himself as a scientist is of importance (Burkhardt, 1970, 275).

Importantly, also reflected in the title of his monograph, Lamarck conceived himself as a philosopher, "a man with the powers of meditation and the breadth of vision necessary to give science the rational foundations and the direction that it needed" (Burkardt, 1970, 285) – a *naturaliste philosophe*, that was interested in "grand facts".

Additionally, Lamarck disregarded the necessity to show the way in which his facts related to his theory. Also, he ignored, that the provided examples were met with much more interest than his evolutionary theory as a whole and thus made no effort to relate to his critics through those case-studies. As his contemporaries rejected the plausibility of the examples Lamarck

[6] In the following, this monograph will be referred to as ***Flore française***

provided, they made little effort to assess the evolutionary ideas behind them (Burkhardt, 1970, 280).

There was also a second problem concerning the choice of his examples: Lamarck – botanist turned zoologist – was viewed as a specialist in invertebrate biology and especially conchology, the study of mollusk shells. Thus it does not seem intuitive that the examples Lamarck provided for corroboration of his evolutionary theory were mostly based on considerations about "higher" animals – vertebrates – a field he had never shown interest in before.

Besides his position as overseer of the menagerie of the Muséum national d'histoire naturelle, he never engaged scientifically with vertebrates and when he did it in his ***Philosophie Zoologique***, he chose an entirely hypothetical way. Thus, Lamarck's theses on the transmutation of species were not elaborated in a manner that also would guide and try to convince readers that might have had *a priori* controversial opinions. Second, the evidence he presented lacked empirical facts and the organisms on which he focused did not belong to his field of expertise.

Also, Lamarck's proposed "fluids incontenables" were often associated with his pyrotic principles of phlogiston chemistry: Lamarck considered fire as one of four basic elements, which could have three different states: natural, "fixed" and expansion. He believed that the three states of fire were not only involved in certain proximate phenomena such as combustion and calcination but also in electricity and magnetism. In accordance to this assumption, he called his theory "pyrotic theory" in contrast to the "pneumatic theory" of Lavoisier and his followers (Burkhardt, 1995, 98). Thus he explained transformations of organs by a derivate of a theory, that was generally rejected in France and replaced by Lavoisier's chemistry at the end of the 18th century, which again shows that Lamarck needs to be interpreted as a scientist of a bygone century. This of course provided his contemporaries with very good reasons for dismissing his theory as a whole.

"Broad" Reasons for Dismissing Lamarck

Probably the most important "broad" reason for rejecting Lamarck's evolutionary theory in the first decade of the 19th century was the general lack of trust in Lamarck's work. It is important to note that Lamarck was not always a discredited scientist but had a promising carrier since the 1770s, when Georges-Luis Leclerc, Compte de Buffon, one of the most respected natural scientists of his time and even greatly honored by Carl Linnaeus, admired

his work in botany, especially his afore mentioned ***Flore françoise***. Thus, Lamarck was chosen – through Buffon's lobbying – for a vacant spot in the botanical section of the Academy of Science and until the 1790ies he could even improve his standings – importantly, he was a supporter of the French Revolution (Burkhardt, 1970, 279f).

In the last decade of the 18th century, Lamarck almost exclusively engaged in publishing in fields, he had no specific expertise in. Most prominently, he was fighting an ineffective battle against Lavoisier's chemistry, in which he defended his "pyrotic" chemistry (that was located in close vicinity to phlogistonism) against the "pneumatic" chemists following Lavoisier, thus rendering him a die-hard phlogistonist in Chang's account (Chang, 2012, 31).

Although Lamarck's academic transition from plants to animals was justified because of his affiliations and also because he often uttered great interest in the investigation of the differences between plants and animals, his works on physics, chemistry, meteorology and hydrogeology clearly transcended his usual intellectual habitat and were met with great skepticism. Thus scholars had reasons for not meeting Lamarck's ***Philosophie Zoologique*** – a thesis that again exceeded his field of expertise as it mainly focused on vertebrates – with enthusiasm, benevolent trust and intellectual charity.

Importantly, the literature suggests that the lack of trust in Lamarck was not merely self-inflicted: During his life time and even after his death, Georges Cuvier remained the most hostile rival to Lamarck. Cuvier's rejection of Lamarck might also be partly explained by the fact, that it was Lamarck, who scorned Cuvier's catastrophism – the thesis to explain the difference between fossils and living organism through large-scale species extinctions through catastrophes – as unscientific. Thus, he made Cuvier an enemy before he had published any thoughts on evolutionary theory (Burkhardt, 1972, 422).

Cuvier's disapproval of Lamarckian accounts on species transformation is coined as one of the most important reasons, why his evolutionary theory was received so poorly. Cuvier, had, opposed to Lamarck, a strong belief in the primacy of scientific facts, and pleaded for a "silence over the systems" (Cuvier, cited in Burkhardt, 1970, 292). As Cuvier was an important figure in the scientific landscape of France, his recommended silence over Lamarck's theories was met by his contemporaries. Unfortunately, this recommended public silence renders it very difficult to assess positions of several scientists towards Lamarck's evolutionary theory as there are few – if any – comments on it.

Also, in Cuvier's accounts on Lamarck, he always quoted him simultaneously with many obscure and disreputable characters (Burkhardt, 1970, 295). Conclusively, Lamarck's theories were publically neglected and privately ridiculed, a combination similar to a death sentence for a scientific theory.

Nevertheless, it is impossible to criticize Cuvier's dogmatic rejection of evolutionary theories, without conceding, that Lamarck rendered it rather difficult to find the epistemic charity to engage into his thinking. Regardless, Cuvier chose to take scientific credibility from Lamarck's idea *a priori* and did not even begin to level serious – justified – scientific objections against it (Burkhardt, 2013, 800).

Importantly, Lamarck was not frustrated, because his works were discussed and rejected, but because they found barely any attention, probably due to Cuvier's belligerence against Lamarck and the silence over Lamarck's theories Cuvier suggested. Firmly believing in his theories, Lamarck suspected that his considerations were omitted on purpose, to keep him away from fruitful discussion and acknowledgment, by researchers who wanted to preserve their own theories:

"There is a certain number of henchmen of greater works, that are widespread in every social stratum, who do not attack their common enemy openly, but ruin him through obscurity, which deprives him of any means of defense." (Lamarck cited in Burkhardt, 1970, 284).[7]

His idée fixe about a conspiracy is most probably too far-fetched, however, the reception and critical discussion of his work was seriously impeded through Cuvier. It seems that because of his assumptions on his fellow-scientists Lamarck did not bother about the impression his ***Philosophie Zoologique*** would make on his contemporaries but already expected their rejection.

Additionally, as he was gradually losing his sight and also his overall health was failing, Lamarck was in fear of not being able to finish his works before his death. Thus, he might have omitted thorough explanations of his theories as he felt he was lacking time and generally did consider every attempt to make his theses acceptable to the scientific public to

[7] Translated by S.V.; original: "Ainsi un certain nombre de suppôts du grand oeuvre, répandus dans tous les coins de société n'attaquant jamais ouvertement l'ennemi commun, mais le ruinant partout dans l'obscurité, ne lui laissent aucun moyen de defense."

be in vein. These circumstances led to his major goal being to record all his thoughts (Burkhardt, 1970, 287).

Importantly, his contemporaries gave accounts on his unwillingness to respond to criticism in a productive way: "It was always himself and he could not receive anything from others"[8] (Henri-Marie Ducrotay de Blainville, cited in Burkhardt, 1970, 289). Conclusively, Lamarck was not only actively excluded by the campaigning of Cuvier, but did also exclude himself from scientific debates as he was not willing to make any changes to his conceptions or acknowledge any comments.

Another possible reason for the bad reception of Lamarck's theories is the fact that people expected something different of him than his ***Philosophie Zoologique***: After having switched from botany to zoology, Lamarck *was* the expert scientist in the field of conchology. After the death of his friend Jean-Guillaume Brugière, who was the leading conchologist of his time, Lamarck was seen as the intellectual heir of his late friend (Burkhardt, 1972, 421). In 1798 he promised an extensive work on mussels of present and past. This work was much expected, as the debate around the fact, that certain fossils did not resemble living animals, was hoped to be settled by detailed studies of various taxa and the comparison between recent and fossil forms to shed light on the question of species extinction. In spite of that, Lamarck never published the much-desired ***Èlémens de chonchyliologie***, but engaged with evolutionary studies on all animals, although he was at most seen as an expert on invertebrates and especially molluscs. Additionally, it was also no smart move of Lamarck to omit mussels in his deliberations, as the debate around extinction was much shaped by the discourse of conchologists and might have brought is ***Philosophie Zoologique*** to their attention (Burkhardt, 1972, 420).

Taken together, the discussion of both narrow and broad reasons for rejecting Lamarck's theory shows, that a combination of both caused the – if even existent - negative reception of his works: For instance, the assumption, that his colleagues generally rejected all his works might have caused him not to feel the need to elaborate certain parts of his theories with better and more fitting evidence. Also, Lamarck could or did not want to acknowledge which investigations and case studies would have met the expectations of his colleagues and would have helped them to lay higher trust in his theorizing. Burkhardt concludes, that the combination of providing a speculative theory, that left open verification to those who were

[8] Translated by S.V.; original: "Il était lui-même et ne pouvait rien recevoir d'ailleurs"

sympathetic to him and the fact that he did not make any effort in convincing his fellow scientists in 1809 – at a time when he was already discredited through his monographs of the 1790s and 1800s – was a very bad decision (Burkardt, 1970, 288).

Was Lamarck a Failure?

