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Do Kuhnian Revolutions Suit Biology?

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INTRODUCTION

Thomas S. Kuhn's aim was to show how adopting a new historiographical approach would reveal a structure of scientific progress that was not cumulative but, from the perspective of hindsight, a series of rather sudden shifts in beliefs and practices by the scientific community. By implementing a historiography that emphasizes sociological and cultural aspects, as well as analyzing original texts in the context of their own time, it would become apparent that most accounts of science history were hindsight revisions made to look neatly cumulative and progressive when in fact they were not. *The Structure of Scientific Revolutions* (1970) serves as the main work of literature in support of my thesis.

The aim of this thesis is to compare Kuhn's historiographical framework that he implemented on the history of physics with the history of biology to discover if it is meaningful to discuss biology from the perspective of a Kuhnian revolutionary historiography. Along with this broader aim, there is a secondary and more concrete reason to investigate if there is any merit in discussing Theodosius Dobzhansky's *Genetics and the Origins of Species* (1937) as a revolutionary text in the sense that it is similar to works like Charles Lyell’s *Principles of Geology* (1830), Isaac Newton’s *Mathematical Principles of Natural Philosophy* (1867) and Antoine Lavoisier’s *Elementary Treatise of Chemistry* (1789) which lay the foundations to modern science in their respective fields. This inquiry will also address the status of Charles Darwin's *On the Origin of Species* (1859) as a revolutionary text as the significance of his work has been the center of much hype and historically questionable claims. By the end of this paper I wish to answer the question of whether a Kuhnian historiography is interesting to implement to biology. Is it able to shed light to new inquiries and interesting nuances about biology and does it help to clarify the still open question of a Darwinian revolution?

In the first chapter I will clarify the main aspects of Kuhn's philosophy: paradigms and incommensurability and show why revolutions cannot be understood without semantic incommensurability in respect to theory change. In the second chapter I will investigate whether the previously discussed notions are applicable to exclusively biological theories and concepts. I am going to use three comparisons: is there a visible shift from proto science to institutionalized science with a paradigm; does biology exhibit the elements that belong to a paradigm; does biology exhibit semantic incommensurability in its key concept of species.

1. THOMAS KUHN ON THE NATURE OF SCIENTIFIC REVOLUTIONS

In this chapter I will explain two key concepts in Thomas Kuhn's philosophy of science: paradigms and semantic incommensurability. As those concepts changed through time, I will select the meanings I find most useful and unequivocal to serve as the basis for my analysis of biology. At the end of this chapter, I will have clarified which parts of Kuhn's philosophy I find useful in analyzing biology and answered the question why semantic incommensurability is the crucial component to understanding a Kuhnian revolution in science.

1.1. What is a Paradigm?

The term held two meanings in the 1962 release of *The Structure of Scientific Revolutions*. This subchapter will discuss the modification published in the *Postscript* in 1970 and also its difficulties as a circular concept. “Paradigm” is closely related to “normal science” which I will use in a way that refers to the process of scientists operating in a paradigm – science as it happens. I will not be focusing on the details of normal science, rather my emphasis is on the structure of the paradigm itself and the attributes that accompany shifts in paradigms.

1.1.1. Was there Always a Paradigm in Science?

In Kuhn's view, there can be a scientist without a paradigm: in fact, there can be something that could under sufficient conditions be called “science without a paradigm”, thus it is not a necessary condition of science. The important distinction to make is that according to Kuhn, the period before normal science does not feature any distinct paradigm-making work, but rather lots of metaphysical schools of thought that each tackle a problem dialectically to the best of their ability. These schools of thought consisted rightly of what we call “scientists” because any definition of “scientist” that excludes even the prominent members of these early schools of thought will also exclude the modern day scientist. Their work, however, was something less than science. Every scientist had the liberty to build a whole new set of methods and beliefs. There was no common ground for a unified method of conducting experiments. In fact, the choice to even take observations and experiments into account was a free choice. The typical formula of the situation is as follows: different groups of scientists compete for the most complete explanation of nature's phenomena. Their theories bare a family resemblance at best, but they usually share some sort of metaphysical framework which gives them room to choose their own points of interest and practice of methods. At some point, one of these schools of thought becomes successful in having the capacity to explain more natural phenomena than the others and draws followers that use those principles and methods for further scientific pursuits. That is when a paradigm forms. (Kuhn 1970: 10-15)

Without a paradigm, facts seem equal and fact gathering is rather random. There comes a point where the pool of scattered gathered data will facilitate the emergence of a paradigm. An example of random fact gathering would be the numerous descriptions written about flora and fauna without the backbone of evolutionary biology to look at the findings in a phylogenetic perspective. In contrast to random fact gathering, normal science displays guided pursuits and experiments. Kuhn gives an example of some scientists believing electricity to be a fluid. This belief directed them to experiments where they tried to bottle it. In this way a sort of common ground is established, thus displaying the role of collective work and background beliefs that guide experimentation. It also gives the scientist new energy and perseverance to take on more time demanding and difficult tasks. Natural sciences usually started their institutional era with a single paradigm arising from one of the classic texts like Newton's *Principia* and Franklin's *Electricity*. That text also served to be the revolutionary jump from a pre-paradigmatic science to a revolutionary science. This process had two distinct results. First one was the quiet disappearance of older competing schools of thought. The members of those communities either integrated their work into the early paradigm driven science, or they were neglected. The second consequence of institutionalization was that scientific work became unintelligible to the common educated person. A person in the 18th century who had the access to be tutored in the Seven Liberal Arts in addition to Greek and Latin found himself to be lacking of sufficient knowledge in mathematics and current naturalism. Before institutionalization, written work was often presented in a way that contemporary educated men would have been able to understand and follow. After that, the regular form of writing transformed from the system-building book to the short article which held the premise that whoever reads it already knows the central ideas and problems of that certain field in which the article was produced. (15-22)

1.1.2. The 1970 Revision of "Paradigm"

After the first publication, it became apparent that the term "paradigm" was used in two different ways: broadly as a set of theories, beliefs, values and methods; and specifically as the puzzle-solving element of normal science which is a consequence of a theory. More specifically, this narrower meaning of paradigm denotes the way in which science-making is governed by experiments and their results rather than rules. In response to criticism from his peers, he revised "paradigm" and called it the "disciplinary matrix" of science. These guiding experiments, initially also called a paradigm, he later called "exemplars" - one of the four elements in the matrix. Another problem remained: it became apparent that his use of "scientific community" and "paradigm" seemed circular which was an obvious target for criticism. It seemed like scientific communities were defined by sharing a matrix, whereas the contents of the matrix were defined by the scientific community. In this section I will discuss the revised definition of a paradigm and the circularity of the community-matrix relation.

Let us break down the disciplinary matrix. It has four elements: laws, exemplars, metaphysics and values. First, it consists of statements like $F=ma$, $\Delta G=\Delta H-T\Delta S$, "energy transfers from one state to another". These can be understood as laws, axioms and definitions. Kuhn said that definitions and laws had a fundamental difference: laws could be revised and modified bit by bit, definitions as tautologies could not. He suspected that all revolutions displayed a process whereby in order to implement a new law the definitions of the constituents of those laws had to be revised. (183) He took the statement about laws and definitions as common knowledge by the way he presented it. When something is added to a definition then with this act, the definition itself changes - that is self-evident. In the case of scientific laws, it is not that obvious how he came to the conclusion that they could be modified bit by bit still retaining their meaning. After all, a scientific law is an abstraction of some causal relation between natural phenomena (also arguably a mathematical truth not tied to the empirical world) and when one changes the constituents of that law, it no longer portrays the same thing that it did before. I believe he meant that by convention laws can be extended by giving them special case extensions. One example would be that the Ohm's law does not apply on heated rods inside a light bulb, or the Mendelian laws do not apply in meiosis where selection has favored other means of genetic division. I also agree that the elements in the form of a law define themselves by being in the form of a law. This is a psychological statement: when a law is formulated, it requires empirical proving but as time goes by, young scientists come to define the constituents of a law with the help of the law. I believe this to be one of the most insightful observations Kuhn made about science. I also believe it to be the reason some philosophers have rejected that F=ma is anything more than analytical (Brigandt, Love 2014). This brings to the point how laws as part of the matrix are susceptible to semantic incommensurability. When the acceptance of a law requires one to accept a different meaning about its elements than was the previous convention, then it is a perfect example of how some forms of old and new laws do not have common measure. The form of a law stays the same but parts of it have been renewed. This is the reason why incommensurability often stays hidden. If whole sets of elements in the disciplinary matrix are switched, then old and new matrixes are incommensurable.

