Interpreting the History of Evolutionary Biology through a Kuhnian Prism: Sense or Nonsense?

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Traditionally, Thomas S. Kuhn's The Structure of Scientific Revolutions *(1962) is largely identified with his analysis of the structure of scientific revolutions. Here, we contribute to a minority tradition in the Kuhn literature by interpreting the history of evolutionary biology through the prism of the entire historical developmental model of sciences that he elaborates in* The Structure. *This research not only reveals a certain match between this model and the history of evolutionary biology but, more importantly, also sheds new light on several episodes in that history, and particularly on the publication of Charles Darwin's* On the Origin of Species *(1859), the construction of the modern evolutionary synthesis, the chronic discontent with it, and the latest expression of that discontent, called the extended evolutionary synthesis. Lastly, we also explain why this kind of analysis hasn't been done before.*

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1. Introduction

About a century ago, the American anthropologist Franz Boas (1911) explained that our experiences of the world are infinitely diverse and that, consequently, reductive linguistic categorization is necessary in order to be able to think and talk about it. Evidently, our categorization of the world doesn't end there but is supplemented with second-order, cognitive abstractions and categorizations. Examples are typologies of earthquakes or human diseases and developmental schemes of civilizations or sciences. An example of a developmental scheme of sciences is that of the Canadian sociologist and historian of science Yves Gingras (1991). He distinguishes three steps in the sociological development of scientific disciplines: the emergence of a new practice, its institutionalization, and the shaping of a social identity. Méthot (2016) uses this simple tripartite framework in his analysis of the history and historiography of virology.

Here, we want to apply the developmental model of sciences that Thomas S. Kuhn elaborated in his *The Structure of Scientific Revolutions* (1962) to the history of evolutionary biology. Of course, as its title indicates, scientific revolutions form the main part of Kuhn's book. Or, as he put it himself (Kuhn [1959] 1977, p. 226): the emphasis of *Structure* is on "the importance to scientific development of 'revolutions'." It cannot be denied, though, that it also offers a "schematic description of scientific development" (Kuhn 1962, p. 160) or of "the essential structure of a science's continuing evolution" (Kuhn 1962, p. 160) and that his analysis of the structure of scientific revolutions merely forms a part of that broader developmental scheme.[1]

In a first section (section 2), we argue that this scheme can be discerned in the history of evolutionary biology, albeit only partly: on the one hand, it encompasses a phase that cannot, or not yet, be discerned in the history of evolutionary biology (a Kuhnian revolution), on the other hand, that history started (in earnest) with a very important achievement (the publication of Charles Darwin's *On the Origin of Species* in 1859) that, strangely, doesn't really fit in Kuhn's original scheme. Our revelation of this imperfect match between Kuhn's developmental model and the standard historiography of evolutionary biology is, in itself, a not very relevant or maybe even trivial exercise in the philosophy of the history of science.[2] Our analysis doesn't end there, though: as

1. Greene points out that *Structure* was the first attempt to "construct a generalized picture of the process by which a science is born and undergoes change and development (...)"([1971] 1981, p. 30).

2. The standard historiography of evolutionary biology is also known as 'Synthesis Historiography' since it tells the story of this history in ways that "turn out right for the Modern Synthesis" (Stoltzfus 2017, p. 4). Stoltzfus and Cable (2014) develop an alternative historiography. They claim that a Mendelian-Mutationist synthesis emerged in the early twentieth century and that contemporary evolutionary thinking seems closer to this synthesis than to the

we will show in section 3, a Kuhnian x-ray scan of the history of evolutionary biology yields several new insights in that history. Lastly, section 4 is dedicated to explaining why the history of evolutionary biology hasn't been interpreted in terms of Kuhn's developmental scheme before.

2. An Imperfect Match

The broad outline of Kuhn's scheme is well known: a pre-paradigmatic phase in the historical development of a science is followed by the emergence of a paradigm, a normal science phase and a phase of extraordinary science and revolution. The main distinction in *The Structure* is that between this revolutionary phase and normal science. This is the 'essential tension' in scientific research (see, e.g., Marcum 2015, pp. 46–7 or Bird 2000, ch. 2). The pre-paradigmatic phase, by contrast, is only briefly discussed in chapter two, "The Route to Normal Science."[3] It is characterized by the coexistence of a number of competing schools which each interpret a natural phenomenon in a different way. The bickering of adherents of these different pre-paradigmatic schools is not entirely fruitless, though. They are genuine scientists, but much of their efforts are devoted to combatting and trying to convince opponents, rather than to cumulative research.

A normal science phase starts with the emergence of a paradigm: a new theoretical framework that inspires the work of a majority of the practitioners of a specific discipline because it solves important problems and offers research opportunities and particularly because it provides them with standard examples of good scientific practice. Or, as Kuhn put it himself:

> [These works were] sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve. Achievements that share these two characteristics I shall henceforth

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modern evolutionary synthesis. Consequently, it is the Mendelian-Mutationist synthesis that should be considered as the foundation of modern evolutionary biology. For another example of a criticism of the Synthesis-dominated historiography of evolutionary biology, see Amundson (2005).

3. In this chapter, Kuhn points out that the study of light and of electricity was characterized by fundamental disagreements before the publication of the work of, respectively, Isaac Newton and Benjamin Franklin (and his immediate successors). These disagreements also characterize (1) "the study of motion before Aristotle and of statics before Archimedes, the study of heat before Black, of chemistry before Boyle and Boerhaave, and of historical geology before Hutton" (Kuhn 1962, p. 15) and (2) the study of life phenomena before the appearance of unspecified biologists. He is less certain about the social sciences: "it remains an open question what parts of social science have yet acquired such paradigms at all. History suggests that the road to a firm research consensus is extraordinarily arduous" (Kuhn 1962, p. 15).