One of the most important points, critics utter when constructing the history of Lamarck's evolutionary theory as a failure, is the claim, that ideas of evolution where "in the air" at the end of the 18th and at the beginning of the 19th century. Following this argumentation, Lamarck failed as he did not assemble these ideas to the concept of natural selection. Instead, Darwin was successful as he managed to complete this task. Although interpretations might vary, a failure can only be attested, when it can be shown, that Lamarck actually tried to find a principle of natural selection. In the following section, I will try to show, that this was none of Lamarck's intentions.

Lamarck is often characterized in the history of science as the precursor of Darwin, thus establishing continuity and providing a further reason for the attestation of failure. Lamarck's work, however, is mostly measured against Darwinist principles, a measure that was not accessible to him as it was postulated 30 years after his death (Sheets-Johnstone, 1982, 449). Therefore, he is often characterized as a *révolutionnaire manqué*.

Importantly, Lamarck's concept of the world and nature impeded theorizing towards natural selection. As he believed optimistically in a balanced, housekeeping nature he would never have used a concept like the "struggle for existence". Additionally, Thomas Malthus' thesis on population dynamics, which influenced Darwin much in formulating that more individuals are produced than can possibly survive, was translated into French as late as in 1805, a point in Lamarck's intellectual career, where he already had almost fully developed his evolutionary theory.

Also, Lamarck's rejection of chance as an important factor of evolution constitutes a major break between his and Darwin's theory. Additionally, his concept of a well-ordered external world was deeply interwoven with his belief in god: "…not only how Lamarck saw things, but also what he saw was influenced by his dedication to a God-given rationality." (Sheets-Johnstone, 1982, 454).

It is important to note that what separates Lamarck from Darwin is not his believe in God - as Darwin was also religious – but the interwoven-ness of his belief and faith with his work as a naturalist. In the legacy of Descartes, Lamarck saw himself as a "rational man

reading the rationality of the universe" (Sheets-Johnstone, 1982, 455): "Everybody knows that the word "chance" only refers to our ignorance of causes."[9] (Lamarck, 1835, 270). Thus, his rejection of chance also shaped his conception of variations: he interpreted them as intrusions and effects, but not as causes. Conclusively, his work does actually explain the origin of variations through his "fluides incontenables" whereas Darwin explained the origin of species through variation. Thus their notions of cause and effect were directly opposite. In his conception of a well-ordered nature, there was either complete adaption parallel to the plasticity of the circumstances or none at all what also caused him to not believe in extinction but in the gradual modification of all organisms of one species (Sheets-Johnstone, 1982, 457f). Nevertheless, it needs to be conceded, that Lamarck formulated his theses on transmutation long before cell theory, Mendel's work on heredity and a theory of fertilization that involved both sperm and egg were even near their discovery (Burkhardt, 2013, 801).

Thus I would suggest, that instead of directly contrasting Lamarck and Darwin by awarding the latter with the winning trophy, two questions need to be asked when criticizing Lamarck, in order to give an unbiased account of his work and the intellectual and empirical possibilities of his times: 1) Could Lamarck have developed a Darwinian evolutionary theory (if he would have wanted…)? 2) Could somebody else in France at the beginning of the 19th century (with a different weltanschauung) have developed such a theory? Tentatively and due to the considerations I offered above I would answer both questions with a "No".

Origin of Species, Neo-Darwinism and Neo-Lamarckism

In the following I will first give an account on the immediate reception of Darwin's ***Origin of Species*** and will try to illustrate, which conditions differed to those of Lamarck's time. Second, I will sketch the debates around Neo-Darwinism and Neo-Lamarckism of the 19th and 20th century – a period coined by the term "Eclipse of Darwinism", until the case seemed to be settled in the favor of Darwinism in the 1940s. Third, I will give an account on several epigenetic phenomena, which suggest, that both Lamarckian concepts of inheritance should be again considered when trying to describe the inheritance of acquired traits.

Darwin & The Origin of Species

For discussing the reception of Darwin and his **Origin of Species** a quote of biologist and T.H. Huxley-scholar Cyril Bibby will serve as a leitmotiv: "The validity of a scientific theory is nowise determined by its consonance or conflict with popular opinion, or by the readiness

[9] Translated by S.V.; original: "Ne sait-on pas, d'ailleurs, que le mot de hazard n'exprime que notre ignorance des causes"

of experts to move in its direction, or by the personal characters of its proponents or opponents, or by its convenience as ideological camouflage for vested interests. All these, however, may markedly influence the speed with which a new theory gains ground, and this was conspicuously the case with Darwin's theory." (Bibby, 1959, 83)

Importantly Philip Kitcher argues, that the Darwinian evolutionary theory, first exposed in his ***On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life***[10] did not enter into a vacuum, and did also not enter into Lamarck-dominated scientific thinking but was published into an epistemic field of various biologists doing biology in various ways (Kitcher, 1993, 11ff). As the previous section has shown, Lamarck's theory was never *the* paradigm on evolution but rather rejected already by his French contemporaries and even less received in other European countries. This stresses the question, why the impact of Darwin's ***Origin of Species*** was that immense. It is important to note that Darwin managed to diligently align vast numbers of facts to support his claims and also was capable of using a language that was simple in style and statement but still compelling, so that it was easily receivable by the public (Bibby, 1959, 76). Also, contrarily to Lamarck, before publishing his Origin of Species it took Darwin years to convince is scientific microenvironment of his theories, thus he had some valuable allies with good institutional standing – such as Huxley, Hooker and to a certain extent also Lyell - from the beginning (Desmond and Moore, 1990, 501).

An important difference concerning the reception of both theories arises because of the fact, that the debate around Darwin's propositions was a public debate, as his first and second editions sold out immediately and European translations as well as a North-American edition appeared rapidly after (Bibby, 1959, 81). Additionally, the debate between anatomist Richard Owen and Thomas Huxley, who was often coined as "Darwin's Bulldog" for his extensive belligerence in proving his enemy, Owen, wrong and to win the debate on the similarity of man and ape, contributed much to public awareness and interest. It is important to note, that animosities between Huxley and Owen reached long back and Huxley was eager to cast out older and more reactionary scientists as Owen was (Desmond and Moore, 1990, 489f). As this emotionalized discussion was of much public attention by the midst of the 1860s both "scholars and streetsweepers" knew that Huxley and thus Darwin was right. Throughout this enraged battle, Huxley also gained more and more influence and became the scientist of most

[10] In the following, this monography will be referred to as ***Origin of Species***

public attention. Also his alleged victory over Owen provided the public ground for believing that Darwin's theory was proven (Bibby, 1959, 79).

Also, resolving the debate around Darwinian evolution seemed to be of major importance for theology. Naturally, the public awareness was raised through the extensive uproar of Darwin's opponents which can partly be explained by the fact, that since the translation of David Friedrich Strauß's ***Leben Jesu*** there was a lot of debate on ideas coined as subversive in British theology, thus the ***Origin of Species*** seemed to the average believer as an assault on the foundations of belief (Bibby, 1959, 77f). Especially Huxley overtly aimed at disuniting Science and Theology through evolution and fighting the authority of bishop Samuel – "Soapy Sam" - Wilberforce (Desmond and Moore, 1990, 535f). Importantly, there were also clergymen who were ready to endorse Darwin's views on evolution and combine them with their religious beliefs. As a result, supporting Darwin became a risk to one's theological carrier.

Additionally, the scientific world of Darwin's time is often described as already awaiting conviction, which was prepared through geologist James Hutton and especially geologist Charles Lyell, who made great impression among the younger scientists. Thus, the superiority of scripture and fixism was already contested. Thus, it is not clear, how well the critiques of Darwin could have withheld, if there was not a wide-spread readiness for evolutionary thoughts through Lyell, an already established high estimation of Darwin and his methodology, an advocate so brilliant and emotional as Huxley as well as so many splendid uses of Darwin's theories for pseudo-scientific apologism (from colonialism and capitalism to eugenics) (Bibby, 1959, 83).

What was Different?

The difference between the factors that led Lamarck and Darwin to the formulation of their theories on evolution is threefold: First, Darwin had first-hand acquaintance with variations of species around the world. Second, Darwin applied the principle of Malthus on species because of his observations. Third, Darwin had a critical awareness on the fact, that "like begets like" (Sheets-Johnstone, 1982, 452).

Contrarily to Darwin, Lamarck had a sedentary museum life, where he studied organisms without their real-life context, thus prone to miss principles like non-uniform variations of plants and animals in their natural habitats as well as their struggle for life, as he believed that nature was rationally and not empirically based. Thus neither in his theorizing

nor in his method he made distinctions between the rational and the empirical (Sheets-Johnstone, 1982, 459ff)

Also, Darwin had since the publishing of his Origin of Species the full attention of the public, whereas Lamarck's assumptions weren't even discussed by his colleagues. Importantly, Lamarck had not an ally, like Huxley, who laid great importance in convincing the Church of England, fellow scientists and the public of Darwin's theses. Considering this it is important to bear in mind that Darwin could not have fought such battles on his own as his health was – similar to Lamarck – already before 1859 severely compromised (Desmond and Moore, 1991, 586).

Importantly, I do not want to engage in discussions, whether evolutionary thought was "in the air" or not, or whether it was more in Darwin's air than Lamarck's. Conclusively, the two main reasons for Darwin's victory are both "narrow" and "broad": First, the support of his theories through many facts and empirical data provided objective grounds for accepting or dismissing his theories, as well as a basis to engage in debate on his theories. Second, the high publicity that was given to the ***Origin of Species*** through both its defenders as well as its opponents tremendously promoted its propagation, thus making possible, what Kitcher coins as Darwin's major achievement: "…to introduce schemata for answering certain families of biological questions and to identify the questions that biologists should address" (Kitcher 1993, 32).