The second element of the disciplinary matrix is its metaphysical aspect (Kuhn 1970: 184). This includes notions that are not a direct result of a theory but may or may not guide scientists. An example would be the numerous interpretations of the world in theoretical physics which postulates phenomena that could not be tested in any way possible. Belief in them can guide a scientist whose field is theoretical physics but for a person studying liquids it makes no difference whether there are ten or eleven dimensions. So belief in metaphysical concepts that can be abstracted from contemporary knowledge is not mandatory for a scientist who does no research these concepts. The metaphysical aspect of a paradigm rather tends to express outside of science and attract ideologically minded thinkers and philosophers. Another example is the question of teleology in biology. Even if Daniel Dennet claims for certain that evolution is a Turing machine, non-teleological and not guided by ultimate causal meaning (Dennet 2012), in practice, unless the existence of a creator or teleology of organisms becomes the center of research, like in the case of proving intelligent design, a biologist can freely believe that whatever she is researching was set in motion by God and it will not affect the work in any way.

A third element of the matrix is a collection of values. Kuhn stressed that values as major contributors to paradigm-change did not render science a subjective irrational enterprise that had little to do with careful reasoning or fact. Rather, values were subject to and resulted from careful reasoning and consideration. That science should be conservative, that theories should be simple and clear, that science should not/should be useful to society are all values. (Kuhn 1970: 186) In physics, it is far less evident that values are major contributors to what should be an object of research. It is most evident in sociology and also easily noticeable in psychology and biology. By being aware of some new part of nature as worthy of pursuit, the results of that inquiry could trigger a revolution. Mendelian laws of heritability could not have succeeded without the Darwinian paradigm. If Mendel had not valued a quiet life and methodological testing to find out whether there was a reason for the appearance of different colored petals of pea plants throughout generations, then his landmark work on genetics would not have come about. It was again a matter of values that at first the scientific community did not take him seriously and that only afterwards, in the light of Darwin's works, he was rediscovered by Hugo de Vries and Carl Correns who thought it useful to study the old works of forgotten scientists. In contrast, there are some branches of science, like computer science, where reading old works written more than a decade ago hold no value.

The fourth and the last element in the matrix is the collection of exemplars. These are the concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science textbooks. This element of the disciplinary matrix is especially important in physics as it determines the “fine-structure” of the scientific community. Exemplars are the problems to which equations are applied to reach accepted solutions. As the education of a scientist progresses, her arsenal of equations and example problems grows and the use of these is different in sub-disciplines. (187) In the book, this was the stricter meaning of the paradigm. Science does not share rules, instead it shares exemplars. Rules stem from exemplars but exemplars themselves can be implemented without rules. Kuhn argues that a case of family resemblance restricts scientists to exemplar-specific research. One does not need to know what exactly gave the accepted methods and objects of research their status, the methods and objects need only to follow convention and tradition. He provides two examples: first is the fact that it is very difficult for researchers to discover the rules that govern science. Secondly, scientists do not learn abstractions, theories and laws as such for themselves. Instead, these are learned through application in an embedded manner. (43-50)

The revision to define the disciplinary matrix made Kuhn's idea of the paradigm much clearer and answered the question of how paradigms can be bigger and smaller in scope. He was using the strict meaning of the paradigm which is that exemplars can be bigger and smaller.

Through specialization, it is possible that a smaller change in exemplars can leave other fields unaffected. An equation in some field that is added to its problem solving methods or is neglected, may be irrelevant to other fields who would use the equation in a different manner. The same exemplars can carry different meaning to different sub-disciplines. It also points to the conclusion that smaller-scale paradigm change signifies in most part changes in exemplars and perhaps in values and metaphysical commitments. A large scale paradigm shift means instead a shift in the whole disciplinary matrix where the key concepts of a theory become incommensurable with respect to the old theory, and forces a revision in metaphysics, values and exemplars.

A paradigm, synonymous with disciplinary matrix, is a set of laws, exemplars, values and metaphysics that the scientific community shares. A scientific community is a set of scientists who have decided to study the world under a paradigm. Is the definition's circularity a problem? It was and is a problem to those philosophers who were/are influenced by Positivist approaches to defining concepts in a manner that should exclude logical ambiguities. Kuhn was a sociologically minded historian/philosopher and his views that one can't characterize the logically inconsistent and fluid phenomena of human enterprise by postulating logically neat concepts is very evident in his critique of Popper and the Positivists and their historical reductionist views on laws and theory. By attempting to characterize science as it was, not how it should ideally be, he applied his concepts from a much more subjective standpoint: science cannot be defined by necessary and sufficient conditions, science is what people think science is and the most authority over what science is was owned by scientists. By abandoning static definitions of scientists and science and instead approaching things historically it seems that strict formalizations are rendered incomplete and too strict to adequately describe the status quo. For this reason, the appeal to circularity is exactly what one would expect from philosophers whose philosophy Kuhn tried to distance himself from in favor of a different approach. This is the first reason why I do not take the accusation of circularity very seriously.

The second reason why I do not take the accusation seriously is closely related to the first reason but with emphasis on the idea that when a philosopher approaches to characterize a phenomenon from a social standpoint, the characterization of the phenomenon is tied to human cognition. Cognition has the inescapable property of circularity embedded in it: the world is something that causes us to sense it and our senses shape the world for us.

Institutions are defined by their members and sets of members are defined by institutional membership, very robustly speaking. Institutional membership surely does not characterize the members fully; they are a part of different sets: men, women, parents, Europeans, Asians etc. Institutions are upheld by their members and the content of them is subject to change if the members decide so. Members are in no way fixed to the institutions, they may leave and thus leave the set with the help of which them being in that institution is being defined. Science as an institution, upheld by verbal convention and physical buildings like colleges and private corporations, and therefore moldable and definable by its members that are not fixed to that institution, is what gives this position its legitimacy.

Those, who still have concerns with the circularity, I propose to define the relation between the paradigm and the community in a one way relation. If there is a paradigm, then there must be scientists upholding it. My premise is that I grant existence to human made theoretical structures through their use. If there are scientists then it does not follow that there must be a paradigm. Scientists are not dependent on the disciplinary matrix, for Kuhn also permits scientists to exist without paradigms. Even if scientists gather inside a community sharing a paradigm, the community is not dependent on the disciplinary matrix for they may change their beliefs and conventions in their entirety and still call it science and we would still have the liberty of calling the whole enterprise of what they do a paradigm. The fact that a group of scientists are working to uncover aspects of the world under a disciplinary matrix, is contingent and causally linked to numerous social and economic factors. It is not the case that a disciplinary matrix ushers scientists into existence.

As the disciplinary matrix and the community that upholds it are both fluent, a simple circular statement “a paradigm is defined by its community and the community is defined by its paradigm” is very misleading and unnecessary for it lacks dimension and pragmatic value and ignores the *de facto* circular and untidy naming by convention. Once again, the aim is to describe what we call science, not to pose normative constrictions on it. Now that I have cleared the basic concepts, from this point onward I will use “matrix” synonymously with “paradigm”.

1.1.3. The Connection of Normal Science and a Paradigm: Why Revolutions become inevitable

An overview of normal science is necessary to later introduce the discussion on incommensurability. In this section I will summarize Kuhn's thoughts on normal science and revolutions which already touch on the subject of theory accumulation and reductionism.