> refer to as 'paradigms,' a term that relates closely to 'normal science'. By choosing it, I mean to suggest that some accepted examples of actual scientific practice (...) provide models from which spring particular coherent traditions of scientific research (1962, p. 10).[4]

This paradigm concept was inspired by the fundamental difference between internally divided social scientists and more united practitioners of the natural sciences that Kuhn noticed during a stay in 1958–1959 at the Center of Advanced Studies in the Behavioral Sciences (see Kuhn 1962, p. x). However, by 1974, he had changed this origin story of his central term. It was now not coined to account for the difference between the social sciences and the natural sciences but, more in general, "to account for the consensus necessary among scientists in a specialty in order for them to pursue their research goals effectively" (Wray 2011, p. 49). Also, Kuhn now claimed that it was "the sense of 'paradigm' as standard example that led originally to [his] choice of that term" (Kuhn 1977, p. xviii): a group's unproblematic and common or paradigmatic conduct of research was facilitated by those shared examples of successful practice (i.e., paradigms in the specific meaning of exemplars). They could provide what a paradigmatic group "lacked in rules" (Kuhn [1974] 1977, p. 318).[5]

In the postscript to the second edition of *Structure*, he had already claimed that the term paradigm had been inspired by the exemplar component "of a group's shared commitments" (Kuhn [1962] 1970, p. 187). He introduced this distinction between exemplars and a group's "shared commitments" (i.e., "the entire constellation of beliefs, values, techniques and so on shared by

4. This is also how he defines paradigms in the preface of *Structure*: paradigms are "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners" (p. viii). The term 'achievement' or 'achievements' appears 28 times in the first edition and 3 times in the postscript to the second edition. However, the idea that such achievements or paradigms (*sensu lato*) define a scientific tradition by providing exemplars to a community of scientists seems to be more applicable to the practice of the physical sciences than to that of the life sciences (see, in this respect, Hacking 2012, p. xxiii; see also note 9; for examples of exemplar-based accounts of episodes in the history of biology, see Creager 2016 and Shan 2020).

5. In the same essay, he claimed that the distinction between what he had called a pre- and a post-paradigmatic phase in the development of a science, was "both typical and important" (Kuhn [1974] 1977, p. 295), but that it could "be discussed without reference to the first achievement of a paradigm" (Kuhn [1974] 1977, p. 295n4). Because, "[w]hatever paradigms may be, they are possessed by any scientific community, including the schools of the so-called pre-paradigm period" (Kuhn [1974] 1977, p. 295n4). However, he also pointed out that the transition to the post-paradigmatic phase usually happens "in the aftermath of some notable scientific achievement" (Kuhn [1974] 1977, p. 295n4) (i.e., a paradigm in the broadest meaning of the term). It probably shouldn't surprise us that the term paradigm "practically vanished from Kuhn's writing beginning around 1980" (Marcum 2015, p. 177).

members of a given community" [p. 175]) or disciplinary matrix in response to criticisms of his multifarious semantic use of the term paradigm in the first edition (Shapere 1964; Masterman 1970; see also Hoyningen-Huene [1989] 1993, pp. 131–32). Exemplars were the main components of such a matrix and "the most novel and least understood aspect" (Kuhn [1962] 1970, p. 187) of *Structure*.

Whatever the true origin of the term, according to the first edition of *Structure*, paradigms are important theoretical achievements (paradigmatic theories) that develop out of one of the pre-paradigmatic theories, or originate as a synthesis of some of these theories. They subsequently inspire and guide the research of a previously divided community of scientists: "Except with the advantage of hindsight, it is hard to find another criterion that so clearly proclaims a field a science, than the creation of a paradigm that proved able to guide the whole group's research" (Kuhn 1962, p. 22). It is only then that scientists engage in the kind of esoteric and technical research that scientists typically do. Symptomatic for this professionalization is that scientists increasingly communicate this research to their peers in specialized journals, rather than in books, directed at a general public. Normal science can also encompass reformulations of paradigms. These are sometimes required to solve theoretical problems. Kuhn refers more particularly to eighteenth- and nineteenth-century reformulations of Newton's mechanical theory "in an equivalent but logically and aesthetically more satisfying form" (1962, p. 33). Lastly, when a paradigm is confronted with serious or significant anomalies, a crisis ensues which, in some cases, is followed by its replacement by a new paradigm. Ruse distinguishes four dimensions in this revolutionary process: the new paradigm unites a new group of scientists (the sociological dimension), provides them with a new *Weltanschauung* (the psychological dimension) and ditto methodology and data (the epistemological dimension) and even changes the world (the ontological dimension): "it is not simply a question of the world seeming to change for us, but rather the world really does change" (1978, p. 246).

In what follows, we will show that at least three of Kuhn's four phases can be clearly distinguished in the history of evolutionary biology: the pre-paradigmatic phase (section 2.1), the emergence of a paradigm (in the general meaning of 'paradigmatic theory') (section 2.2) and a normal science phase (section 2.3).[6] Lastly, we will briefly discuss the question whether we are currently witnessing a Kuhnian crisis and the start of a Kuhnian revolution (section 2.4).

6. Our previous analysis of these phases (Tanghe et al. 2018) is here slightly modified and also in various ways extended.

2.1. The Pre-Paradigmatic Phase

Charles Darwin (1859) managed to put the notion of evolution (as it later came to be known) on the late-nineteenth century scientific agenda, but he did not succeed at all in creating a single, united community of evolutionary biologists. Put differently, Darwin's original theory of evolution *never became a paradigm*. Instead, there emerged a large number of not always sharply delineated pre-paradigmatic schools of evolutionists (Bowler 1983) which each interpreted the phenomenon of evolution in a different way and focused on specific biological phenomena that suited their theory best.[7] For example, neo-Darwinists were preoccupied with adaptations, mutationists with discontinuous variations and orthogenesists or adherents of the idea of straight-line evolution with (presumed) trends in the fossil record. All of these theories "competed for status" (Largent 2009, p. 3). The standard term for this pre-paradigmatic phase in the history of evolutionary biology is "the eclipse of Darwinism."

2.2. The Paradigm

The pre-paradigmatic chaos in evolutionary biology came to an end after the Second World War, when the long genesis of the modern evolutionary synthesis (henceforth MS) culminated in its coronation, at the 1947 Princeton Conference on Genetics, Palaeontology and Evolution, as the first paradigm of evolutionary biology (Smocovitis 1996). Provine speaks in respect with this genesis of an "evolutionary constriction" (1989, p. 61): one pre-paradigmatic approach of evolution, that of population genetics, came out victorious and all other alternatives lost all credibility among a majority of biologists. To be precise, population genetics became the "formalized core of the MS theory" (Müller 2017, p. 2).