Neo-Darwinism and Neo-Lamarckism

The period of 50 years following the publication of the ***Origin of Species*** are often called the "eclipse of Darwinism". This describes the state of science in the last decades of the 19th and the first years of the 20th century, when scientists generally believed in evolution but did not only embrace Darwin's explanations but also others, amongst them Lamarck's (Bowler, 2003, 169). Thus it was actually Darwin whose work initiated the first revival of Lamarck. Although there were other notions of evolution around, such as theistic interpretation, saltationism and orthogenesis, I will only focus on Neo-Darwinism and Neo-Lamarckism in the following account.

When talking about Neo-Darwinism, the most important figure in this enterprise – at least in its early days – was August Weismann. He was one of the first supporters of Darwin in Germany and performed experiments to further prove the principle of natural selection. Later,

in the 1880s he developed a theory that was interpreted to disprove the inheritance of acquired traits and thus he became the founder of Neo-Darwinism (Hoßfeld et al., 2014, 2).

It was a prerequisite for embracing such a theory that he was aware of the fact, that the germ cells are separated from somatic cells very early in development, thus rendering transmitting acquired traits to the next generation rather impossible. Rejecting this principle, he had to provide another explanation for arising variation, which he found in the *amphimixis*, the recombination of gametes, which was first described in the late 1870s (Hoßfeld et al., 2014, 6). Weismann's most serious blow against Lamarckism and it's public perception was the experimental proof against it he provided by cutting off tails of generations of rats and showing that tail sizes did not decrease in the course of generations (Koonin and Wolf, 2009, 2).

Importantly, the arguments for rejection of this kind of experimental proof are twofold: On the one hand, the fact, that circumcision does not cause a shortening of the foreskin in subsequent generation was a fact that was known since more than 2000 years. Thus, Weismann's show-case example did not provide any new knowledge. On the other hand, Weismann completely ignored that Lamarck mainly stressed the inheritance of beneficial changes caused by the use or disuse of organs, which is certainly not what Weismann did mimic by mutilation.

Neo-Lamarckism in the late 19th and the beginning of the 20th century was especially a commitment to the thesis, that the origin of variation is external. One of the reasons, besides the empirical, was the fact that Lamarck's theory was viewed as metaphysically superior to Darwins theories, as they preferred an ontology of human kind based on an ever-ongoing strive towards perfection to an ontology based on mere chance (Bailey, 1894, 663). In contrast to Neo-Darwinism, Neo-Lamarckism was an integration of both Lamarckian and Darwinian concepts and thus in opposition to Neo-Darwinism rejecting all Lamarckian influences. Along with the difficulties of both doctrines came the question where the burden of prove lies. To provide an example, it was argued, that the Neo-Darwinians or "Weismannians" are in demand of it, as they rejected phenomena that were earlier considered as established through Darwin's ***Origin of Species***, such as the principle of use and disuse of organs, which Darwin adopted from Lamarck (Bailey, 1894, 668). In the following, I will provide a compendium of experimental evidence which was provided for or against Neo-Lamarckism as well as Neo-Darwinism and I will conclude by discussing the reasons for the rejection of Neo-Lamarckism in the 1940s.

Especially entomology – the study of insects - provided the Neo-Darwinians with arguments against Neo-Lamarckism: it was, for example hard to explain, how the (non-living) exoskeleton of insects – which surrounds and supports the animal's body externaly – could be subjected to Lamarckian evolution during the life-time of an insect as it consists of "dead matter". Also the elaborate behavior of worker-bees who have no reproductive capacity and instinctive behaviors only performed once in life time left the Neo-Lamarckians in need of explanation (Haig, 2006, 418).

It is important to note that Neo-Darwinism was also not fully free of contradictions: as Weismann postulated, the complete separation of germ- and somatic plasma he could not explain switches in sexual and asexual reproduction as well as the fact that for instance begonias could be propagated through their leaves, without admitting certain exceptions of the rule, which would again let in "Neo-Lamarckist contaminations" (Bailey, 1894, 669f). Also, many Neo-Lamarckians argued, that the lack of variety within found fossils would corroborate a principle of directed and channeled evolution, as variations within one generation should be visible in the fossil evidence.

In spite of that it was a strong argument of Neo-Darwinians that even though environmental conditions might change, the physiology of e.g. mammals keeps the body under a strict thermal as well as saline homeostasis, thus questioning how environmental changes should make their way to the germ-cells (Huxley, 1942, 417).

The end of Neo-Lamarckism was partly decided by the fact, that in the first half of the 20th century, many experiments that were used as evidence for Lamarckian evolution were shown to be incorrect: The structural changes in plants through planting them at different altitudes was shown to be due to genetic contamination of the surrounding environment (Kohler, 2002, 167). Also alcohol intoxication mediated malformations in offspring of guinea-pigs was shown to be explained by Darwinian mechanisms (Blumberg, 2010, 69f). Additionally, the transmission of the ability to solve mazes quicker in rat offspring was shown to be due to poor experimental controls (Medawar. 1985, 168).

What also discredited Neo-Lamarckism was the work of Paul Kammerer, who claimed to have shown that salamanders and toads inherit a change of color-pattern to the next generation through exposure to certain environmental conditions until it was discovered that he injected black ink into his showcase animals. It remains yet unclear whether Kammerer only wanted to augment his results for the eyes of the public. Still, he discredited studies in

the favor of Lamarckism in general. Also Lysenko's campaign for Lamarckism did not help its reputation (Koonin and Wolf, 2009, 2f).

This shows, that besides August Weismann's campaign against the inheritance of acquired traits in the end of the 19th century, there was also a vigorous debate between Neo-Darwinists and Neo-Lamarckians in the 20th century. In the 1940s the debate seemed to be settled, what is illustrated by volumes like ***Evolution – the modern synthesis*** by Sir Julian Huxley, who provided to each claim of Lamarckian evolution a counterexample or rendered it unscientific.

Epigenetics – Lamarck's Revival?

The term of epigenetics was both coined by Conrad Waddington in 1942 and by David Nanney in 1958. Waddington believed that the environment has a causal role in development and in the induction of alternative phenotypes. His proposal did not challenge Neo-Darwinism in particular, as he suggested that adaptive triggers and genetic switches were products of natural selection. On the other hand, Nanney understood cellular heredity, not based on a mechanism of DNA replication as epigenetic, as he generally challenged the central dogma of molecular biology (Haig, 2006, 420).

In the second half of the 20th century, evidence arose, that DNA is not the only medium of inheritance in cells, and that environmental cues alter gene expression through modifications of DNA, which can be transmitted through mitosis and in rare cases even through meiosis. Thus, the status of Lamarckian evolutionary theory in biology was reconsidered. Importantly, there are several objections of Neo-Darwinists against interpreting epigenetic inheritance as Lamarckian: First, the possibilities in the epigenetic state of a DNA sequence is an inherent property of the sequence which is subject to natural selection and mutations. Second, natural selection promotes useful epigenetic switches and eliminates maladaptive ones. Third, the epigenetic landscape of a DNA sequence is steadily reset and thus cannot represent open-ended evolutionary change. Fourth, the epigenetic machinery is a set of proteins, whose sequences are incorporated by the DNA sequence and thus subject to mechanisms proposed by Darwinian evolutionary theory (Haig, 2006, 421). Thus, epigenetics itself is no challenge to Neo-Darwinians, but rather the possibility that their exist mechanisms, that are not random, but direct those epigenetic changes to the right places.

Jablonka and Lamb believe that the gene-centrism of current Neo-Darwinism is to blame for the incompatibility with Lamarckism. They argue that Lamarckism and Darwinism are compatible and complementary, even in the light of epigenetics. Thus they regard the

admission of Lamarckian principles to our current evolutionary theories as most beneficial (Jablonka and Lamb, 2006, 95). Such thought also leads in other cases for a plea towards a "unified theory" of evolution (Skinner, 2015, 1296). For instance, one hypothesis states, that strong environmental signal might trigger Lamarckian responses, whereas weak – usual – signals trigger Darwinian modalities of evolution. This proposes a spectrum of evolutionary adaption that embraces both Darwinian and Lamarckian scenarios of evolution (Koonin and Wolf, 2009, 9).

Importantly, the definition of acquired characters does in certain senses also seem problematic, for example :"If an epimutation is an acquired character merely because it is induced by the environment, then a genetic mutation, caused by exposure to radiation or a chemical mutagen, is also an acquired character" (Haig, 2006, 424). Thus, if radiation or chemical mutagenesis can be understood in a broader sense, such as stress, the argument gets weaker, as there are several accounts of when stressful environment elicits epigenetic transgenerational changes (e.g. Molinier, 2006). In the following paragraphs, I will provide examples of both epigenetic inheritance, as well as other phenomena that seem to prove the inheritance of acquired traits.

One instance of a genuinely Lamarckian process is the CRISPR-Cas (Clustered regulatory interspaced short palindromic repeats – CRISPR associated protein) system in prokaryotes: If a cell survives the attack of phages (viruses that infect cells of bacteria or archea), fragments of the viral genomes get integrated into the DNA of the cell and propagated to the next generation. Those fragments become small RNA guides that lead nucleases to their complementary sequence in newly attacking phage genomes, which are then target for degradation. Thus, a trait acquired through environmental conditions – a phage attack – was directly integrated into the genome, propagated and is beneficial as it prevents cell lysis through infection by phages of the same kind (for review, see Koonin and Wolf, 2009, 4f).

Similarly to CRISPR-Cas but much less explored is the integration of viral sequences and mobile elements into eukaryote genomes. For example, piRNAs (piwi-interacting RNAs) are fragments of mobile elements integrated in the genome which prevent the germ line from transposons (DNA sequences that change their position in the genome) by serving as guides for enzymes that degrade those transposons. Although it was shown, that some endo-siRNAs (small interfering RNAs which are encoded in the genome) are complementary to viral genes, their effect in active immune response has still to be confirmed. What is known up to now is

that genomes of viruses are fragmented within the cell after their attack to function as exo-siRNA (small interfering RNAs of exogenous origin) guides for infections to come, which persist in cells for several generations through RNA-mediated inheritance (for review, see Koonin and Wolf, 2009, 6).