The term “normal science” means the practice of science that is firmly based on an acceptable foundation of observations and theories. It is not only descriptive, containing the classic texts and an account of discoveries from the past, but also normative, for it dictates the nature of scientific problems and the appropriate questions and puzzles to be answered and solved. The aim of normal science is to enforce the accepted disciplinary matrix with special emphasis on exemplars by further characterization and specification of already known phenomena. This allows scientists to study certain topics to the absolute detail, which would otherwise be rendered much more difficult due to the sophisticated nature of the problems at hand. One of the aims of normal science is to take a class of important facts provided by the paradigm and perfect the knowledge of the properties of those facts. (30-34)

Scientific revolutions are “taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one” (92). The term “revolution” is not randomly selected by Kuhn for shock value. In the wake of political revolutions, people lose confidence in institutions and some members of society start to behave in eccentric ways. Soon those eccentrics will propose a positive program which aims to provide solutions to a widening crisis. At that moment society is broken into groups which compete for establishing hegemony. The parallels with what has been previously written are inescapable. Much like political revolutions happen extra-politically, outside institutions, the choice for paradigm happens outside the former paradigm and is therefore also a community issue. The circularity of everyone arguing in their own paradigm is not a problem. They can adequately characterize the way the world would look and how science is to be conducted. The choice between paradigms cannot be explained through logic or theory-experimentation analysis. The choice comes from subjective and pragmatic considerations. (91-94)

A theory can develop from three classes of phenomena: phenomena already covered by a paradigm, phenomena that require paradigm extension and anomalous phenomena. The last one is the source of a new theory. A new theory gives predictions that are logically incompatible with the predictions of the older theory. It means that a new theory should be different from the old one in such a way that it is in no way reducible to the old one. The counter argument usually goes in some form of the following: older theory A and newer theory B are not contradictory because both are still used in their respective fields. We now know that A is a special case of B. No theory can contradict with its special case. In addition, sometimes scientists make extraordinary claims about the world, leaning on a particular older theory. This may show the older theory as incompatible with the new one, when in reality, the older theory never had the explanatory scope for the claims that had been made to have proper cause or foundation. Kuhn counters this by stating that, if this sort of positivist ideas of science would be true, there would be no new discovery or, more ironically, science would be reduced to a kind of pre-paradigmatic state where separate scientists practice science, but the overall outcome would not be science. If scientists really held to the strict logical scope of their theories there would be no accidents which lead to new discoveries. (94-101)

As for the arguments considering derivability, Kuhn states that in order for one theory to be derived from the other there should be no need to redefine concepts which point to the same phenomena in both theories. If one has to do that, then the two theories are not linked in that manner. This applies to the Newtonian-Einsteinian shift. In order to say that Newtonian physics is a special case of Einsteinian physics one has to ignore the fact that besides redefining narrower concepts one also has to redefine the understanding of the fundamental structure of elements from which the universe is made of. The essence of a scientific revolution is the need to redefine the conceptual framework through which scientists work. The reconceptualization is always something that can be undertaken in hindsight with the guidance of the current theory. (101-103)

1.2. Incommensurability as the Heart of Paradigm Change

In this subchapter I will show how incommensurability, tightly woven with the epistemological argument, is at the heart of Kuhn's theory on paradigm change. I will go over the three stages of incommensurability that Kuhn developed over the years. It is the acquiring of a different world view brought on by a gestalt switch that gives evidence of incommensurability between old and new paradigms. Although I will be focusing on semantic incommensurability, the epistemological aspect of a paradigm change is very important as the way language is used and perceived is part of our cognition and therefore applies to the epistemological claims that I will introduce in the next section.

1.2.1. The Epistemology of Scientific Revolutions

A considerable portion of Kuhn's account is tied to the epistemology of science. He wrote about the difference between seeing the world through the old and the new paradigm as seeing different worlds, although the world itself remains the same. He was better able to articulate it in a way that the world consisted of objects of our sensations. Neurologically speaking, we have evidential reason to believe that people have different sensations from the same objects and the same objects cause different sensations in people. We also have strong experimental reason to believe that education and more broadly culture is an important factor how a person has sensations about some object. Since sense data is all we have to go on, we define our world completely in the realm of it. If sense data differs drastically from person to person, it is quite safe to admit that the expression “living in different worlds” no longer sounds as esoteric as it might have earlier without proper context. (Kuhn 1970: 192-193)

The central argument he gave was that when we sense something, it already is an interpretive process, as opposed to the idea that our sensing is pure from any higher order brain activity. I would like to present a scientific example from contemporary neurology: up to this day, scientists have difficulty understanding how sense data is gathered and formed in a way that is meaningful to us. It is known that sense data is interpreted through a propagation of neuronal signals between different parts of the brain. First the data provokes a neuronal response in the parts of the brain that are not associated with higher order brain function. The pattern of activity is subsequently propagated into parts which are. From those parts, additional feedback gets sent to the earlier sensory areas. This manifests in a series of feedforward and feedback loops going back and forth the functionally higher and lower order brain areas. Parts of the brain not associated with direct sense data thus effectively modulate the neuronal activity in areas that are. Another side to this is that sensing in its common form is already a process that we are aware of. So it already presupposes that our experience has gone through some centers of the brain that deal with conscious or subconscious interpretation.

(Kafaligonul; Breitmeyer; Ögmen 2015) Another example is the phenomenon of cognitive penetration. An experiment conducted by Delk and Fillenbaum in 1965 showed that test subjects' color perception was skewed depending on the shape that the color was presented in. What was reported to be orange in the case of geometric shapes was instead reported to be more red when presented in the shape of a heart, which is commonly depicted to be red within western culture. (Delk; Fillenbaum 1965: 290) I believe these two cases support Kuhn's view that sensing itself is an interpretative process.

1.2.2. Semantic incommensurability

Incommensurability has been one of the most talked about issues on the topic of scientific revolutions. Revolutions occur when the majority of the old and new disciplinary matrix become such that logical bridges between them cannot be constructed. It does not mean that there are no good or objective reasons why to favor one or the other. The existence of incommensurability does not point to irrationality. It does, however, point to the matter that when there is no neutral operation for choosing a better theory, values come into play which behave in a different way than a simple procedural follow-through from premises to conclusions. The problem is not in hazy semantics or irrationality. The core of the problem is whether opposing sides come to an agreement on what is denoted by some term. It is about convincing the other side to implement a whole new system of links and associations. As previously discussed, the change has to go through all at once. This poses a problem: Kuhn did not endorse a complete incommensurability like Paul Feyerabend did, but he still claimed that the gestalt switch has a complete grasp. I will pay attention to this difficulty in a few paragraphs.

First I would like to introduce the first account of semantic incommensurability that Kuhn gave in his book. A new paradigm incorporates much of the old paradigm's methods and terminology but the conflict, the incommensurability of those terms, lies in the way those terms were previously used. Behind a meaning there is a whole network of other meanings and connections. When Copernicus used the word 'earth' and stated that earth was a mobile body, his opponents rejected his claims, for 'earth' was a synonym for 'immobile'. Only after changing a set of other terms as well, may the idea of earth moving be acceptable and even unquestionable for science. This is summed up as the incommensurability of terminology.

(Kuhn 1970: 149-150) This implies that meaning is not embedded strictly in the process of referring to an empirical phenomena by universal means, rather meaning is achieved in relation with other meanings. By changing a meaning of a term, its immediate network of meanings changes as well.