The main reason why population genetics was victorious is that it offered a convincing, mathematical solution for a major pre-paradigmatic problem: the question whether the natural selection of continuous variations can cause major adaptive transmutations of populations. The Cambridge mathematician H. T. J. Norton was the first to make use of the Hardy-Weinberg equilibrium principle (i.e., the relative frequency of alleles remains the same in a population, unless this equilibrium is disturbed by forces like selection) and to show, in an appendix to R. C. Punnett's *Mimicry in Butterflies* (1915), the efficacy of natural selection. The finding that even a slight advantage would lead to a substantial increase in the frequency of a favorable allele over a few generations "came as a surprise to most people, and it stimulated a flow of

7. Bowler (1983) distinguishes three schools: Lamarckism or neo-Lamarckism, orthogenesis and the mutation theory. Elsewhere (Bowler [1989] 2009), he speaks of four schools: Lamarckism or neo-Lamarckism, orthogenesis, the mutation theory and Weismann's neo-Darwinism.

new research" (Young 1993, p. 190). Population genetics thus also introduced "a new and more rigid definition of the field" (Kuhn 1962, p. 19): "a change in the genetic composition of populations" (Dobzhansky 1937, p. 11). This became the primary or default definition of evolution for many evolutionary biologists and an important inspiration for evolutionary research (Michod 1981, Charlesworth and Charlesworth 2017), although it was "never totally accepted" (de Jong and Scharloo 1988, p. 1). The MS was, nevertheless, welcomed as a breath of fresh air since "it was soon appreciated that a basis had now been laid on which future evolutionary studies could be safely built" (Young 1993, p. 218).

2.3. The Post-Paradigmatic Phase: Normal Science

It is undeniable that the study of evolution became more professional after the establishment of the MS as a paradigm. The fuzzy, philosophical concept of "progress" was expelled from evolutionary theory (Ruse 2009a). The phrase "evolutionary biology," first coined in 1881 in Grant Allen's *Vignettes From Nature* (he spoke of an evolutionary biologist), "increasingly became an accepted disciplinary appellation" (Smocovitis 1996, p. 163). In 1945, the Society for the Study of Evolution (SSE), replaced the Society for the Study of Speciation. Specialized journals, dedicated to evolution, were founded in several countries. In 1946, the SSE, on the occasion of its First Annual Meeting, established a research journal in the field of evolution: *Evolution: An International Journal of Organic Evolution*.[8] The journal *American Naturalist* turned from experimental biology to evolutionary biology. Courses in evolutionary biology blossomed and important evolutionists moved back from museums into university departments.

Most importantly, the MS marked the start of a period of normal science. Dickins and Rahman (2012, p. 2914), for example, speak in respect with the period after the emergence of the MS of "many years of normal scientific activity exploring the hypothesis-space that it created." Normal science consists, in general terms, of the realisation of the promise of success in the investigation of nature that largely explains the attraction of a paradigm. It encompasses an empirical and a theoretical dimension and is achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing or relevant ("factual determinations"), by matching of facts with theory, and through a further articulation of the paradigm itself. It is through normal

8. The original subtitle was: "*An International Journal of Evolutionary Biology*." The SSE changed it in 1947. Smocovitis (1996, p. 163n199) couldn't find a reason, in the archives, for this change. She believes that palaeontologists, who occupied positions in geology departments, found it difficult to adopt the term "evolutionary biology." Mayr's remark that some older members of the SSE wanted the word "organic" included in the journal's title offers an alternative or additional explanation. See: http://www.evolutionsociety.org/history.html.

science that a paradigm acquires the precision that is necessary to reveal the anomalies that can lead to a crisis and, eventually, a revolution. Empirical normal science can be thought of as forcing "nature into the preformed and relatively inflexible box that the paradigm supplies" (Kuhn 1962, p. 24), whereas theoretical normal science (i.e., the modification, reformulation and further articulation of the paradigm) can be thought of as extending and also modifying that "relatively inflexible" box.

The MS clearly inspired these two general kinds of Kuhnian research.[9] Examples of empirical research are the application of the models of mathematical population genetics in the field and in the laboratory (e.g., Lewontin et al. 1981), the study of swift changes, through natural selection, of body and beak size in Galápagos finches in response to changes in their food supply (Grant and Grant 2003, 2008), and the empirical study of cladogenesis through geographical isolation (allopatry; Mayr 1942). The MS has, secondly, also been greatly extended and substantially modified (i.e., theoretical normal science), especially in the last few decades.

Some examples of important modifications of the MS are Kimura's neutralism (1968, 1991), the exploration of alternative speciation models (i.e., next to allopatry) (Mallet 2001), and the acknowledgment that macroevolution is not always merely an extrapolation of microevolutionary processes—one of the tenets of the original MS (Erwin 2000). Lastly, Lu and Bourrat (2017) distinguish three kinds of extensions of the MS: classical population genetics has been generalized to quantitative genetics for continuous traits and is now also better suited to account for the evolution of microorganisms and plants. A third category of extensions consists of progress in various biological subfields. Two examples are the theory of major evolutionary transitions (Maynard Smith and Szathmáry 1999) and Bill Hamilton's (1964) neo-Darwinian explanation (i.e., kin selection) of altruism.

2.4. A Kuhnian Crisis?

Some scholars suggest that evolutionary biology is in the thralls of a Kuhnian crisis and that the so-called extended evolutionary synthesis (henceforth EES) (e.g., Pigliucci and Müller 2010) is about to replace the MS. For example, Dickins and Rahman write:

> The MS is now a commonly agreed framework adhered to by many biologists. But one might expect a gradual accumulation of glitches, of

9. Kuhn's detailed account of 'the nature of normal science' (ch. 3) is structured around these two kinds of research. They are each subdivided in three subcategories. However, these subcategories are clearly very much inspired by the practice of the physical sciences and, therefore, not always applicable to the practice of evolutionary biology (for example experimental efforts to articulate a paradigm by determining universal constants).