An extreme case for the inheritance of acquired traits not based on changes in the nucleic acid is provided by prions. Prions coopt the last step of decoding genetic information – they ensure that a newly synthesized protein is folded correctly so that it is functional. Prions transmit information via their conformation – their very specific folding pattern - which is profoundly different from normal and causes several diseases such as spongiform encephalitis. Whereas the de-novo formation of a prion consumes much energy, the costs for the propagation of their state through serving as templates for other proteins during protein folding are minor. Certain extreme changes in pH, temperature or metabolites – conditions seen under stress - can cause an immediate increase of de-novo prion formation. This phenomenon can be interpreted as a bet-hedging strategy making the cell become a pluralist: under stressful conditions, several new protein phenotypes are tried out, hoping that one of them might prove beneficial. Not only can prions template their own propagation but they can also interact with other prion-forming proteins to convert them to their own prion states. In yeast, prions are disseminated to daughter cells after cell divisions. As they are stable during mating and meiosis they can also be transmitted to the germ line (Halfman and Lindquist, 2010, 630f).

It is important to note, that epigenetic phenomena interpreted as Lamarckian are not only important for basic research, but also are relevant for possible applicability in medicine: For example, extensive licking and grooming (LG) of their offspring by rat mothers leads to a high number of glucocorticoid receptors (GR) in the hippocampus, as genes encoding for GRs are hypomethylated (adding a methyl-group to DNA modifies the activity of the methylated sequence, as a rule, many methylation marks (hypermethylation) mean less activity, thus hypomethylation causes increased activity). This provides a parallel to Lamarck's use/disuse hypothesis, as the use of receptors that are respondent to licking causes the production of more such receptors. Most importantly increased methylation of GR genes in the offspring of low LGing mothers could be observed throughout three generations. Such methylation patterns in GR promoter regions were found by suicide victims abused as children (Handel and Ramagopalan, 2010, 2).

Also, some authors argue, that horizontal gene transfer, a process were a bacterium acquires a plasmid (circular DNA) from the environment, that offers adaptive value such as antibiotic resistance is also a manifestation of Lamarckism (Koonin and Wolf, 2009, 7). Additionally, there exists the notion, that stress induced mutations are quasi-Lamarckian, as they are triggered by environmental conditions and induce the adaption to the stress factor (Koonin and Wolf, 2009, 8).

Additionally, besides the study of molecular biology, Lamarck's original work should earn some credit for the emphasis on the role of behavior in the process of evolution. This principle was "rediscovered" in the 1960s by Alistair Hardy, but never associated with Lamarck's work (Burkhardt, 2014, 804).

Still, it remains a question, whether it is sufficient to call any inheritance of acquired traits Lamarckian or whether one insists on an interpretation of Lamarck that stays close to his text. In this case, the most demanding aspect in arguing for Lamarckian concepts in evolution would be the fact that Lamarck strictly insisted on a channeled mechanisms for adaptions, his "fluides incontennables". I will focus on this problem in chapter 4.2. As of yet, there has not been found any general mechanism which channels the modifications of the genome to certain locations. Nevertheless I would concede that the notion that Lamarckian evolution is an exception to the rule of Darwinian heredity seems very intriguing (Koonin and Wolf, 2009, 8).

Concluding Remarks – Scientific Revolutions, Experimenters' Regress

In this section, I will discuss and contrast Lamarck's and Darwin's conceptions of evolution by asking two questions: 1) Did their work lead to a scientific revolution? And 2) Can their discrepancies be characterized as an Experimenter's Regress? For doing so, I will also consider positions on evolution that preceded Lamarck and Darwin.

Was there a Revolution?

When discussing whether Lamarck, Darwin, or both brought about an "evolutionary revolution" I will omit other scientists, such as Erasmus Darwin, who also uttered thoughts supporting transformism. Importantly, as is illustrated by the debate between Klein and Chang, canvassed in section 3.1, the definition of what constitutes a scientific revolution can be interpreted in many ways. In spite of that, I will try four approaches towards attesting a revolution, namely fundamental changes concerning ontology, methodology, causal explanations and incommensurability.

In my opinion, the ontology assigned to living organisms by Lamarckian theory was fundamentally different and revolutionary compared to other theories in place. Against the notion of fixism, influenced by a literal interpretation of the bible, he provided an explanation for transformism. Importantly, Lamarck was not the first one, who recognized, that the differences between fossils and their living counterparts pose serious troubles to fixism, but was first in providing a synthetic explanation of this phenomenon. Thus, instead of arguing, that all species are the same since god has created them he argued, that all organisms would thrive towards perfection, and change their traits over time due to environmental changes. Thus he created a historical axis to the ontology of living organisms. Considering this change as the major difference between fixism and *all* evolutionary theories that argue the mutability of species, I would then conclude, that concerning the ontologies, Lamarck's and Darwin's assumptions were revolutionary in the same way, however, Lamarck was first to utter them.

On the other hand, one could of course argue, that a revolution can only be attested if more people than the author of the revolutionary thesis are convinced by it. If this position is taken, the credit for bringing about a revolution should certainly be awarded to Darwin. In the same vein, I would argue, that Erasmus Darwin's views on mutability contemporaneous to Lamarck's might not be classified as revolution-material, as they were uttered in the context of poetry, thus lacking the right medium – of his time, and also of ours – for scientific thoughts that might cause a revolution. Thus the question remains, whether the scientist who was first or the scientist who was successful should get the credits for bringing about a scientific revolution concerning the ontology of organisms. Of course, a similar question could be asked when considering the work of Alfred Russel Wallace's, who had simultaneously to Darwin the same thoughts but was too slow in publishing them. For better illustration, I would argue that the case of Lamarck and Darwin has its similarity to the "discovery" of America by the Vikings and the Europeans. If writing the winner's history then it is clearly Darwin who brought by the revolution, but as Darwin was both aware of the theory of his grandfather as well as of Lamarck's – especially because of its critical discussion by Lyell in the 1830s – he certainly did not make the intellectual but the formal contribution – in providing good explanations and proof – for the revolution.

When discussing, whether Lamarck or Darwin brought about a revolution in terms of scientific method, it needs to be questioned, whether Lamarck's methodology can actually be described in a satisfactory way. As discussed in the previous sections, it is generally thought, that Lamarck made his claims about evolution not based on specific empirical evidence but

rather independently and in accordance with a general set of beliefs. This kind of "methodology" was already discredited by his contemporaries, who acknowledged facts over grand theories. On the other hand, Darwin made most definitely first observations and then induced his theories from them. This was in principle nothing new and innovative. Also, expeditions to gain knowledge on the flora and fauna of other countries were a practice that reaches at least back to Carl Linaeus. Additionally, as discussed before, Darwin's attention for the struggle for existence was elicited through the works of Malthus, thus not methodology but rather his background knowledge facilitated the formulation of his thesis. Conclusively, neither Lamarck nor Darwin changed scientific methodology of their discipline.

Importantly, Darwin was revolutionary concerning the causal explanation of evolution. Contrarily to Lamarck, who saw variation as the effect of speciation, Darwin argued variation to be the cause of speciation. Also his admittance of chance, instead of intangible "fluids incontennables" was revolutionary. On the contrary, explanations based on certain electric or thermal fluids were quite common in Lamarck's time and had therefor no revolutionary potential. Thus, Darwin's admittance of chance in a process that was formerly believed to be in god's hands was certainly revolutionary.

Concerning incommensurability, I would argue, that Lamarck's and Darwin's original theories are incommensurable, but in a very mild form. As elaborated in the previous section, the two main differences between Lamarck and Darwin are 1) the elaboration and corroboration of their theses and 2) the contrary characterization of cause and effect of evolutionary processes. Thus, if both would see a certain species, adapted to a certain environment, the Darwinist would claim that it varied and thus adapted, whereas the Lamarckist would claim that it adapted and thus varied.

Importantly, Darwin introduced auxiliary hypothesis in order to explain adaption, namely the struggle for existence and the influence of chance on variation. One was not accessible whereas the other was not acceptable to Lamarck. As indicated before, Darwin believed in the principle of use and disuse of organs, although it was not central to his thesis. Additionally, there exists a "modern synthesis" of Lamarckism and Darwinism when taken to the molecular level as canvassed above. Thus I claim, that on the individual level (especially Lamarck's) both theories are incommensurable, as both Lamarck and Darwin would have regarded the same evidence as corroboration for their respective thesis. Conclusively I would argue that through the discovery of the molecular mechanisms underlying heredity and

especially because of the notions of integration Neo-Lamarckians argue for, Neo-Lamarckism and Neo-Darwinism might not fulfill the criteria for attesting incommensurability.

In conclusion, I would argue, similar to my elaborations on ontologies, that Lamarck's theory was clearly incommensurable to both fixism and catastrophism (as an explanation for fixism). The conceptual frameworks of Lamarck's transformism and – e.g. Cuvier's – fixism did not overlap enough, so that the same empirical evidence could favour each theory, relative to the scientist assessing it. For example, the discovery of a fossil, not resembling any living organism on planet earth would be explained by the "fixist": Obviously, this organism lived before the last catastrophe – i.e. the Noachian Flood – thus it does not resemble any of our living organisms. On the other hand, the transformist would state: This provides evidence for my thesis, as living beings adapt to their habitats, the habitats of this particular organism changed, thus the current living form looks different to its predecessor.

Conclusively, especially Darwin's causal explanations offered for the process of evolution are revolutionary in compare to Lamarck. Concerning species ontology, I am rather inclined to attest some revolutionary credit to Lamarck.