What brings about the wish to use terms in a different manner? A central aspect of incommensurability regards the point made by Kuhn that scientists operate in different worlds. Here the epistemological argument given previously becomes relevant. He wrote that the world itself has not changed in a strict sense but scientists are not able to see the same things when looking at a certain aspect of the world. Due to the fact that e.g. two characterizations of a law are incommensurable, the conversion from one to another cannot take place step by step or by the use of careful calculation or logic. The gestalt switch must take place fully and all at once. (150) And since our vocabulary is limited and its use tied to convention – it is easier to use the same word with a different meaning than make up a new word, semantic change will suffice to embed new information in our conventional speech. One example to illustrate the incommensurability of working in different worlds through defining parts of a system differently is to look at mathematical systems. Anyone who has either for professional or recreational purposes, tried to abandon the decimal system point of view to adopt a 6-base, 2-base, or, 12-base view of mathematical calculus, finds it odd at best, extremely nerve-wrecking at worst. The old way of understanding the meaning of numbers is of no use in a 12-based system. In fact, the way we mark the 12-based system, – with a 1 and a 2 ,– is already understood in the decimal system. In English, the words “eleven” and “twelve” are remnants from a non-decimal based way of counting and doing calculus. “Thirteen” and “fourteen” already have a meaning of one full round and three and four more. In a 12-based system, the numbers 10, 11, 12 can easily be replaced with other symbols for clarity. In fact, it helps to operate and understand the process of adding, negating, dividing etc. better. Adopting the otherworldly view of the 12-based system forces one to abandon familiar logic of mathematics and understanding the world, embrace the change on all levels at once and have a gestalt switch because of it. This kind of psychological transition is easier for a young person who has an open mind and is not burdened by long tradition of thinking.

The examples Kuhn and I made are quite intuitive and lack any strict formal interpretation. That is because he did not focus strictly on language. Since Kuhn expressed his views on the matter quite laconically in his book, it made them susceptible to criticism. That pressed Kuhn to philosophically refine his position. Howard Sankey has written about the subject. In his 1993 article *Kuhn's Changing Concept of Incommensurability*, he wrote about the ways in which Kuhn modified his work in later years and how the most understood and famous meaning of incommensurability was not the one that he himself endorsed during his later years. According to Sankey, Kuhn's position on the matter had had three phases: the early account in his original book, the intermediary phase in the 1970 *Postscript* where incommensurability becomes a semantic notion and the third phase where incommensurability becomes a refined semantic and linguistic concept – "a failure of translation in a localized cluster of interdefined terms". (Sankey 1993: 760) In order to show my support for any one of those three accounts, I will clarify their differences.

The first phase was the broadest and seemed to commit Kuhn to some form of idealism. This was because of his explanation of world change. The ability to spot anomalies and the need to construct a new paradigm is due to the fact that these scientists are living in a different world and this world view is facilitated by observational language which cannot be logically linked to the language used to describe the world in the old paradigm. The incommensurability expresses in two ways: words differing in terms of their referent and words differing in ways that they point to a different position in the overall web of meanings. Most of all, incommensurability expresses itself in a broad world view switch which is best described as operating in two different worlds. Aside from seeming like a very nice standard account of simple observation language incommensurability, much like Feyerabend's view, (Oberheim, Hoyningen-Huene 2013) Kuhn took a step back to state that not everything is incommensurable between two paradigms after the world change, rather just the most important and defining concepts of a theory. This contradicted with his dramatic claim that paradigms are a filter for perception and that the change in Gestalt is complete and not achievable by piece-by-piece analyzing. Sankey's comment adds to my summary:

> "In sum, not even the conceptual component of Kuhn's original diffuse notion of incommensurability admits of unified analysis. Paradigms which are incommensurable due to conceptual variance are not derivable from one another; in some sense, they may even be about different worlds; or perhaps they simply fail to have common reference. These disparate elements begin to coalesce during Kuhn's transitional phase, which we will now consider" (Sankey 1993: 765).

In the end of his *Postscript* (200-204), his transitional account was more focused on language. Incommensurability is an issue in translation failure and that is because there is no neutral language to compare theories in, when incommensurable. Deciding which theory is favorable comes on the basis of values and pragmatic concerns. What exactly did this view entail that the old one did not? He clarified that he did not mean that communication would break down completely and that the empirical world still stayed the same after a paradigm change which cleared him of alleged idealism. Communities should try to translate each other's positions. Although, even after translating, the experience of a person formerly in the old paradigm would still be different. He would be a tourist, whereas the younger generation would be natives in that theory. One cannot access the other's world view by translating alone. “Instead, at some point in the process of learning to translate, he finds that the transition has occurred, that he has slipped into the new language without a decision having been made. /../ The conversion experience that I have likened to a gestalt switch remains, therefore, at the heart of the revolutionary process” (Kuhn 1970: 204) As Kuhn turns to translation failures, he draws parallels from Quine's theory of translation indeterminacy but, as Sankey agrees, does so in a flawed way. While according to Quine all translations are of equal measure as there are no true translation rules, Kuhn deviates from this view by treating the difficulties in finding the “right” translation as products of different ontological categories posed by different theories. In doing so he posits an empirical standard against which the testing of righter translations would be possible. (Sankey 1993: 787) The difficulty that cannot be overcome with this new emphasis is that now translation becomes the central aspect of incommensurability but he never provided sufficient explanation as to how these translational failures occur. His first account of logical bridging seemed a much better position and with a bit of tweaking, much less ambiguous than the initial representation in his book.

Kuhn's third position redeems the shortcomings of the second account. Sankey gives an overview of the third position: translation failure occurs in localized clusters of interdefined terms. That is called local incommensurability. Parts outside this local cluster have common measure and can be translated but since the terms are interrelated, the inability to translate has a “holistic effect”. The translation locally fails because meanings are in relations with other terms in the interdefined set. These concepts in the cluster cannot be translated piece by piece because they would lack the necessary conceptual relations in the other theory. (Sankey 1993: 772) I would like to support Kuhn's third position with the following points, although he did not approach this problem from the angle I am going to take. It is an argument from the perspective of the people who are building a new theory.

Between old and new disciplinary matrixes, there are terms that are incommensurable and there are terms that are not. The ones that are not incommensurable I call “anchor terms”. They are anchored to the old paradigm but through them a new paradigm is constructed where the associations and laws between phenomena drastically change also giving rise to new metaphysical commitments and values. By the time the process from revolutionary science settles down to normal science and final wider disputes are settled, the semantic incommensurability even in anchor terms may come about but not necessarily. Kuhn agreed that there was a period between revolutionary and normal science and further paradigm-clearing work is done during this period. It is possible that the anchor terms are also revised to produce a more coherent new theory. This claim stems from my understanding that neither Kuhn nor I endorse a view that there cannot be a world outside our perception but our perception and our language are the only things we have to go on. I will clarify this thought in the next paragraph.

The first premise for my support comes from the fact that puzzles, observational data and values are the basis for developing a new paradigm by the young minds in the field. I think of theory building as connecting different dots to draw a coherent picture. If some dots remain unconnected by associations and laws, they become problems for the younger community who have not had lifelong commitments to perfecting the details of the picture. By approaching science through the old paradigm, it is impossible for them to approach those problems by not committing to anything that has been taught to them very methodically. By holding onto some aspects of the old paradigm they are able to construct a new way of connecting dots that differs drastically from what was before but has some of the same elements as their basis. Theoretically, only a complete outsider could usher a theory based on an entirely different language but as we know, this is not the case in real life institutionalized science.

The second premise comes from the theory of meaning mostly known to be endorsed by Jacques Derrida in his work *Of Grammatology* (1997). A word, even when about simple material phenomena like tables or cows is never as simple as merely pointing to a table or a cow and saying that there it is, that corresponds to a “table” and “cow”. There are no means to bind language to the world. Words hold references to other words which means that meaning is gained in relation to other words and their meanings. It is an inevitability that terms beyond anchor terms lose their reductionist potential and also translational capacity in comparison to the old paradigm because their position in a network of new associations, laws, phenomena and metaphysical commitments have changed and the meaning of words depicting these new patterns change with them. In addition, a very important note about paradigms in the broader sense is that paradigms never solve all the problems which give room for inconsistency within the paradigm and in this way old ideas and seemingly incompatible notions remain.

From the perspective of linguistic webs, linguistic theory and exemplar-dependence, the claims that Kuhn made about cognition being an interpretative process seem sound. The conditions for a Kuhnian revolution come from a shift that cannot be extensions of the same language or mere modifications of it. A Kuhnian revolution has to exhibit deep epistemological and semantic shifts and analyzed with the benefit of hindsight. This will be the key conclusion that will guide the analysis in the second chapter.