> findings that do not quite fit with the common framework, and as this accumulation increases more suitable theories will begin to be sought, theories that might encompass the glitches as well as reorganize our previously accepted facts. We would anticipate a paradigm shift. It is this form of analysis that informs those seeking an extended synthesis. (2012, p. 2914)

That remains to be seen though. As the eminent Kuhn scholar Hoyningen-Huene writes in his Kuhnian analysis of the chemical revolution: "Normal science ceases and a phase of extraordinary science begins when significant anomalies indicate that there are very serious problems concerning the currently accepted theoretical and/or experimental framework" (2008, p. 105), and "[t]he term 'significant' just used to qualify anomalies is of extreme importance," since most anomalies are "an omnipresent concomitant of normal science" (2008, p. 105). Are evolutionary biologists currently really faced with significant anomalies that indicate that there are "very serious problems" with the MS? We doubt it. Telling, in this respect, is that most adherents of the EES themselves do *not* believe that this model is an emerging, new paradigm. Rather, they tend to interpret it as an alternative, Lakatosian research program (see, e.g., Pigliucci and Finkelman 2014; Laland et al. 2015; see also Pievani 2012).[10] In the following section, we will propose an alternative and new Kuhnian interpretation of the EES.

3. New Insights and Contributions

As pointed out above, the identification of a certain correspondence between phases in Kuhn's developmental scheme and stages in the history of evolutionary biology is, in itself, quite trivial. The more pertinent question is whether a Kuhnian analysis of that history is heuristically rewarding. We believe that this is indeed the case: such an analysis particularly provides us with a better insight in the so-called eclipse of Darwinism (3.1), in the still poorly understood genesis of the MS (3.2), and in the relatively turbulent normal science period in the history of evolutionary biology (3.3), including the precise nature of the EES. As we shall see, the extreme multidisciplinarity of the science of evolutionary biology (Welch 2017) left a strong mark on each one of these three Kuhnian phases in its history.

3.1. The Misnomer, Called the Eclipse of Darwinism

We will return to the place (or lack thereof) of Darwin's *On the Origin* in Kuhn's (original) scheme and the implications of our Kuhnian analysis for

10. Müller (2017) is an example of a more ambitious proponent of an EES. He claims that the term (or acronym) 'EES' is not meant as a simple extension of the MS, "as sometimes wrongly implied, but to indicate a comprehensive new synthesis (…)" (p. 9).

the historical status of his theory of evolution in section 4.1. (see particularly note 17). Here, we want to focus on what followed the publication of this seminal work. Largent portrays the phrase 'eclipse of Darwinism' as a deterministic and Whiggish metaphor that has served the purposes of certain biologists and historians, at the "expense of our understanding of the research, conclusions, and worldviews of early twentieth-century American evolutionists" (2009, p. 7). It constructed the MS as an inevitable, discontinuous and "predictable solution to the problem of darkness" (2009, p. 4) that supposedly had plagued evolutionary biology after the publication of *Origin* (1859). He refers more particularly to Vernon Lyman Kellogg's book *Darwinism To-Day* (1907) which has erroneously been interpreted from the perspective of this metaphor but which, in reality, "heralded the primacy of Darwinian natural selection (...)" (2009, p. 15). The eclipse metaphor is a lingering relic that is harmful to our ability accurately to depict twentieth-century evolutionary biology and should be replaced with "a new term and a new conception of the work done in evolutionary biology between 1880 and 1940, one that analyzes early twentieth-century evolutionary biology on its own terms, not merely in the context of what followed" (2009, p. 18).

As we explained previously (Tanghe et al. 2018), the phrase 'eclipse of Darwinism' is indeed somewhat Whiggish or anachronistic as we can only describe this episode as an eclipse with the benefit of hindsight. It is in several ways very misleading. Firstly, as pointed out above, Darwinism was never the dominant interpretation of evolution (i.e., a paradigm); secondly, it did never completely 'disappear' (there were always Darwinists or ultra-Darwinists); thirdly, the eclipse metaphor ignores the fact that, during the so-called eclipse, Darwinism was one of several competing pre-paradigmatic theories (Junker 2008, p. 496); and, fourthly, Darwinism also didn't reappear unchanged in the twentieth century, like the sun after a literal eclipse. We shouldn't overestimate the negative impact this metaphor has had on modern historians, though. In 1983, Bowler (1983, p. ix) already claimed that his thesis that the eclipse represented a crucial phase in the development of modern evolutionism "has become widely accepted." Still, "pre-paradigmatic phase" seems to us to be a better phrase since Kuhn's characterization of this period is a more accurate description of what evolutionists did between 1860 and 1947 than what is suggested by the phrase 'eclipse of Darwinism' (or by Largent's alternative term "interphase"): they did sometimes important research but within the context of different and quarrelling, pre-paradigmatic schools of thought.

However, in one important way, the pre-paradigmatic phase in the history of evolutionary biology was unlike pre-paradigmatic phases of other sciences: the multidisciplinarity of this science led to a tentative association between pre-paradigmatic approaches of evolution and specific biological disciplines. They each tended to approach evolution from the specific perspective of their own disciplinary specialty, which, as we shall see, highly complicated the construction of the MS. Some Mendelists, for example, were drawn to de Vries' mutationism because they studied the transmission of discrete and clearly defined characters, field naturalists were attracted to Lamarckism because it seemed a good explanation for the adaptive patterns they observed in the field, whereas many paleontologists were adherents of orthogenesis because of the linear patterns they discerned in the fossil record. Put differently: the specific interpretation of evolution of each one of these disciplines was distorted or lopsided by their specific, disciplinary perspective.

The so-called "population genetics approach to evolution" (Michod 1981, p. 3) which interprets evolution in terms of changes in the relative frequency of alleles, can, as already indicated, be considered one of these pre-paradigmatic theories or approaches, even though we normally don't interpret it as such from our modern perspective, since it was this approach of evolution that eventually won the pre-paradigmatic competition.[11]

3.2. The Complex Construction of the MS

The question of how the MS was constructed, remains "one of the most vexing problems in the history of biology" (Smocovitis 1996, p. xii; Amundson 2005, section 8.3), despite the existence of a veritable 'Synthesis Industry' (e.g., Mayr and Provine 1980; Mayr 1982; Smocovitis 1996; Bowler [1989] 2009; Gould 2002). From a Kuhnian perspective, its construction is, like the pre-paradigmatic phase, classic and atypical at the same time.