Experimenters' Regress

When considering, whether the theories of Lamarck and Darwin constituted an experimenter's regress I will also include the current debate between Neo-Lamarckians and Neo-Darwinians into my considerations. First and foremost it is important to note that Lamarck was no experimenter. Neither was Darwin. Both made observations, which provided them with data. Thus, arguments about competently performed experiments could not have arisen. Also, certain characteristics of the experimenters' regress do not fit to Lamarck and Darwin, for instance, as they were not contemporaries they could not engage in dispute. Also, debates being characterized as an experimenters' regress constitute cutting-edge science, however, Lamarck was never conceived as cutting-edge.

Thus there are two possibilities of characterizing the controversies between Lamarck and Darwin. On the one hand one could argue that their dispute is underdetermined, as when making the same observations, they would deduce different theories from them. On the other hand, one could claim an experimenters' regress, as they would not have been able to agree neither on the data, which their observations yielded nor on the theories that were drawn from them.

I believe, that the case of Lamarck and Darwin provides on all accounts a case for underdetermination, as from the same data – e.g. the difference between the past and present morphology of a mussel – they would have deduced different theories of species evolution. Due to the reasons canvassed above, it is yet harder to argue for the presence of an experimenter's regress. However, they had two different styles of making their observations, which provided them with data. Darwin observed during his expedition with the Beagle *living* organisms in their *native* environment and took many samples for later analysis. Lamarck, however, made his observations, mostly based on *dead* material, left alone the animals in the menagerie (for instance, *one* elephant), who lived in an *artificial* environment. Additionally, as mentioned in previous sections, Lamarck regarded himself as a *naturaliste philosophe* and thus his weltanschauung influenced his observations. Thus, I conclude that Lamarck and Darwin would not agree about through their respective observations competently produced data.

Importantly, the debate between Neo-Lamarckians and Neo-Darwinians proves another case for an experimenter's regress and is additionally a dispute of cutting-edge science. As canvassed in the previous section, there are generally four arguments provided by Neo-Darwinians, to show, that all epigenetic phenomena can still be explained without considering Neo-Lamarckism. In spite of that Neo-Lamarckism aims at combining both theories and using each explanation where most appropriate whereas Neo-Darwinians exclude any Lamarckian thought.

Thus I would argue, that especially the outcome of experiments trying to prove inheritance of acquired traits shows some features of an experimenters' regress, as both positions regard a different outcome as desirable (the existence or non-existence of heritable acquired traits). To offer an example I will discuss the case of horizontal gene transfer in bacteria. Horizontal – also often termed lateral – gene transfer means the transmission of genes within one generation. Thus, genetic material is not transmitted from the parental to the filial generation (vertical gene transfer) but from one bacterium of the same generation to another. Through e.g. cell-to-cell contacts of two bacteria or "shipping" by a virus (a bacteriophage) a certain piece of DNA, which confers resistances against antibiotics or other benefits can be transmitted from one cell to another. The recipient cell has therefor acquired a specific trait through its respective environment and as it is genetic material it can inherit it to the next generation. Importantly, Neo-Darwinians interpret this phenomenon as merely another way of sex, where not another allele but another gene is acquired. Contrarily, a Neo-

Lamarckian would interpret it as the propagation of an acquired trait. Thus, there is neither consensus about the experimental data nor about the theories drawn from it.

Another argument leveled against Neo-Lamarckism is that attesting a Lamarckian touch to certain phenomena only obscures the simple ways by which nature operates (Weiss, 2015). On the other hand, I do not think, that considerations concerning the instruments in use are much in debate, as they are broadly the same within molecular biology. In spite of that, questions about competently performed experiments might of course arise. For example, some authors argue that those effects observed transgenerationally might be due to other factors, such as the transmitted microbiome and its metabolites, thus claiming that the experiments of Neo-Lamarckians are not competently performed as such factors are neither controlled in experimental setups nor considered in analysis (Heard, 2014).

In conclusion, both the debate between Darwin and Lamarck as well as between Neo-Lamarckians and Neo-Darwinians can be interpreted as an experimenters' regress. Importantly, while the former is merely hypothetical as it has never taken place in that form, the latter is a phenomenon of nowaday's cutting edge science.

Referring to Kitcher's schemes of values, which I discussed in chapter 3.2 I want to argue, that I believe that Neo-Lamarckism and Neo-Darwinism are reconcilable. Clearly, both parties share their cognitive set of values, as both hold important to investigate by which mechanisms heredity operates. Still, it is in question, whether Neo-Lamarckians and Neo-Darwinians are reconcilable in their probative scheme of values. They both consider a specific question as crucial: both want and need to investigate, whether acquired traits are heritable. Admittedly, this research question is most crucial for the Neo-Lamarckians, whereas Neo-Darwinist secondarily needs to engage with it in order to counter her opponent's theories. Given the outline of their schemes of value, I believe, that both could be integrated in a system that, as proposed by others, embraces both Neo-Lamarckian and Neo-Darwinian explanations, where each fits best. Still, a demand for simplicity needs to be applied and Darwinian hereditary theory is less complicated and complex than epigenetic mechanisms. Thus, the main challenge concerning either a full integration of both positions or at least a very beneficial pluralism is the acceptance of Neo-Lamarckian heredity by die-hard Neo-Darwinians.

4.2 A Case for Pluralism?

Having reconstructed the history of the debate between Lamarckism and Darwinism as well as Neo-Lamarckism and Neo-Darwinism in this chapter my aims are twofold. On the one hand, I will try to argue, that even though Lamarckism seems to have some grounds in current molecular biology – although this is also debated – researchers at Lamarck's as well as Darwin's time were more than justified to repudiate his evolutionary theory. This seems to suggest that this specific paradigm shift led to a tremendous amount of Kuhn loss, which only Chang's pluralism could have prevented by keeping Lamarcks theory around. Thus, it needs to be discussed what could have happened if Lamarckism would have been kept alive all along and whether this would have been beneficial for science.

The second aim of this chapter is to ask, where there is room for plurality, by one the one hand questioning, whether Chang's campaign would convince all scientists. On the other hand I will consider the possibility that scientists might resent to have to engage in research of a scientific paradigm they reject. Also I will discuss problems that would arise when e.g. inviting creationist to conferences or letting them publish in science magazines. This of course will also lead to considering science policy when dealing with instances of pluralism and Kitcher's deliberations on well-ordered science.

Justification vs. Pluralism?

Chang's historical and hypothetical evidence for pluralism stems exclusively from the discipline of physics, and thus he claims: "Whether my arguments hold up with respect to other sciences is something I leave to the judgment of better-informed scholars." (Chang, 2012, 254). Thus I tried to elaborate the historical settings (as they are threefold – (1) Lamarck/Darwin, (2) Neo-Lamarckism/Neo-Darwinism and (3) current molecular biology) of another case, that might serve as evidence for Chang's prescriptive pluralism. Forms of pluralism that apply in this scenario are twofold: conservationist pluralism – i.e. keeping a theory around even if it seems that there is a better one at hand - is in place for arguing the historical cases of Lamarck and Darwin whereas Chang's active, normative and epistemic pluralism should be applied for assessing the debate around epigenetics and the inheritance of acquired traits.

Having chosen to test the case of scientific pluralism and Chang's thesis on Darwinism vs. Lamarckism might seem somehow self-evident and not as bold as Chang's approach on phlogiston: As epigenetics has initiated the revival of Lamarckian theories of the inheritance of acquired traits caused by the environment, the case seems to be somehow won by pluralism

at first glance. A pluralist might for example argue, that if Lamarckism would have been kept alive for a longer time and if the somehow clumsy experiments of the Neo-Darwinist August Weismann, who cut off the tails of 901 mice and showed that all mice in the next generation do have again tails would not have been taken as an instance of decisive evidence for refuting Lamarckism, epigenetic phenomena might have been recognized earlier, seed/soil and nature/nurture debates would have been discussed differently and also the so-called "epigenetic cures" could already be in use nowadays.

Further, it also needs to be discussed whether Lamarck's original theories have much in common with modern Neo-Lamarckism and thus could have led to epigenetics. Also, as elaborated by Philip Kitcher, it needs to be asked, whether the advances in genetics that were propelled through Darwinian theories and especially the synthesis with Mendelian laws, providing the molecular basis for his theory, were necessary for the discovery of epigenetic phenomena. Additionally, it has to be asked, whether the relevant scholars at certain time points, would have been able to access and evaluate Lamarckian theories properly. And lastly, it needs to be questioned, whether the advances in genetics could have been slowed down by a co-cultivation of Lamarckism to an extent that would severely shrink our genetic knowledge in a sense that e.g. certain cures for life-threatening diseases would not have been developed yet.

Should Lamarckism have lived?

First I will discuss whether Lamarck should have been accepted at the time of his publications and – although empirically insufficient – should have been preserved at least until Darwin to provide a basis for scientific assessment of both theories at Darwin's time.

In the last chapter, I provided both the "broad" and the "narrow" reasons for rejecting Lamarck. Thus I concluded that researchers were justified in rejecting his thesis, mostly because of the fact, that Lamarck did not present any empirical evidence and did not even aim at making his thesis understood by his colleagues. As stated in the previous chapter, the practice by which it was rejected seems to me altogether reprehensible: Cuvier's campaign against Lamarck, who ridiculed his theories and suggested scientific silence over them sullied the epistemic reasons of his colleagues for rejecting transformism. Also, he wrote an "eulogy" on Lamarck after his death and red it to the Academy of Science in order to make sure that the public opinion on Lamarck's scientific work would not improve for the next decades to come (Cuvier, 1835).

It is important to note that for both the theories of Cuvier and Lamarck there was no decisive evidence, as neither a belief in catastrophes inspired by the Old Testament nor an ontological conviction on the well-orderedness of nature fulfills this criterion.