2. ON BIOLOGY

In this chapter I will focus on all the aspects discussed in the first chapter: pre-paradigm science, paradigms and semantic incommensurability. My aim is to compare Kuhn's view of science with the historical knowledge about biology and see if there are any similarities that would justify a Kuhnian view on the science of life. In the subchapter on paradigms, I will draw parallels between the constituents of the disciplinary matrix (laws, exemplars, values, metaphysics). I chose the most central term “species” to inquire how it has changed in the course of different theories and ages. By the end of this chapter I will have answered the question of whether it is meaningful, in the sense that it opens up new lines of inquiry and aspects of science, to discuss biology in a Kuhnian framework.

2.1. The Pre-Paradigm Period of Biology

The pre-paradigm period of science or proto-science is illustrated by the absence of an institutionalized network of people who share the same disciplinary matrix or aspects of a wider matrix. Early researchers were people who resembled modern scientists in the procedural sense: they dealt with explaining the world by adopting some sort of method and tried to explain natural phenomena to the best of their ability. The fruit of their labor, however, was something very different from what we are used to calling “science” in the modern era. Kuhn had the same view about proto-scientists working in the early days: “This is the period /…/ during which individuals practice science, but in which the results of their enterprise do not add up to science as we know it” (Kuhn 1970: 163). Texts were written in a way that I call “world building” which means that the presentation of ideas and introductions presupposed an audience that was not automatically familiar with what the author wanted to write about. The majority of the arguments were formed in a step by step manner rather than the common knowledge presupposing article of contemporary science. I do not mean to say that these men from the past were somehow lonely islands in a sea of an ideological or informational void. The Aristotelian influence and the assumption of a basic classical education were of course strongly there. Thinkers in the Middle-Age tended to write difficult and dense texts about the natural world that usually started with the allocution to God like the piece on time and species by Thomas Aquinas (Rickaby 2005: 13-21). What they lacked was a more detailed and sophisticated background system of knowledge that would have been the product of thorough research, peer reviewed and heavily institutionalized. I will present five examples of works of biology to support my claim that in the study of organisms the characteristics of a proto-science lasted from the scholastic era well into the 20th century.

The Aristotelian philosopher and botanist Andrea Cesalpino (1519-1603) is known for his original and thorough descriptions of flowering plants. He classified plants according to their morphology and physiology, rather than on their medicinal properties or just alphabetically. He used the fruits and seeds of plants as the basis for his classification and is thus worth noting as a very talented thinker who differed in his specific method of botanical classification while still retaining his Aristotelian background as a philosopher. Like other thinkers in his time, his theory about the circulation of blood, remained purely speculative and could not be demonstrated by experiments. (Catholic Encyclopedia) Ernst Mayr has also written that Cesalpino's *magnum opus, De plantis,* was written in a familiar logical division that was known to every educated person and directly drawn from Aristotle (Mayr 1982: 158–159). I made him an example of a medieval naturalist because his classification of plant organisms was innovative. However, after his passing, many others provided their own classificational systems from scratch which illustrates the foundational difference between pre-paradigmatic and paradigmatic science.

Charles Darwin and his *On the Origin of Species* is a perfect example of the major groundwork texts that did not presuppose an ingrained common set of commitments. Even in the 19th century, when the Linnean Society of London had been around for some time (it was founded in 1778) and one could rightfully argue that some sort of community had arisen, the introduction and composition of Darwins's *Origins* had still the world-building characteristics that Kuhn referred to in the second chapter of his book. The *One Long Argument* is an easily readable book and its introduction displays a similar style of personal confessions and general information like so many naturalist works at the time. Unlike the dense and highly philosophical works from medieval naturalists (sacral teleology lasted well into the 19th century, of course), it resembles more of a book like today's popular science writing: not too simplistic but not quite what one can read in the form of a modern article in *Cell*, *Nature* or *Science*. It was a book that an educated person was able to read very well. Despite becoming famous and giving rise to several wide-scale discussions outside the academia, it did not start a revolutionary science in a way Lyell's *Chemistry* did. Evolution was a topic reserved for public lectures or an interesting idea that some men pursued but not necessarily in a Darwinist fashion. A paradigm initiating text allowed for the scientists to group under a set of commitments that could be concluded from that text. That enabled them to study the world in detail. After Darwin this did not happen for a couple of generations. It did not provide enough ground on its own. Genetics took off in a separate field of study, highly mathematical and outside the Darwinian evolution that were broadly theoretical and broad at the time. It is a widely held belief that Mendelian genetics and Darwinism were seen as contradictory at the time as it is frequently addressed in genetics classes and by historians (Ceccarelli 2001: 13-60). Resembling metaphysical frameworks rather than evidence based precise notions, the community dealing with the study of organisms was still very much divided between different beliefs and disciplines and in the center of that was the problem of speciation.

Theodosius Dobzhansky and his book *Genetics and the Origin of Species* (1937), as indicated by the title, marked the synthesis of the knowledge accumulated from genetics and Darwin's theory of evolution through natural selection. From the perspective of proto-science versus mature science, I believe this to be the landmark text to be thorough enough in its arguments and mathematics to apply as the base for revolutionary science and to be the initiator of a biology that we know today. As Darwin lacked the notion a hereditary vessel – the gene – to support his theory, theories like neo-Lamarckism were favored because they did not postulate something that carried hereditary modifications, instead organisms were modified by their environment directly. After rediscovering Mendel, whose work had been another quite isolated bundle of independent ideas in the tradition of the researchers and thinkers before modern science, the case for genetics and evolution as compatible domains of research could be made in a satisfactory manner backed by calculations through sophisticated mathematics. Where Darwin could not provide detailed explanations to speciation, Dobzhansky could use the pool of knowledge accumulated by naturalists and geneticists to postulate that evolutionary theory should account for variations in all aspects of the domain of life: from the individual to the species. The goal was to use experimental data from genetics to demonstrate the validity of his claims and by extension, Darwin's and all of his supporters claims. He succeeded, though partially, and is now considered one of the most important scientists in evolutionary biology and his text is presented to young students to be as important as Darwin's. Coincidentally his own famous words echo the notion of a paradigm that puts scientific research into perspective. It was the title of his essay “Nothing in Biology Makes Sense Except in the Light of Evolution” (Dobzhansky: 1973).

James Watson's and Francis Crick's famous paper on the structure of the acid that is responsible for overall most heritability is considered as a revolutionary paper. In this case, the work of significance was a continuation of research that had started with recognizing that DNA was the building block of life. Published on the 25th of April 1953 as an article in *Nature*, it lacked the characteristics of previous examples. Not readable by a layman and strictly for the educated scientist, published in a medium used by the scientific community, it bespoke of the normal conduct in a mature science. Driven by the knowledge that this substance is present in viruses and cells as the mechanism of heritability, the very young field of molecular biology was studying the properties of this substance for the knowledge it might contain about evolution and therefore life in general. I propose that this achievement is not revolutionary in a Kuhnian sense for it was a major breakthrough in an already established program as multiple teams in different universities were conducting similar research. As Michael Ruse also put it in his paper about the Darwinian Revolution, normal science does seem a bit unexciting, a phase between the interesting bits in science (Ruse 2009: 10040) and this makes people want to claim “revolutionary” for something or the other. I understand how it might seems so, but I disagree in the sense that the most detailed and illuminating work is done during the period of normal science. The Higg's boson was a famous discovery but there was nothing revolutionary about it. It was posed by the theory and finally there was the accepted level of probability in measurements to state that this is the Higg's boson. The “molecular revolution” is called such because it was a major achievement of a paradigm driven science and opened up a whole new range of study. Still, it was not like the discovery of X-rays, which were not supposed to be there and thus ushered a change in theory. The structure of DNA as a complementary double helix was received well by geneticists and was almost universally accepted only a few years later. It was a success after anticipation, not an anomaly. Discussing the explosion of molecular biology in the way that I have does not undermine its scientific significance, it just points to the different usage of the term “revolution”. Philosophers will always have the liberty to use revolution in a much broader sense and it is definitely being done as well.