We already know in what way the genesis of the MS can be called a Kuhnian process: it is based on one of the pre-paradigmatic approaches of evolution (population genetics). However, due to the highly multidisciplinary nature of evolutionary biology, its construction was at the same time also atypical. It is not difficult to understand why: since the victorious evolutionary approach was that of one particular biological discipline, the construction of the MS also had to encompass a synthesis of information from other biological disciplines (hence its name). Theodosius Dobzhansky's *Genetics and the Origin of Species* (1937) started this synthesis process and inspired a small number of other biologists from various disciplines (Huxley 1942; Mayr 1942; Simpson 1944; Rensch 1947; Stebbins 1950) to elaborate a more or less coherent interpretation of evolution (i.e., the MS). This heterogeneous set of books in its turn inspired many other biologists and started a neo-Darwinian research tradition. They clearly constitute an example of a Kuhnian "universally recognized scientific achievement" and at the same time also form yet another reflection of the

11. Another pre-paradigmatic theory that is not always recognized as such is August Weismann's neo-Darwinism.

extreme disciplinary heterogeneity of evolutionary biology: Kuhn's paradigms are, typically, elaborated in single books.[12]

3.3. The Turbulent Normal Science Period

This complex, binary construction of the MS (constriction and synthesis) helps to explain why it has turned out to be somewhat schizophrenic in its performance as a paradigm: functional and dysfunctional at the same time.

3.3.1. A Run-of-the-Mill Paradigm. Above, we saw how the MS facilitated the start of a still ongoing phase of normal science. It is in that Kuhnian light that we can understand a mystery, pondered by John Maynard Smith. He wondered why a major characteristic of evolutionary biology since 1960 has been the attempt to develop Darwinian explanations of seemingly anomalous biological phenomena such as sex, ageing, sexual ornamentation, and, "most important of all, cooperation" (Maynard Smith 1994, p. x; see also Mayr 2004, p. 139). From a Kuhnian perspective, there is no mystery whatsoever: as long as scientists still discuss the fundamental nature of the natural phenomenon that they study (electricity, light, evolution, …), there does not exist much interest in that kind of esoteric puzzles. It is only once a paradigm emerges that they attract widespread attention or that they become the 'main business' of scientists and thus can inspire a crisis and a scientific revolution.

One of the main Kuhnian puzzles in evolutionary biology, next to cooperation, is the existence of sex. In the preface to his *Sex and Evolution* (1975), George Williams even claimed that the prevalence of sexual reproduction in higher plants and animals is "inconsistent with current evolutionary theory" and that, consequently, there was "a kind of crisis at hand in evolutionary biology" (p. v). With his book, he wanted to propose "minimal modifications of the theory in order to account for the persistence of so seemingly maladaptive a character" (p. v). Sex may have been (and still be) the "queen of problems in evolutionary biology" (Bell 1982, p. 19), but it is (contra Williams) not serious enough to have inspired (or inspire) a true, Kuhnian crisis. Rather, it is an example of the "omnipresent concomitant of normal science" that, as we saw, most anomalies are. The problem is not that it cannot be explained but rather that we are still not certain which evolutionary explanation(s) is (or are) correct (see, e.g., Barton and Charlesworth 1998).

As stated above, a paradigm implies "a new and more rigid definition of the field. Those unwilling or unable to accommodate their work to it must proceed in isolation (…)" (Kuhn 1962, p. 19). Entire schools can thus endure in

12. Kuhn refers to "Aristotle's *Physica*, Ptolemy's *Almagest*, Newton's *Principia* and *Opticks*, Franklin's *Electricity*, Lavoisier's *Chemistry*, and Lyell's *Geology*—these and many other works served for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners" (1962, p. 10).

"increasing isolation from professional schools" (Kuhn 1962, p. 19). Examples of such groups are or were adherents of astrology (once an integral part of astronomy) and of "romantic" chemistry (i.e., alchemy). Something similar happened to traditional or "naïve" group selectionists (i.e., adherents of the idea that prosocial behaviors will automatically spread in populations because they benefit groups), once the first generation of biologists, educated in the new paradigm, started their career. The fathers of population genetics (Ronald Fisher, John B. S. Haldane, and Sewall Wright) had each considered the question of group selection or multilevel selection, but only briefly. Consequently, group selection did not really form an integral part of the original MS. In the wake of its emergence, individual selection even became the norm. As Borrello puts it:

> Essentially, the neo-Darwinian paradigm held that all evolution is due to the accumulation of small genetic changes, guided by natural selection. The theory of group selection clearly did not fit within this definition, and the era of peaceful coexistence came to a decisive end. (2005, p. 45)

Ironically, V. C. Wynne-Edwards (1962), who is generally seen as one of the main modern proponents of the idea of naïve group selection, was also inspired by the MS (Borrello 2003): he saw in its focus on populations a way of thinking about evolution that was in line with his own research interests (in populations and population regulation). It is particularly through reactions to his work that individual-level adaptation or selection was turned into "a core concept of the modern synthesis" (Borrello 2003, p. 531) and group selection into something of a (non-paradigmatic) heresy (to many evolutionists, it still is suspect or of little importance, in spite of its resurrection, in recent decades, see, e.g., Borrello 2005). Within-group (individual) selection was assumed to be almost always stronger than between-group selection. For example, in a letter in *Nature*, entitled "Group Selection and Kin Selection" (1964), Maynard Smith directly challenged group selection. "I will contrast group selection," he wrote, "with something I will call kin selection. Kin selection has been treated by Haldane and Hamilton" (quoted in Segerstraele 2001, p. 63).

Likewise, in the preface to the 1996 edition of *Adaptation and Natural Selection: A Critique of Some Current Evolutionary Thought* ([1966] 1996), Williams refers to "what seemed to be a pervasive inconsistency in the use of the theory of natural selection" (p. ix). Adaptive changes are, in theory, only caused by the selection of individuals. However, in practice, many biologists continuously spoke of group adaptations, which evolved through group selection and required individual organisms to forgo their own interests for the greater good of the group or the species. A lecture by the renowned ecologist and termite specialist A. E. Emerson about beneficial death, the idea that senescence evolved for the benefit of the species, which Williams attended while on a teaching fellowship at the University of Chicago, was the "triggering event" (p. ix). If that was acceptable biology, he rather wanted to become an insurance salesman. In the introduction, he wrote that, with some minor qualifications, "it can be said that there is no escape from the conclusion that natural selection, as portrayed in elementary texts and in most of the technical contributions of population geneticists, can only produce adaptations for the genetic survival of individuals" (pp. 7–8).