In spite of that it is my opinion that pluralism is not necessary to prevent such lobbying in science – Kitcher's well-ordered science for example is sufficient. Ridiculing Lamarck could then be identified as Cuvier's broad scheme of value. Thus, Cuvier's attacks would have been seen in the light of this and would have been separated from the scientific points and evidence both rival theories, fixism and transformism, could provide.

If Chang's pluralism would have been applied, fixism and transformism would have been kept alive, and scholars would have been required to engage in gathering evidence for the one or the other theory – at least until Darwin. In spite of that, I argue, that through the provided "broad" and "narrow" reasons for rejecting Lamarck's theories, no scientist would have taken interest in trying to prove Lamarck's theories and it would have proved difficult to demand of any scientist to do so.

In the light of the publication of Darwin's ***Origin of Species***, the case for pluralism seems even weaker. Darwin provided both causal explanations and evidence for his theory of evolution which was clearly superior to Lamarck's work. Given the initial reasons I provided for rejecting Lamarck's theory and the careful considerations on him by Lyell and the shortcomings considering its elaboration and corroboration, I do not think that it would have been rational in any sense to maintain Lamarckism after Darwin's publication.

Thus I argue, that Lamarck's theory was serious enough, to be leveled against fixism and catastrophism and worthy of discussion by contemporary scientists. Whether Lamarck would have responded to critique in a cooperative way is of course beyond my judgment, but probably he might have not (Burkhardt, 1970, 289).

It is important to note, that Lamarck's theories were accessible all the time. They were discussed by Lyell, received and adopted by Darwin, they were at hand for the Neo-Lamarckists during the "eclipse of Darwinism" and although not immediately, they were revisited when epigenetic phenomena arose. Thus, I would argue – in a very minimal-pluralist manner – that the accessibility of *scientific* theories, that were rejected for several reasons, should be ensured and fostered. I believe that the theoretical preservation is not only interesting for the history of scientific ideas but secures a theoretical plurality of explanations. This would also be less problematic in terms of science policy, as of course scientists could

engage in this practice and might often be asked for advice, but this task can mainly be handed over to historians and philosophers of science. Due to the interest, that has already been shown in scientific revolutions and losing sites in the history of science, I do not believe that the assignment of such a task would force anybody to do what she dislikes.

One of the strongest arguments concerning preservative pluralism in the case of Lamarckism is the attestation of Kuhn loss. The inheritance of acquired traits was omitted in any evolutionary theory until and after Darwin and was again secluded in the 1940s when Neo-Lamarckism seemed altogether defeated. As of today it seems that Lamarck's theories find evidence in contemporary scientific inquiry.

Thus, the question needs to be asked, how Kuhn loss could have been prevented, even though I claimed that from 1810 to very recent times people were justified in rejecting Lamarckian thoughts. One solution to this problem might be to claim, that scientific revolutions provide a special case and thus in the rare and extreme cases where one takes place, also the old paradigms should be preserved. In this way one could prevent science from reckless triumphalism and its risks. From my discussion of the revolutionary character of both Darwin's and Lamarck's theories, I infer, that the often coined "Darwinian Revolution" can be treated as such.

Thus, one could argue that as there was *dramatic* change and reformation concerning scientific beliefs, as Kuhn loss took place. Thus, theories that were part of debate before the publication of the ***Origin of Species*** should have been further fostered, in order to hedge one's bets and identify the elements of Kuhn loss and assess whether there are important questions that still seem important and are not tackled by the new paradigm. Such questions could then be „rescued", by trying active integration or co-optation or at least kept alive until the new paradigm manages to tackle these questions itself or the question seems completely irrelevant. In the same vein concerning the chemical revolution, one could argue, that in the short period in which Lavoisierian chemistry completely overran phlogiston theory, it was certainly not possible, to integrate all important problem-fields of phlogistonism into the new paradigm and thus, phlogiston should have been kept alive for reasons similar to those sketched above.

Of course, this proposal demands, that not only historians or philosopher of science - to whom I attributed an *a priori* interest in the history of ideas and thus also already rejected theories in science - but also scientists would need to engage in such an endeavor in order to assess the relevance of the questions and problem fields that would need to be preserved from

Kuhn loss. Although also other scholars could try the assessment, in the end, the scientists would need to be willing to foster those ideas, questions and theories of the old paradigm in their laboratories and probably with their money: Thus, it depends on their willingness to do so and on their awareness of the set of problems of Kuhn loss. It also needs to be discussed how long such a transitory state should last. Admittedly, as Kuhn claimed, scientific revolutions are very rare and thus such a prescriptive awareness concerning Kuhn loss would bother maybe one in five generations of researchers.

In conclusion, special sanctions concerning theories of an old paradigm would help to prevent Kuhn loss in a case like that of Lamarckism and Darwinism. Nevertheless, they depend on the willingness of scientists to preserve those theories and their eagerness to engage with the exciting new paradigm. Still, raising awareness of researchers on the problem field of Kuhn loss and scientific revolutions would in my opinion be a worthy addition to their scientific curriculum and such considerations might also influence science policy.

After having discussed reasons for the justification of the rejection of the theories of Lamarck and suggesting two very tentative principles - maintenance of accessibility and special attention to scientific revolutions - I will engage in theorizing to which extent phenomena observed in modern molecular biology should be identified as Lamarckian phenomena and what would have happened if Lamarckism would have been kept alive.

The „eclipse of Darwinism“ resembles in certain aspects what Chang cherishes as the cultivation of multiple systems of practice: as a lot of researchers were dissatisfied with Darwin’s explanation for the process of evolution, alternative theories were tried out and experiments in these systems of practice were performed. Still, at least in the example of Neo-Lamarckism as discussed in chapter 4.1, almost all experiments were shown to be fraudulent or badly performed. In my opinion, this disqualifies this example as a historical show-case for the benefits of pluralism.

Importantly, a lot of biologists and philosophers of biology claim, that one should not be too rash to interpret the phenomena of epigenetics as the prophecy of Lamarck come true. When the question arises whether we should grant Lamarck the honor of being the very first epigeneticist we need to consider three possible levels of this argumentation that vary in depth: 1) If we focus on Lamarck’s thesis on the inheritance of acquired traits as a very broad concept, then we might feel very inclined to agree, that epigenetics represents Lamarckian phenomena. 2) If we leave the surface of Lamarck’s theory, we arrive at his understanding,

that the use or disuse of organs leads to their reduction or further development. Interpreted metaphorically - and indeed some researchers try to make links to this concepts in their research - there do not arise any problem with this concepts either, if we interpret „organs“ as well as „use“ and „disuse“ more broadly and match and translate it to concepts of our molecular biological understanding. 3) The last level needs to deal with Lamarck's hypothesis that certain „fluides incontennables“ guide reduction or further development of organs. In my opinion, this is the level were most problems arise:

On the one hand, when associating these fluids with his phlogistonist convictions, they need to be rejected. Of course, it might also be unnecessary to say that there are no such fluids involved in the molecular mechanisms of epigenetics. On the other hand, these fluids could be treated as an auxiliary hypothesis. Also, as stated in the previous section, explanations based on the existence of thermal or electric fluids were quite fashionable in Lamarck's time and thus we could grant him some interpretative charity. Assuming such a charitable interpretation, one could argue, that through his studies and considerations, Lamarck did „discover“ the inheritance of acquired traits but had at his time not the scientific insight to claim an appropriate explanation. Thus he came up with the auxiliary hypothesis on fluids directing the effects of use and disuse. Especially due to these historical conditions one might be justified in dismissing fluids while still maintaining the two other levels of Lamarck's theory, because Lamarck made the correct inference from his observation but lacked the molecular insight to explain them.

I strongly believe that Lamarck should certainly not be revived in a way, claiming that he has foreseen what molecular biology achieved to show in the second half of the 20th century. Importantly, I agree with Chang that pluralist inclination can help in preventing the rule of the "winner's history". Instead of presenting Lamarck in high-school biology teaching and undergraduate university courses as the "loony with the giraffe", he should be taken seriously in the history of ideas concerning evolution. It should be accredited that Darwin drew on some of his principles and the historical context in which he uttered his thesis – against fixism – should be taken into account when evaluating his work. Also, it is often not remembered that Lamarck was the first to discern vertebrates from invertebrates and amongst a few others he coined the term "biology". In addition, he was a well-respected botanist and conchologist.

While the empirical adequacy of Lamarck's work needs of course to be criticized - also due to the fact, that even his contemporaries viewed it as insufficient - I do embrace the apologetic version of the discussion of Lamarck's fluids and thus I believe, that when talking

about phenomena of the inheritance of acquired traits, one is justified in referring to Lamarck and also re-establishing his reputation because of them. I do strongly believe, that until the discovery of modern epigenetics, people were justified in rejecting Lamarckism for their explanations of evolution: On the one hand because of the inadequacy of Lamarck's elaborations, on the other hand - especially for Neo-Lamarckians – the non-scientific reasons for rejecting Darwinism, as prefigured conceptions of mankind and nature - including some kind of teleology that would not accept a factor such as chance - were never based on scientific grounds.

What if…?

My last point in assessing the justification of abandoning Lamarckism at his time and also in the light of Darwin's ***Origin of Species*** is to engage in musing „What if…?". There are several possibilities what could have happened if a „science-policy pope" of the 19th century would have ordered to investigate both Lamarckian and Darwinian concepts of evolution (let alone all the other theories that were around as alternatives). I assume that a certain percentage of researchers (for it is not clear to me if 50% or 1%) would have had to engage in confirming Lamarckism explanations of evolution. Naturally, investigations in Darwinian concepts of evolution would have been slowed down to a certain rate depending on how many researchers were drawn from it.

Formulated in a very crude way, the integration of the laws of heredity of Mendel with Darwinian evolution as well as the discovery of the material basis for this - the molecular structure of the DNA double helix - contributed tremendously to modern genetics and the development of molecular biological methods. Through the development of those methods (and also instruments) deep insights could be obtained that allowed the discovery of epigenetic as well as other instances of the inheritance of acquired traits, discussed in chapter 4.1.