To further illustrate the before and after effect of paradigm driven science, I would briefly like to discuss the work of Motoo Kimura and his team. He researched molecular evolution and applied mathematical models to theorize about the mechanisms of population diversification like the genetic load. He published his very influential article on neutralistic mechanisms in molecular evolution also in the journal *Nature* in 1968. He devoted his later life defending the idea that evolution was driven mostly by neutral mutations in respect to natural selection thus making the genetic drift the prominent mechanism of evolution. His student continues his work but neither of their ideas have been accepted in their full form. Rather “neutralism vs. adaptionism” debates have been continuing in respect of which is more important in terms of evolution. The idea of genetic drift has been present in science for a long time and Kimura's theory with its novel aspects was a continuation of a tradition with its articulated problems and frameworks that stood on the mechanism of genetic drift. Since the “neutral theory of evolution” has actually been integrated to the theory of evolution in the broader sense with emphasis on natural selection, it remains as a special case and complements evolutionary theory for the time being.

In conclusion, the first three examples demonstrated the characteristics brought forward by Kuhn to illustrate a science that was not paradigm driven. Texts were lengthy and dense with world building tendencies and philosophy, a very broad scope, readable by educated laymen and thus not written in a strict tradition of common scientific knowledge. The two latter examples, although very influential and novel displayed the characteristics of paradigm-driven science: articulated narrow problems, continuation of a tradition, argumentation from the position of that tradition, original papers posted in niche magazines meant for a trained scientist, the debates following publication occur within the scientific community and not in the terms of cosmological abstractions. In the light of these examples, I conclude that the Neo-Darwinian Revolution is more accurate than the Darwinian Revolution because it was followed by a noticeable cultural homogeny in science. But this is not nearly enough. Without concentrating on the parts of the disciplinary matrix, over half of Kuhn's claims about the nature of science would be lost. Let this serve as one criterion for establishing whether it is meaningful to speak of a Kuhnian biology.

2.2. On the Structure of Paradigms

Biological phenomena seem to have little to do with the much more rigid reductionist nature of the phenomena studied in physics. Physics is taught with emphasis on formulae and relations with the practicing of problem solving (thus the great importance of exemplars); biology is taught more like history: students must learn gargantuan amounts of facts and their relations are more of an open question for her to ponder on. Naturally, there are mathematical abstractions of phenomena like in every other natural science. The simple laws that make up part of the disciplinary matrix are not as evident in biology as they are in physics despite biology's obvious mathematical nature. I intend to discuss the constituents of the disciplinary matrix to show the reader that biology displays overall similarities with Kuhn's framework but also great differences when it comes to exemplars.

2.2.1. Laws and Exemplars

The discussion about the importance of laws got more precise in the 1970 *Postscript* where laws were posed as a constituent of the disciplinary matrix. Laws are an important element in theory reduction: Tb is reducible from Ta if Tb is logically entailed in Ta. There have been many models of theory reduction but most of them, stronger or weaker, have had the notion that Ta has to logically have the capacity to explain the laws of Tb (Brigandt, Love 2014). I have chosen to work in a Kuhnian framework and given how Kuhn and also Feyerabend were critical of the empiricists' notions of reductionism by introducing incommensurability, I will approach the question “Are there laws in biology?” from the perspective of function rather than theory. More specifically: are there statements in the general realm of biology that are used in the same way laws are used in physics according to Kuhn?

Where do philosophers usually stand when speaking about biological laws? Ernst Mayr was quite known for rejecting reductionism and therefore laws (like they are generally rigidly understood) of biology (Mayr 1982, 19). Kuhn himself did not discuss the topic of biology. Robert N. Brandon thought that laws were an unsuitable term for biology and proposed the term “contingent regularities” be used instead (Brandon 1996: 445). Beatty has proposed his own theory of “evolutionary contingency” and thereby the stance that there are in principle no laws in biology (Beatty 1996: 435). Alex Rosenberg said there was just one law (Rosenberg 1994) and Elliot Sober thinks there are more than one but they are not empirical (Sober: 1997). On the surface, it seems that there are a lot of different views but they all agree on one point: the Positivist definition of scientific law does not hold. The differences in their writings stem from the different emphasis they put on the Positivist definitions of laws and what they think is the suitable alternative to call the relations in nature discovered by biologists.

I agree with Sarah Mitchell and the previously mentioned authors that if we take a strict normative approach to laws, biology has none. A statement like (x)(Px→Qx) where x is the scope of all the world during all time finds no grounding in biology. This has two reasons. First, generalizations like the “dogma of molecular biology” concern biological organisms contingently evolving on our planet, which leaves little room for absolute and timeless statements like (x). Secondly, laws cannot have any exceptions. Of course such a tough criteria is hard to meet even in a field like physics, as has been also argued by Nancy Cartwright (Mitchell 2000: 250)(Cartwright: 1999; cited through Mitchell 2000: 250) By accepting this line of reasoning as fruitless in the current paper, it would, then, make more sense to compare different laws. I am going to discuss the Mendelian Law of Dominance with the help of Mitchell's three-dimensional framework. I chose this specific "law" because most laws in physics are empirical and Mendel's laws are empirical. This leaves out all the statistical law-like statements of genetics that are *a priori* mathematical truths.

Mitchell posited three dimensions to characterize a law: degree of abstraction, stability and strength from zero to one scale. She posited Goodman Coins as zero on all three scales and the Conservation of Mass as 0.9 on all scales. Degree of abstraction stands for how much "noise data" has to be removed for a pattern to emerge; degree of strength stands for the relation between a deterministic an probability based law; degree of stability stands for how stable are the conditions on which the law like pattern of causality relies on. (Mitchell 2000: 258-260) Let us compare the stances on Mendelian Laws. Mitchell puts the laws of inheritance all together in one point on the three-dimensional conceptual space. In her view they are not abstract but quite strong and a little more stable than unstable (263).

I agree with her that the laws of inheritance are not statistical laws in the sense that the amount of various outcomes is not high and they were not discovered by a top down method which is usually the case with statistical abstractions. They are definitely not abstract as they concern visible chromosomes and variations in the phenotype and do not need the postulation of abstract concepts. I do not agree on the stability issue. They are quite unstable as they can be subject to future change when natural selection no longer permits simple two-allele dominance relations. The third law of Mendel is very unstable and relies on the contingency of natural selection like all other laws of evolution. It also rarely applies in nature in pure form. It exhibits the traits of a quintessential empirical law as defined by Carnap while conservation of mass-energy would be the quintessential theoretical law (Carnap 1966). I have not found an equivalent in physics for Mendelian laws which could be for the reason that if some relation in physics has such a narrow scope, it would not be considered a law as the standard for laws is much higher in physics. Mendelial laws, especially the third, have well known deviations appearing almost as frequently as the cases where they actually apply. It is currently no more than a matter of faith whether humans will ever be able to discover more fundamental laws in biology but at the present moment we have the Mendelian inheritance as well as statistical laws and mathematical truths like the Hardy-Weinberg Principle or the power law.

The mathematical *a prioris* are a separate issue. I agree with Sober that, contextually speaking, we should not discard *a priori* statements that form the bulk of the remaining candidates for laws in biology (Sober 1997: 466), but in the case of the Hardy-Weinberg Principle, it does not exist in nature. It is an idealization and works as a comparison model to the data scientists actually get by studying populations. The ideal gas law in physics is a little similar but different in a way that it is applied as an approximation of data, not for comparison. The *a priori* laws exhibit a very low degree of strength, they are more stable than the Mendelian laws for they characterize phenomena that the Mendelian laws depend on and their abstraction level is higher than the Mendelian laws have. This places them in a distinct position on the three-dimensional scale, thus giving evidence of a quite different set of propositions about the world compared to the laws of physics.

Exemplars can be found in the more mathematical branches of biology like ecology, population genetics, biophysics and -chemistry. The latter operate in the general disciplinary matrix of physics and chemistry and try to implement puzzle solving techniques from those paradigms to study biological quantum level phenomena – like the aspects of photosynthesis – or the molecular realm of metabolism. As I discussed previously, exemplars are an extremely important element of paradigms and their science guiding properties. In physics, exemplars provide the basic set of tools and skills for puzzle solving. Biology in general does not exhibit exemplars in disciplines other than population genetics and physics and chemistry based branches.