Dawkins, who was "greatly influenced" by "Williams's great book" (Dawkins 1976, p. 11) wanted with his own more popular book *The Selfish Gene* (1976) to "examine the biology of selfishness and altruism" (p. 1)—i.e., propound recently published ideas about the evolution of social and particularly altruistic behaviors—, and to falsify the "erroneous assumption" (p. 2) behind books like Konrad Lorenz' *On Agression* (1966) and Robert Ardrey's *The Social Contract* (1970) "that the important thing in evolution is the good of the *species* (or the group) rather than the good of the individual (or the gene)." The reason why he sounded so "evangelist" in *The Selfish Gene*, he later explained (Dawkins 1979), is that he wanted to rebut the still popular group selection mechanism by bringing in gene selectionism as an alternative.

3.3.2. Not-So-Normal Normal Science. In some ways, though, evolutionary biology was and/or is an atypical paradigmatic science. For example, its professionalization proceeded surprisingly slowly (Antonovics 1987).[13] Also, the MS was soon challenged by an ever-increasing plethora of alternative evolutionary models (Huneman and Walsh 2017). Or, as Bowler puts it: the founders of the MS, who had hoped that their paradigm would inspire a long period of what Thomas Kuhn called normal science, "were to be disappointed. The diversity of new theories has never matched that of the 'eclipse of Darwinism', but it has been possible to articulate divergent perspectives within the narrower frame of reference established by the synthesis" ([1989] 2009, p. 347). Evidently, not all proponents of those 'divergent perspectives' would agree with that last claim. Some scholars even believe that the original MS has ceased to exist and that "there is no longer a single, unifying 'Darwinian evolutionary theory'" (Kutchera 2013, p. 544).

In any case, as we explained elsewhere in more detail (Tanghe et al. 2018), this unusual degree of disagreement among modern evolutionists can, once again, only be explained by the extreme multidisciplinarity of evolutionary

13. Antonovics (1987) even believes that the MS "had little direct effect on the progress of evolutionary biology as a discipline (…)" (p. 321). After the foundation of the journal *Evolution: An International Journal of Organic Evolution* (1946), no other evolutionary journals appeared until the 1970s; for a long time, textbooks were absent and evolutionary biology was, as late as 1987, rarely thought of as a discipline in its own right (i.e., there was a scarcity of international congresses, departments or university programs and institutional organizations, exclusively dedicated to evolution, as well as a painful lack of funding). See, in this respect, Ruse (2009a).

biology. It not only, and evidently, sharply increases the chance of disagreements (among practitioners of various evolutionary disciplines) but was, through the complex construction of the MS, also antecedent to four problematic and contentious features of this paradigm. First, since the approach of evolution that became the basis of the MS was that of one evolutionary discipline (population genetics), the MS was inevitably somewhat lopsided (i.e., quite gene-centric). As aforementioned, it even identified evolution with its genetic dimension and, more particularly, with changes in allele frequencies. Dobzhansky (1937, p. 11) put it as follows: "Since evolution is a change in the genetic composition of populations, the mechanisms of evolution constitute problems of population genetics."

Furthermore, the additional synthesis of data and concepts from a variety of other biological disciplines around this genecentric approach of evolution led to two other problems. That synthesis was not only incomplete—or, as Eldredge (1985) put it, unfinished—but also imperfect. It was incomplete because not all life sciences (fully) participated in the synthesis—some of course only came into being after the establishment of the MS—and it was imperfect because of two related reasons: practitioners of several biological disciplines believed that their field should have made a more substantial contribution to the MS and the MS did not resolve the old, pre-paradigmatic conflict between an organism-focused and a gene-focused approach to the study of (evolving) life (Mayr 1982, pp. 540–50), as soon became painfully clear in the comments of some of its main architects (see, e.g., Mayr 1959, p. 13). Lastly, due to its convoluted construction (a lopsided constriction, followed by a problematic synthesis), the MS is also surprisingly fuzzy, as has often been pointed out: it is a 'moving target' and meant and means different things to different people and, particularly, to practitioners of different biological disciplines (see, e.g., Craig 2015, p. 255).

This brings us back to the EES. As we pointed out above, only a minority of its proponents sees it as a potential new paradigm. It can be conceived as an alternative Lakatosian research program but it can also, and maybe more appropriately, be interpreted as a classic, Kuhnian reformulation of the modern, extended MS by more organism-focused biologists. This interpretation is corroborated by Müller's (2017) remark that the new way of thinking about evolution of the EES is historically rooted in the not-so-new "organicist tradition" (p. 8). The EES is, in any case, at least partly inspired by a strong aversion, among organism-focused biologists, for the baked-in, greedy gene-centrism of the MS. This is the theoretical problem that they want to solve through their reformulation.

Laland et al., for example, argue that "important drivers of evolution, ones that cannot be reduced to genes, must be woven into the very fabric of evolutionary theory" (2014, p. 161). Genes are not causally privileged as programs or blueprints, but are rather "parts of the systemic dynamics of interactions that mobilize self-organizing processes in the evolution of development and entire life cycles" (Müller 2017, p. 7). Also, organism-focused biological disciplines like Laland's ethology or Müller's developmental biology and the somatic, ecological and epigenetic phenomena that they study, occupy a much more important place in the EES than they do in the MS. The current friction between the MS- and the EES-community can, in this sense, be seen as a continuation or resurgence of the old conflict between an organism-focused and a gene-focused approach to the study of (evolving) life.

4. Why Was This Kuhnian Analysis Not Done Before?

If, as we have argued, Kuhn's developmental model of sciences is partly discernible in the history of evolutionary biology and if a Kuhnian analysis of that history is heuristically interesting or rewarding, why then has this kind of analysis not been done before? We already know one possible reason: there doesn't seem to be place in Kuhn's scheme for the publication of *On the Origin of Species* (1859) (4.1). Furthermore, a Kuhnian analysis of the history of evolutionary biology also constitutes an unusual application of *The Structure* (4.2) and an odd, philosophical kind of historiography to boot (4.3). This multifaceted explanation involves a deep dive in the Kuhn literature but much of that more esoteric information will be discussed in extended footnotes and can be ignored by readers who are only interested in the gist of this section.