Thus, through embracing Darwinian evolution amongst other factors it was possible to re-discover Lamarckian evolution. If less researchers would have worked on this, because they had or wanted to focus on proving Lamarckism all along, this process would have been slowed down. Therefore, it is crucial to assess whether Lamarckism could have gotten so far all on its own.

To my understanding, the integration of Lamarckism with Mendel's law would not have provided the molecular basis for the inheritance of acquired traits and neither had the

discovery of the structure of the DNA double helix: Importantly, although phenomena such as DNA methylation that are nowadays interpreted to be epigenetic, were discovered along with investigations into the structure of the DNA, they were for a long time not associated with epigenetic inheritance. Unaware of the machinery that causes such modifications and the stimuli that launch this machinery these phenomena would not have provided any explanation how acquired traits could cause changes to DNA and thus would not have supported Lamarckian inheritance.

Thus I see no way, in which a constant pluralist embrace of Lamarckism could have propelled the discovery of epigenetics. I am aware that I leave out the benefits of interaction such as concurrence. Still, I am of the conviction that assigning a certain percentage of people to Lamarckism instead of Darwinism would have not propelled the discovery of epigenetic phenomena. In contrast, I believe that it would have rather slowed down all the discoveries that were necessary to be made until the identification of such phenomena was possible.

Thus, this case study provides no corroboration for active epistemic normative pluralism and only little for preservative pluralism.

Re-assessing Chang: How Much Pluralism Can We *Really* Afford?

In this last section I want to ask the question, that seems most pressing to me after having concluded, that the case of Lamarckism does in my opinion not provide a case for pluralism. Importantly, I do think that there is something worthwhile about Chang's pluralism and that we might do something more than accepting it in the case it shows up by itself, as for instance the pluralist stance would suggest. Thus, I will try to identify the main problems that arise when intending to actively "plant 100 flowers" and consider possible solutions or at least moderation for these problems.

The main conceptual problem about Chang's agenda proposed in ***Is Water H2O?*** is a too optimistic and thus misconceived interpretation of the scientific society concerning his version of pluralism. Thus he fails identifying substantial problems that would arise when pluralist practice gets established:

I think that there are two main issues concerning active normative epistemic pluralism, which can be again subdivided in a lot of individual sub-problems: The first group of problems concerns a case, where pluralism becomes actually mandatory through science policy (I believe this is Chang's ideal scenario). The second group of problems concerns a milder case of pluralism, in which certain systems of practice self-assemble through the work of

individuals, where Chang's pluralism would demand to integrate those systems of practice in scientific discussion.

First I will try to discuss problems concerning mandatory pluralism. If active normative epistemic pluralism becomes established, there is of course an ideal scenario: If scientists, the public and politics (i.e. well-ordered science) decide in accordance (or at least by a convincing majority vote) that active, epistemic, normative pluralism is the first and foremost epistemic desideratum, then be it – I would have no problem with that. If this is the case, researchers would sacrifice their own research aims for a higher goal – namely seizing all the benefits of toleration and interaction that arise through pluralism. This would also have major impact on both school and university education. Unfortunately, Chang omits the discussion of this consequence except of when talking about creationism, where he seems to suggest that different systems of practice should be taught to kids to let them decide on their own which is most convincing. In spite of that, I will leave out discussions about possible impacts on education, as I think that this is beyond the scope of this master thesis.

One needs to discern cases, were scientific debate seems to be settled and one system of practice is established and current debates in the sciences, where a paradigm needs yet to be found. I believe that arguing for pluralism in the latter is not half as problematic as in the first case, as hedging one's bets seems to be the smart thing to do in such cases. Also, in chapter 3.2 I provided the equation Philip Kitcher proposed for assessing if plurality is helpful in such cases. Thus I will focus on cases, where debates seem to be settled, meaning where knowledge is black-boxed and thus not contested any more.

In such cases, a certain percentage of the available researchers would be assigned to a plurality of theories which provide different accounts and explanations for a certain phenomenon. The first question that arises is how we decide on such systems of practice. Of course, there exist certain theories around, most of which are normally rendered pseudo-scientific, to which scientists could be assigned to. If Chang speaks of actively "planting" the seeds of pluralism I wonder, from where we should draw this prescriptive plurality of theories. One could of course assign a council of expert pluralists, which deduces several possible hypotheses for a certain phenomenon and then assigns researchers for investigating into them. As a next step, someone would have to decide, how many researchers should focus on which systems of practice. Let us assume that our council of expert pluralists is also in charge of that. There are several possibilities for doing so: For example, each system of practice could be viewed as equally promising, and thus researchers and resources would be

shared equally among them. Alternatively, one or a few systems of practice, that seem somehow superior – for example, in the last decade, they made the most ground-breaking discoveries – get 50% of all funding, whereas the rest is shared equally among the remainder of systems of practice. The most modest version of this practice would be – as also Chang tentatively suggests – that 1% of all scientists and resources gets distributed to all alternative systems of practice, whereas the leading system of practice gets the lion's share.

Although many of these propositions for the administration of pluralism seem questionable, the most problematic issue is the willingness of scientist to accord to this system of mandatory pluralism. I seriously doubt that any scientist would want to engage with corroborating creationism or arguing in favor of homeopathy, while her colleagues do the research she believes in. On the contrary, I am of the opinion, that this would make many researchers turn away from and lose their faith in science. A resulting lack of competent researchers could lead to hiring less accredited individuals that are more eager to corroborate creationism or homeopathy and thus would reshape and most likely deteriorate scientific method and objectivity. Thus with the claim that 100 different flowers should be planted, the major problem lies in the availability of the seeds.

We might be more open for special scenarios, where such mandatory pluralism seems not all together wrong. There are certain fields in research, where although a lot of knowledge is black boxed and only one system of practice is established, substantial progress has not been made for long. For example, although the understanding for all the different protein signaling cascades, micro RNAs and environmental cues causing cancer rises every day, cancer remains the most taunting and most wide-spread disease. Also, treatment with antibiotics – although almost worldwide established – proves to be extremely challenging due to bacterial resistances in the last decades. Another example, where I do not have much expertise in, might be string theory.

Thus, a council of pluralist experts could be inaugurated to assess science for such special cases, where disassembling the current paradigm and trying a plurality of approaches might have great values. Even such a tempered and situational approach of pluralism might prove problematic for certain researchers, but still it seems more promising. Of course the communication of science policy would need to be optimized and a consensus within the scientific community would need to be established, that acknowledges that there are in fact such cases where pluralism would be desirable. Also, it would need to be ensured, that assigning scientists to more obscure theories does not harm their track record. Additionally,

trust in such a pluralist council needs to be carefully established. One possibility would be to initialize a rotating membership for e.g. 3 years that ensures both best practice as well as trust in the members, as they would be recruited from as many different institutions as possible. Thus I believe that approaching pluralism in science locally, based on problems that are easily realized by most scientists would provide a fruitful niche for Chang's pluralism.

The second, more moderate scenario that needs to be discussed is a prescriptive openness to researchers, that perform their research in different systems of practice and which provide for themselves. Such openness would for example consist in inviting such people to important conferences and engaging in serious discussion with them. Also, providing them some space in scientific journals, in order to make their ideas accessible to the scientific community would be crucial. Additionally, lab space could be provided for collaborations and integration could be tried wherever possible.

Importantly, the willingness of scientist to cooperate in such scenarios needs to be seriously questioned. Given the fact, that there are vigorous battles of immunologists against the anti-vaccination movement or of doctors strongly opposing homoeopathy without borders, giving the actors of such causes even more space might seem too dangerous for most scientists. To provide an example for enormous publicity of questionable theory, I will draw from the debate concerning vaccination:

There was one paper published by Andrew Wakefield in 1998 that reported that the MMR-vaccine is associated with the occurrence of so-called "regressive autism", which caused thousands of parents not to vaccinate their kids against MMR. After the publication of uncountable papers disproving Wakefield's thesis and the retraction of his paper, MMR-vaccination of children is still low and causes preventable deaths and serious disease such as encephalitis in western countries that would have full accessibility to vaccination. Thus, it is more than understandable that granting people making such claims even more publicity and showing some kind of support seems immediately dangerous to researchers.

Another reason for the refusal of committing to such prescriptive openness might be simple lack of interest. There are a lot of problems that immediately concern the individual researcher and thus she might not spent her time in discussions with alleged researchers making claims that are lacking all the standards she requires for taking it seriously and thus is simply not interested in any openness.

Besides those two points, I believe that scientists hold the demarcation of science from pseudo-science as very important and thus would be very unwilling to blur the established border.

Similarly to the previous discussion about mandatory pluralism, I believe that there could be some scenarios, where it would be affordable and also beneficial for letting some more systems of practice sit on the table, if certain scientific areas appear to be not as promising as previously believed. As stated before, in such cases I would suggest an increased amount of epistemic humility, by considering other systems of practice.

As a last point before concluding this section I want to stress the point that I strongly believe that certain positions do not need to be considered by any pluralist approaches. Concerning this, my opinion is opposed to Chang's, as for example, he proposes, that e.g. creationists should also be included in the pluralist debate as there might also arise certain benefits of toleration and interaction. In Chang's view, someone who claims the exclusive truth of Neo-Darwinian evolutionary theory is just as immature as someone claiming the truth of Intelligent Design or Creationism (Chang, 2012, 262). This is certainly not a position that is generally embraced by scientific pluralists. For instance, Dupré's notion of this case is substantially different. He argues, that it is vitally important to be able to maintain criteria for assessing scientific claims, thus he states that it would strike him, "for example, as a fatal flaw in my position if it led to the conclusion that nothing could be said in explanation of the epistemic superiority of the theory of evolution over the apparently competing claims of creationists" (Dupré, 1993, 242).