I support my claim on the position that exemplars are the experiments and historical problems that students learn in order to become puzzle solvers. With exceptions, biology students do not learn to solve exemplary problems. Rather, they are presented with myriads of information that they have to learn by heart and understand that information holistically. Their knowledge is tested in exams by posing questions like "Describe what would happen in the synapse, if organism X had an overexposure to sodium?". Calculating the ion gradient of cells is very straight forward and simple, not requiring anything more from the student than to just learn the formula by heart. Ecology is learned in the same general way: by learning different phenomena and how to divide them into types. The exception to this is using principles from statistics like the power law to represent data, or formulas from epistemology. In ecology, it would be the generalization that when given an infinite amount of optimal habitat, organisms would multiply exponentially *ad infinitum*. In addition, because of the great difficulties in reaching a consensus on whether biology has any laws (Beatty 1996; Rosenberg 1994; Sober 1997; Brandon 1996) or whether biology is a generally reductionist science (Brigandt; Love 2014), the field does not fit the Kuhnian model of science where puzzle solving is guided by exemplars, thus making exemplars the very core of science. Kuhnian science demands that there are laws and the representation of the natural world ties to a more or less neat web of relations that are reducible one way or another. Reductive reasoning within a single paradigm is commonplace and exemplars quite literally exemplify that fact.

2.2.2. Metaphysics and Values

I would like to briefly discuss the role of metaphysics and values as parts of the matrix. A shift in a paradigm obviously requires a shift in all four aspects of the matrix. I favor Dobzhansky's work as a good candidate for a revolutionary text for the reasons that it provided Darwinism with methods, mathematics and further evidence. However, in the light of metaphysics and values, I believe Michael Ruse is right when he claimed that there is a before and after Darwin, rather than a before and after Dobzhansky. The two of my previous conclusions support a Neo-Darwinian revolution, the conclusion I will draw from this section does not.

Ruse has three points to support his claim of a metaphysical change brought about by Darwin’s *Origin*:
a) humans are a part of the natural world,
b) humans are shaped by natural selection,
c) humans do not have any objective value over other parts of nature like trees or mice.
(Ruse 2009: 10043)

I agree with these examples to a certain extent. All three are considered true by popular accounts of history but I would like to specify that the three statements are not required from a scientist to do acceptable science. Therein lies the rub, the shift in metaphysics and values is the most lucid of Kuhn's ideas and deserves separate thorough discussion, for it is unclear if one should focus on the changes more connected to attitudes about research and cosmology or on a broader change in culture outside science due to scientific work. Ruse appealed to a bigger cultural change in his examples and I take the same standpoint in my arguments. The extent to which I do not agree with Ruse, is that as far as metaphysical or value incommensurability goes, there seems to be an odd case of the value vocabulary's semantic morphing which brings about serious doubts whether there has actually been a significant leap in our understanding of life. I argue that there has not been a profound change in the general line of reasoning about the natural world, rather there has been a profound change in words. This is demonstrated by the cases of teleological reasoning and essentialism ever present in scientific discourse where in its most robust form “God” is substituted with “nature” or “evolution”.

Ruse has insightful examples. Indeed, around the time of Darwin, the question “What is human?” took a turn as the nature of man became a serious topic for discussion outside the realm of theology and ethics. Evolutionism as an atheist approach to life was discussed by philosophers before the synthesis and e.g. social Darwinism became a social philosophy well before the 1930s when the synthesis happened. In these two cases, the addition of genetics provided a complementary piece of evidence for the mentioned ideas but did not facilitate any on its own. Genetic determinism is an upgraded addition to support hierarchic social structures and oppression of individuals based on their alleged traits that scientists are allegedly able to determine. Therefore I do not believe the synthesis provided anything more than support for ideas that were already in circulation in the social sphere. It is undeniable that there was a change in tune after Darwin. On the surface, it seems that there were significant cosmological changes concerning very broad and big questions like where should humans look for moral guidance and how to live in accord with nature. It does not seem so obvious when the topic of teleology comes into play.

The existence of teleological thinking is embedded in the way laymen and even some scientists, particularly those who are strongly ideologically minded, interpret terms like “function” and “design”. Most philosophers of biology believe that teleological notions, usually attributed to pre-Darwinian naturalists, are an ineliminable part of the contemporary field of biology (Allen 2009). Speaking about function and design, it becomes apparent that organisms or parts of organisms follow a plan that was crafted by nature. The moral dimension of these claims come to play when failing to execute that plan gets assigned the value “bad” and succeeding the value “good”. One example of this would be the moral pressure to procreate because it is what we do. It is what evolution meant when it created us. Another example is more subtle: because humans are sexual creatures, it is justified to explain their behavior from the basis of sexual procreation in most aspects. A third example is the belief that objective behavioral science should aid people in finding the best mate and to better society according to our “real” human nature. Even if these claims are a minority (I believe they are not) they are a vocal minority, mostly expressed by public thinkers, politicians and popular science writers alike. As their voices are heard and their ideas discussed, they reflect a part of our culture very present and real, and mirror the same type of thinking assigned to “a more primitive time”. I believe that when “natural” was once considered from God and in accordance with his plan (idea and form), it gave moral weight to claims about how humans and animals should behave. In present secular discourse and in the cases I just mentioned, “God” has been substituted with “nature”. The phrase “like nature intended” is a synonym for “like god intended”. God created us to rule over the beasts of the earth is a synonym for “we evolved to be the top predator, thus we have the right to treat our food as we please”. Grasping the contemporary scientific work on evolution gives people a new verbal arsenal but does not in and of itself make everyone accept evolution as a blind and non-teleological process in line with e.g. Dennet (2012). I cannot see any evidence to suggest that we have somehow broken away from anthropocentric thinking in broad terms like Ruse pointed. Rather I have noticed a new form of anthropocentrism from the perspective that it is natural for a species to use up all the resources available to them and this will not stop until a catastrophe happens.

Teleological explanations that resemble the very discourse attributed to past eras, is still prominent in discussions everywhere and even in the classroom itself. It is questionable whether the points made by Ruse have really sunk in to society – it could be more superficial than we would like to admit. To completely deny a change is, of course, absurd and I support Ruse's view that Darwin's conclusions on humans and our understanding of our roots in terms of having common ancestors with other animals on this planet was a true novelty that has profoundly shaped our culture and intellectual life.

2.3. Semantic Incommensurability

Semantic incommensurability is a key concept to understanding a Kuhnian revolution. In this subchapter, I will discuss the biological concept of species. I want to show how the concept has changed through time and how the use of a concept so central to a theory is able to hinder scientific progress and guide research in a very Kuhnian way. The history of the term will also reveal whether Dobzhansky’s work included a revolutionary aspect in terms of semantic incommensurability.

Jonathan Hodge wrote about the Aristotelian way of understanding species as the embodiment of their purposes through their natural actions. Species as forms are preserved but forms of individuals originate from the forms of their ancestors – a cyclic change happening in a fixed domain of species; like the changing of the seasons: different but on a broader level the same. Species do not have origins of their own. (Hodge 1990: 374-376) Christian thought like that of Aquinas, Hodge characterizes, was influenced by the same Platonic and Aristotelian metaphysics: species are the diverse imitations of the Ideas in the divine mind and they come in different forms for they have to be diverse in order for the goodness of the holy mind to be represented on earth. Here forms are both Platonist and Aristotelian: forms independently originate from God and species are everlasting in their nature. All forms were brought into being and no new forms can come, all that can be already is. (378) After a few centuries, this trend starts to change. As an Enlightenment era naturalist, Buffon's view was incommensurable with the previous theories of the world because he spoke not of Ideas, Forms nor Creation from the divine mind. His view of the world was thermodynamical: everything on this earth came to existence because of extraordinary circumstances in the past and the particulars that succeeded in keeping stable constellations are the species and structures he saw in nature. Since gravity was constant, he explained it as a matter of heat. (Hodge 1990: 374-380) “All that can be, already is” was substituted for “things are because causal forces change what was”. Species no longer signified the divine design reflecting a structure that was ineluctable; they signified structures that came to be because of the state of the causal conditions in the distant past.