4.1. On the Origin of Species (1859): A Kuhnian Anomaly

Several scholars have investigated whether *On the Origin of Species* (1859) can be considered an example of a Kuhnian revolution.[14] The standard result of these studies is almost invariably negative: Charles Darwin's theory of evolution may have been hugely successful and influential but most scholars do not believe that it caused a Kuhnian revolution.[15] This is corroborated by the present anal-

14. Kuhn seems to have believed so. In his preface, he points out that "Far more historical evidence is available than I have had space to exploit below. Furthermore, that evidence comes from the history of biological as well as of physical science. My decision to deal here exclusively with the latter was made partly to increase this essay's coherence and partly on grounds of present competence" (Kuhn 1962, p. xi). However, in later chapters, he does refer to evidence from the history of biology. For example, he refers to parts of biology "—the study of heredity, for example—" where "the first universally received paradigms are still more recent (...)" (p. 15). In various places, he also refers to Darwin and/or *On the Origin of Species* (1859) (Kuhn [1962] 1970, pp. 20, 151, 171–2, 180). It seems clear that he considered the emergence of the idea of evolution through natural selection to be an example of a scientific revolution (see also table 1 in Wray 2011, pp. 18–19).

15. Greene ([1971] 1981, p. 31), for example, examines "the developments leading up to the Darwinian revolution in natural history to see to what extent they fit the pattern of historical development described in Kuhn's book." His conclusion is that the Darwinian

ysis as it implies that *On the Origin* merely initiated the pre-paradigmatic phase in the history of evolutionary biology and simply does not fit within Kuhn's original scheme (since that scheme doesn't mention the various ways in which pre-paradigmatic phases in the history of scientific disciplines start).

This implication of our analysis, in turn, is perfectly in line with yet another strand in the Kuhn literature, namely the criticism that his developmental scheme does not "capture some crucial episodes in the history of science (…)" (Politi 2018, p. 2284).[16] Bird refers in this respect to the discovery of the structure of DNA. It was "clearly revolutionary in [its] consequence for

revolution "overthrew the static view of nature and natural history but failed to establish a clear-cut paradigm in its place" (p. 54), and that "nothing approaching a 'Darwinian' paradigm became established until the 1930s (…)" (p. 53). He set the pattern for subsequent studies. For example, Mayr (1972, p. 988) claims that "the Darwinian revolution does not conform to the simple model of the scientific revolution, as described, for instance, by T. S. Kuhn" (see also Mayr 1994, 2004, p. 165; Wilkins 1996, p. 696; Hodge 2005; Smocovitis 2012). Ruse has developed a somewhat more nuanced view. Kuhn's analysis taken head on "fails on the Darwinian example" (Ruse 2005, p. 13) but in some ways, it is legitimate to call the publication and success of *On the Origin* (1859) a (Kuhnian) revolution. Darwin's notion of evolution through natural selection was paradigmatic and constituted a different world view (Ruse 1989). There were "different paradigms if you will" (Ruse 2009b, p. 10045), "but note that it is not just a question of evolution or not evolution, and certainly not of selection or not selection." There is also a kind of incommensurability between functionalism (which stresses the function of organic structures) and formalism (which emphasizes the form of organic structures). Darwin was a functionalist but Thomas H. Huxley, for example, was a hardline formalist: "I would argue that in a real sense we have Kuhnian paradigm differences operating here. Different visions, unable to bridge the gap" (Ruse 2009b, p. 10045), even though this sense of paradigm "does not fit exactly with the senses of paradigm found in the *Structure of Scientific Revolutions*" (Ruse 2009b, p. 10045). Lastly, the philosopher Massimo Pigliucci (2012) believes that the only time in the history of biology when a Kuhnian transition occurred was "when Darwin's original theory replaced the dominant 'paradigm' of the day, Paley-style natural theology" (p. 46) although this "paradigm shift did not really happen within the confines of an established science" (p. 56) (see also Pigliucci 2009). This is indeed correct and also highly problematic as genuine paradigm shifts per definition happen within the confines of an established or mature science (since the creation of a paradigm is the hallmark of a mature science).

16. One of three traditional criticisms of Kuhn's central distinction between normal science and revolutionary science is that these two categories "fail to provide us with the conceptual resources necessary to understand the variety of changes that occur in science" (Wray 2011, p. 24). The other two criticisms are that the various changes that Kuhn regards as revolutionary (1) are a mixed lot and do not belong in one and the same category and (2) are not different in kind from changes that occur during the normal science phase (see Wray 2011, pp. 21–4; see also Casadevall and Fang 2016). Wray (2011, pp. 29–33) defends Kuhn's later, revised account of scientific revolutions against these criticisms. In this analysis, Kuhn doesn't refer to paradigms anymore but to conceptual taxonomies or lexicons. Thus, he came full circle as he first had assigned the functions of a paradigm to "language and predispositions" (Reisch 2016, p. 23). For a discussion of the applicability (or non-applicability) of that revised account to the history of biology, see Keller (2012).

biochemistry and molecular genetics" but "simply does not fit Kuhn's description of scientific development—it originated in no crisis and required little or no revision of existing paradigms even though it brought into existence major new fields of research" (Bird 2000, p. 60).

We agree with Politi that the discovery of the structure of DNA was, contra Bird, *not* revolutionary in its consequence for molecular genetics "simply because, before such a discovery, there was not such a thing as 'molecular genetics'" (Politi 2018, p. 2284). Rather, Watson's and Crick's discovery was revolutionary "precisely in virtue of its role in the creation and establishment of molecular biology (…)" (Politi 2018, p. 2284). Likewise, the discovery of the deep history of Earth (Rudwick 2005), subatomic particles, the true nature of viruses (Méthot 2016) and evolution was revolutionary in virtue of the role it played in the establishment of geology, quantum mechanics, virology and evolutionary biology, respectively.[17] Consequently, scientific discoveries can be revolutionary because of the impact they have within an established science or because of the seminal role they play in the emergence of a new science.