Considering the historical example I discussed in chapter 4.1 and the fact, that I declared that I believe that Neo-Lamarckian interpretations have their justification I need a criterion for supporting both Lamarckism and Darwinism rejecting Creationism in evolutionary research. For doing so, I will again use Kitcher's scheme of the three types of values: broad, cognitive and probative. Each set of values derives from the former one, and together they define the different types of incommensurability between different systems of practice.

Adopting those schemes, which I canvassed in section 3.2 one could argue, that Lamarckism and Darwinism share the same cognitive scheme of values, as they agree on what kind of knowledge they want to gain but substantially disagree about the set of problems to tackle and also the answers they want to see for their problems. On the other hand, both Lamarckism and Darwinism disagree already on the first level of values with Creationism, as

this position holds it most appropriate on the broad level of values to emphasize the importance of the belief and the inerrancy of the literal interpretation of the scriptures. Thus I argue, that if such a broad scheme of value can be identified, the respective „scientific“ theory does not need to be considered when arguing for active normative epistemic pluralism. For creationism especially psychological self-deceiving can be attested, as their cognitive goals are not cognitive, as discussed in chapter 3.2.

I think that Chang misidentifies creationism for what it is. If he argues for *scientific* pluralism then he would have to seriously modify the conception of science in order to let creationism sit at his pluralist table. If the broad scheme of values of creationism focuses on corroborating the literal reading of the bible and thus dismissing all evidence against it, this does not match our common conception of science.

Conclusively, I believe that some of Chang’s pluralism can be imported into scientific practice and policy. As shown above, trust in it needs to be carefully established, and situations where it could be applied need to be diligently proven. Also studies of cases where such a pluralist principle was applied need to be carefully assessed for the beneficial potential they had. Thus I am strongly against a full rescue of Changian pluralism but believe that some of his prescriptiveness can and should be applied.

5 Conclusion

In my master thesis I had several goals in analyzing and testing the pluralism as argued by Hasok Chang's ***Is Water H_2O?***. In this concluding section I will summarize my discussions and add some new thoughts and suggest some possible future perspectives.

In a rather conceptual first part of my master thesis I tried to elaborate Chang's position on pluralism and contrast it with the notions of Dupré as well as of Kellert, Longino and Waters. By assessing those positions based on their commitments towards realism, monism and relativism as well as on their commitment to action I tried to show, that a classification scheme of pluralism based on the respective "modesty" or "radicalness" of each individual position has several shortcomings. Therefore, I introduced a scheme based on a commitment to pluralist action. Such classificatory schemes need further elaboration. Accordingly, I intend to offer a more refined theoretical framework in my PhD thesis that also aims at taking into account the positions' historical backgrounds, metaphysical versus empirical accounts as well as predictions concerning scientific monism.

Next, I offered a survey of critique leveled against Chang that challenged his historical account on the Chemical Revolution. In this vein, I introduced a discussion on the concepts of scientific revolutions as well as the "experimenters' regress" and other "broad" reasons for the justification of theory choice. Further, I offered Kitcher's concept of well-ordered science as another possibility for the democratization of science which does not necessarily imply pluralism and his discussion of scenarios, where pluralism is especially beneficial. Insights gained through those discussions, became salient in the last chapter, which aimed at testing another "revolution" for its potential as a pluralist's show-case example. I am aware of the fact that the choice of critique leveled against Chang was rather selective as it was mostly motivated by the fact that Chang responded to the two papers discussed. For future research aims, a wider range of historically as well as philosophically leveled critique needs to be taken into account and also concepts, such as "scientific revolutions" will have to be scrutinized in more depth.

My last and most important aim was to "test" Chang's pluralism on a case that seemed to be an *a priori* show-case example for conservative as well as active pluralism. Thus, I re-assessed the case of Lamarck's rejected evolutionary theory in comparison to Darwin's theories, which had almost immediate success. It was my aim to show, that there were reasons – which I denominated "narrow" and "broad" - which justified the abandoning of Lamarck's thesis until very recently. These notions were contrasted by the fact, that the discovery of

epigenetic phenomena that suggested instances of Lamarckian inheritance provided some corroboration for Chang's claim, that preservative pluralism should be applied, as certain important theoretical concepts might be lost, that will once again gain importance.

In spite of that I wanted to prove the point that this example makes not necessarily a case for pluralism. Thus I argued that, on the one hand, at several time points throughout the 19th and 20th century, researchers had very good both "broad" and "narrow" reasons for rejecting Lamarckism. On the other hand, I hypothesized, that Lamarckian theories would have faced several difficulties when tried to be integrated with other very important concept of late 19th and early 20th century biology and thus might have halted scientific progress.

In a concluding section I discussed the limits and possible applications for Chang's pluralism by suggesting, that there are special scenarios, such as scientific revolutions or cases of stagnancy in certain fields of science, that justify scientific pluralism in its strong form as argued by Chang. Additionally, I argued, that theories of bygone scientific paradigms should be kept accessible to both historians and philosophers of science as well as scientists, so that they are kept at hand if the theories of a current research paradigm fail at explaining certain phenomena.

In summary, I believe that Chang's commitment to action is both very attractive and very distinctive concerning his position. However, this commitment needs to be tempered in order to appear workable to both scientists as well as historians and philosophers of science. Thus, the respective niches where his prescriptive pluralism is applicable need to be both elaborated in detail as well as diligently communicated to all respective parties involved.

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7 Appendix

Abstract (English)

This master thesis provides a critical analysis of scientific pluralism, as argued by Hasok Chang. It can be devided in two different parts: In a rather theoretical and conceptual part I contrast Chang's notion on pluralism with two other recent positions: the metaphysical pluralism of John Dupré and the "pluralist stance" as argued by Stephen Kellert, Helen Longino and Kenneth Waters. It is my goal to show that the common classification of pluralist stances that is based on the relative "modesty" and "radicalness" of the respective position, fails at capturing essential parts of pluralist notions. Therefore it is my intention to discuss other classifications, based on inclinations towards realism, monism and relativism and additionally introduce a new way of classifying pluralist positions, namely their relative demands for pluralist action.

In addition, I provide a survey of critique on Chang leveled against his historical reconstruction of the chemical revolution and also discuss philosophical and conceptual limitations of his scientific pluralism.

In an empirical part, I provide a case-study on a historical incident that could serve as another argument for Chang's pluralism. I will discuss the rejection of Lamarckism through Darwinism and its late "resurrection" through the discovery of epigenetic phenomena. On the one hand, I will provide the reasons for the rejection of Lamarck's thesis on species evolution at various timepoints and will close with an assessment of contemporary neo-lamarckist and neo-darwinist positions. On the other hand, I will test this historical example for its usability as evidence for scientific pluralism. In this vein, it is my goal to show that despite the fact, that Lamarck's theories seem to have regained some of their justification, scholars were more than justified until very recently to reject his thesis. Thus I will conclude this section by showing that the case of Lamarck does not serve as grounds for arguing for scientific pluralism.

Besides the analysis of this historical case study I will discuss, whether there are certain "niches" for "Changian" pluralism that could prove its utility in contemporary science. Thus I will conclude that a very tempered version of Chang's pluralism might be rather beneficial for science.

Abstract (Deutsch)

Meine Masterarbeit befasst sich mit einer kritischen Aufarbeitung der Ansprüche an Wissenschaftspluralismus, wie er besonders von Hasok Chang vertreten wird. Grob lässt sich die Arbeit in zwei Teile gliedern: in einem theoretischen Teil wird der Pluralismus Changs mit zwei weiteren Positionen verglichen: dem metaphysischen Pluralismus von John Dupré und dem „pluralist stance" von Stephen Kellert, Helen Longino und Kenneth Waters. Es ist mein Ziel zu zeigen, dass die gängige Art von Einteilung in „radikale" und „bescheidene" Positionen das jeweilige pluralistische „Programm" nicht auch nur ansatzweise wiedergeben kann. Deshalb ist es mein Ziel, neben Klassifikationen basierend auf der Beziehung zu Realismus, Monismus und Relativismus die Inklination zu pluralistischer Praxis als neues Kriterium einzuführen und zu testen.

Anschließend werden historische Einwände gegen Changs Rekonstruktion der chemischen Revolution, sowie philosophische Einwände gegen die Durchführbarkeit von Wissenschaftspluralismus erörtert.

In einem „empirischen" Teil wird versucht, einen weiteren Fall, der Changs Argumentation für Pluralismus dienlich sein könnte, nämlich die Verdrängung des Lamarckismus durch Darwinismus und dessen späte „Auferstehung" durch Phänomene der Epigenetik zu rekonstruieren. Zum einen werden die historischen Umstände der Ablehnung der Lamarck'schen Thesen zu diversen Zeitpunkten erörtert und mit einer Diskussion der zeitgenössischen neolamarckistischen sowie neodarwinistischen Positionen geschlossen. Es ist mein Ziel zu zeigen, dass obgleich die Epigenetik den Lamarck'schen Theorien einiges an Berechtigung zurückgibt, Forscher bis vor kurzem gute Gründe hatten einen jeglichen lamarck'schen Einfluss abzulehnen. Daher schließe ich, dass dieses Fallbeispiel kein Argument für Wissenschaftspluralismus liefert.

Neben dieser historischen Analyse werde ich diskutieren, ob es gewisse „Nischen" für Chang's Pluralismus in der gegenwärtigen Wissenschaft gibt. Daraufhin werde ich abschließend versuchen zu zeigen, dass eine stark gemäßigte Version von Changs Pluralism in gewissen Teilbereichen der Wissenschaft von großem Vorteil sein könnte.

Curriculum Vitae

Education

2003-2005 GRG Franklinstraße 21
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