Coming closer to Darwin, the Newtonian frameworks of Linné and Lamarck were at first reluctant to make any bold claims about the origins of different species, although, as geology advanced, the fact that there had been older species living on earth was indisputable to these men. Lamarck broke away even more, claiming that the heat from earth was enough to bring about constant changes in species, thus further moving away from whatever staticism there was left in the thermodynamical cosmology of Enlightenment naturalists. By this time, the world is characterized by constant shifts in forces dictated by natural laws – it is fully mechanistic and naturalistic. Separate phenomena do not reflect God's work directly; rather the world in its entirety was once set in motion – with purpose – and now ticks on its own with the exception of human souls and morality.

What was novel about Darwin? As others were reluctant to express their views on species origins and focused on the regions and adaptations of organisms that might give a clue where some species came to existence, Darwin solved the mystery of the origins of species by focusing on the “how?”, not “where?”. He was the product of his era, a thinker in the same cosmology as his predecessors, but as he decided to focus on reproduction rather than regional adaptation, he constructed a whole new structure of systematics which changed the concept of species to organisms shaped by common ancestry and selection of fixed traits. Those traits were adaptive and ever subject to pressures and change. There were no longer talks of fixed degrees of stability in nature which determine the different appearance of flora and fauna. Species were no longer static units with rigid boundaries in respect to their creation. The deconstruction of the whole web of associations about species was incommensurable with respect to the preceding accounts of more or less fixed sets of organisms. As Kuhn stressed, the inability to translate concepts comes from wide taxonomical differences about ontology and here it shows very clearly that this is the case.

As very few people accepted Darwin's work without criticism, the already divided fields of biology all continued on their separate issues and programs. After some time of discord between organismal biology and genetics, enough evidence and theoretical fine tuning had accumulated for motivating Dobzhansky to unify the sciences. “/../ Dobzhansky's *Genetics and the Origin of Species* /../ is not merely a synthesis of Darwin's and Mendel's legacies, but of a Western tradition in theoretical population genetics and a Russian tradition in experimental genetics on populations,” Hodge concluded; and added that new developments in genetics meant a new evolutionary causal theory and that book specifically delivered on the evidence (Hodge 1990: 393). In this book, species were discussed on the basis of genes and gave rise to the phylogenetic concept of species. But is this shift significant enough to call it incommensurable?

I dare to argue that no. Darwin and Dobzhansky used terms with quite equal measure. Richard Lewontin is in agreement with me. Although famous for taking a genetical standpoint to illustrate natural selection at work, Dobzhansky still used the so called classical definition of species taken from Darwin:

> “Species are groups of interbreeding organisms that have been cut off, biologically, from sharing heredity with other species with which they share a common ancestry in the remote past. This reproductive isolation is the final step in divergence between geographically separated populations, geographical races, which were originally kept apart only by geography, but which have acquired during their geographical separation sufficient genetic difference to prevent future interbreeding” (Lewontin 1997: 351).

Dobzhansky used the same concept in two separate works, *A Critique of the Species Concept in Biology* (1935) and *Genetics and the Origin of Species*, favoring a definition that mainly applied to sexually reproducing organisms. By using the concept in essentially the same way and by showing how genetics is able to answer questions about speciation – also a term relying on the works of Darwin, Dobzhansky's work exhibits no drastic change in relation to this concept – there is no significant incommensurable cluster of meanings that would trigger a holistic change in world view. He did manage to start a new discipline of evolutionary population genetics but it did not fulfill the requirement of a gestalt switch.

CONCLUSION

In the first chapter I specified the features of pre-paradigmatic science. I also articulated the definition of a paradigm and explained that the circularity of the definition was a pseudo-problem in respect to Kuhn and other sociologically minded philosophers. At the end of the first chapter I showed how semantic incommensurability and Kuhnian paradigm shifts as scientific revolutions were tightly connected, making semantic incommensurability the most important condition of a paradigm change.

In the second chapter I compared the features of pre-paradigmatic science with the short history of biology and found that Dobzhansky's *Genetics and the Origin of Species* exhibited far more traits of a paradigm launching text than Darwin's *Origin of Species*: the unifying of different disciplines, institutionalization of science and a solid mathematical base. The next step was to compare the constituents of the disciplinary matrix with aspects of biology. I concluded that there are far less universal generalizations about biological properties than there are propositions of the same type in physics about nature. When positioned in Mitchell's three dimensional system for laws, biological generalizations are either weak, abstract and unstable; or strong, particular and very unstable. I also concluded that in biology there are generally no science guiding exemplars of the kind that Kuhn talks about existing in physics. In the section about values and metaphysics I concluded that contemporary biology still exhibits traits of the cosmology that was there even before Darwin: it exhibits both teleology and anthropocentrism and the shift in metaphysics and values is not as profound as Ruse described. Finally I concluded that semantic incommensurability of the term ’species’ did come about after Darwin but this was not the case for Dobzhansky who instead used the Darwinian concept throughout his work.

From the separate smaller conclusions I made in course of my text I can answer that neither Darwin's nor Dobzhansky's text alone suffices to meet all the conditions of a Kuhnian paradigm initiating text. Taken together they satisfy the conditions of a paradigm initiating text more loosely than I would like to permit, although in a significantly more satisfying way than separately. This brings me back to the bigger question I wanted to answer. Is it sensible to apply Kuhnian terms to biology? I believe Kuhn's framework has too much restrictions to fit biology, thus drawing away attention from the interesting aspects of it like the rapid branching out in the 20th century and the role of developmental biology and the molecular revolution. The constituents of the disciplinary matrix did not emerge clearly and if one has to specifically search for phenomena to make the picture fit a certain theory, then our view of science is distorted rather than illuminated.

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Kas Kuhni teadusrevolutsioonid on bioloogiale rakendatavad?

Resümee

Thomas Kuhni teost *Teadusrevolutsioonide struktuur* kasutati siin töös, et võrrelda selles sisalduvate ideede rakendatavust bioloogiale. Autor jõudis järeldusele, et Kuhni raamistik on liiga piirav, et bioloogia uurimisele midagi uut ja huvitavat juurde anda, sest bioloogias ei ole tema poolt postuleeritud tingimused rahuldaval määral täidetud. Bioloogias ei esine laiahaardelisi seadusi lihtsas vormis, õpilased ei tutvu teadusega laialdaselt läbi eksemplaride matemaatilise lahendamise ja nii väärtuste kui ka metafüüsiliste uskumuste järsk muutus on kaheldav. Sellegipoolest võib möönda, et võetuna eraldi suuremast teooriast on ideed semantilisest ühismõõdutusest ja normaalteaduse end taastootvast funktsioonist ka bioloogias huvitavad ja seda on väärtuslik uurida. Kõige huvitavam küsimus on see, et kui eksemplare bioloogias ei ole, siis mille järgi õpivad noored teadlased institutsionaalset teadust tegema?

Do Kuhnian Revolutions Suit Biology?

Abstract

Thomas Kuhn's book *The Structure of Scientific Revolutions* was used in this paper to compare whether the ideas written in that book were applicable in analyzing biology. The Author came to the conclusion that Kuhn's framework was too restrictive to provide new and interesting perspectives on biology. The conditions thought by Kuhn were not met with enough accuracy. There are no wide-scale simple laws in biology, students do not adopt a paradigm with the help of mathematical solving of exemplars and the required revolutionary rapid change in values and metaphysics is also not self-evident. Although, taken separately, semantic incommensurability and the idea of normal science reproducing its foundations through research is interesting also in biology and worthy of research. The most important question that rose from this work is that if there are no exemplars in biology, how does the student integrate into the research tradition.

Lihtlitsents lõputöö reprodutseerimiseks ja lõputöö üldsusele kättesaadavaks tegemiseks

Mina Anna Elise Rohtmets

(sünnikuupäev: 31.03.1992)

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