Undoubtedly, the aforementioned fact that Kuhn treats the period in the history of a science that precedes the emergence of a paradigm and the normal science phase (annex scientific revolutions) in a somewhat stepmotherly fashion, is an important reason why he did not foresee a separate phase or specific place in his developmental scheme for these important, primordial occurrences in the history of some sciences. When, in his later work and in the context of his evolutionary philosophy of science (Marcum 2015, ch. 6),

17. Evidently, Earth was already empirically studied before the discovery of its deep history but that was not geology in the modern meaning of the word. It is only when it became clear that Earth has a deep history that the scientific study of that long and convoluted history (i.e., geology) could start in earnest (Rudwick 2005). As we saw in note 3, Kuhn (1962, p. 15) refers in this respect to the work of the Scottish geologist James Hutton and portrays him as the father of historical geology. See, however, Rudwick (2005, p. 172): "A sense of the *history* of the earth, whatever its source may have been, certainly did not come from Hutton." Likewise, evolutionary biology could only emerge as a new scientific field once Darwin put the notion of evolution on the late-nineteenth century scientific agenda. Of course, the idea that populations can undergo certain transmutations was centuries old. By contrast, the idea of evolution (i.e., life as we know it has developed from a very simple beginning) only dates from the 18th century. By scientifically consolidating that hitherto vague, unorthodox and philosophical notion, *On the Origin* played, without a shred of a doubt, a crucial role in the emergence of the science of evolutionary biology. It is not only a milestone in the history of (evolutionary) biology but maybe even a unique achievement in the history of science as it, at the same time, also identified the mechanism behind adaptive evolution (i.e., natural selection). Also, it was 'consilient'—i.e., it connected and explained data from a wide array of areas (Ruse 1975)—, reshaped the way we see the world (Langdon 2016) and inspired the concrete research of some biologists or naturalists (Bowler 1996). That, however, is not enough to assign it the status of (new) paradigm. It merely marked the true start of the history of evolutionary biology and subsequently became an important source of inspiration for the MS.

he finally did devote special attention to the birth of new sciences, he focused on specialization or specialty formation (the equivalent of biological speciation, see Wray 2011, ch. 7; Nickles 2017, sections 4 and 6.2; Politi 2018).[18] In his Rothschild lecture, he even seemed to replace the notion of scientific revolutions with the notion of 'scientific speciation'. The episodes that "he once described as scientific revolutions," he declared, "are intimately associated with (…) speciation" (Kuhn, cited in Nye 2012, p. 561). Also, as we shall see in the next subsection, his developmental scheme wasn't even meant to be complete or perfect.

4.2. An Unorthodox Use of *Structure*

There are at least three reasons why an interpretation of the history of evolutionary biology through the prism of Kuhn's historical developmental model constitutes an unusual way of applying *The Structure* to the study of science. To begin with, that model was a "highly schematic sketch" (Kuhn, cited in Sigurdsson 2016, p. 27) that wasn't meant to be applied this way:[19]

> I've always said, assimilate this point of view and this way of doing it, and then see what it does for you when you try to write a history, but

18. In *Structure* (1962, p. 169), Kuhn had already suggested that increasing specialization plays an important role in the historical development of sciences but he saw it as a consequence of scientific progress. Later, he regarded specialization as the *cause* of scientific progress and emphasized its importance as an alternative for scientific revolutions within existing sciences, although he never made this idea the focus of a specific paper or book (Wray 2015, pp. 177–80). Nickles (2017) points out that he still gave little attention to the reverse process of hybridizations between various fields. Also, some sciences originated partly or completely in pre-scientific practices (particularly natural history and natural philosophy).

19. As Hacking (2012, p. xi) puts it: Kuhn "thought of *Structure* as more of a book outline than a book." *Structure* originated as a contribution to the *International Encyclopedia of Unified Science*. Therefore, Kuhn presented his views "in an extremely condensed and schematic form" (Kuhn 1962, pp. xlii–xliii). In what seems to have been the first version of the first chapter of *Structure*, "Kuhn reckoned the monograph would have only five chapters (and about eighty pages, according to the editors' suggestion)" (Pinto de Oliveira 2017, p. 748). Unfortunately, he never wrote a longer version of his theory. Instead, in his later work, he focused on a few components of his 1962 sketch, mainly the notion of paradigms, the notion of incommensurability, and the passage about evolutionary epistemology (Renzi 2009): *Structure* truly "continued to dominate Kuhn's thinking until the end of his life (…)" (Marcum 2015, p. 53). As Hoyningen-Huene (2015, p. 185) puts it: "A really comprehensive account of these development (sic) remains to be written (…)." Kuhn's own account, *The Plurality of Worlds: An Evolutionary Theory of Scientific Development*, remained unwritten (see Hoyningen-Huene 2015). Holton (2016, p. 35) believes that "he was not able to publish the work. And that, in my view, was a chief source of Tom's internal state of dismay, especially in his last decade, as he was trying to reach the new, high professional identity level he had set for himself." This maybe also explains why the one surviving possible editor of the manuscript, designated by Kuhn, James Conant, still announces the work as "soon to be completed," a quarter of a century after Kuhn passed away. See: https://philosophy.uchicago.edu/faculty/conant.

> don't go out looking at history to see whether this is true or false, to test the ideas. The only test of the ideas, at least at this level of development, is going to be whether having assimilated those ideas, you see the material usefully different. But it's not going to be "Can you always locate the paradigm, can you always tell the difference between a revolution and a normal development?" It's not meant to be applied that way. (Kuhn, cited in Sigurdsson 2016, p. 27)

What did Kuhn mean by "this point of view and this way of doing" history of science? With *Structure*, he wanted to contribute to a "historiographic revolution in the study of science, though one that is still in its early stages" (Kuhn 1962, p. 3; see also Pinto de Oliveira 2020). In the introduction, he pointed out that the point of departure of the traditional, Whiggish history of science was the constellation of facts and theories in modern science textbooks. The history of science then constituted of the piecemeal and gradual addition of items to this modern constellation and the main task of historians was to determine when each item had been added and by whom. Science was compared to "an ever-growing edifice to which each scientist strives to add a few stones or a bit of mortar" (Kuhn, cited in Pinto de Oliveira 2017, p. 748). By contrast, according to the revolutionary new image of the history of science that he wanted to help elaborate, science did "not progress by adding stones to an initially incomplete structure, but by tearing down one habitable structure and rebuilding to a new plan with the old materials and, perhaps, new ones besides" (Kuhn, cited in Hufbauer 2012, p. 459).[20]

He wanted to contribute to this new image in two different ways. The first way was by elaborating a non-Whiggish developmental model of sciences and thus promoting an non-Whiggish way of doing history of science. The reason why scientific revolutions occupied a central place in that model is of course that they were at the heart of the new, non-gradual image of science (although it should immediately be added that Kuhn believed that most science is of the 'normal', non-revolutionary kind).[21] In what seems to have been the first

20. He had first come into contact with that "new image" in 1947, as a graduate student in physics. That was the year when, at Bernard Cohen's recommendation, he read Alexandre Koyré's *Études Galiléennes* (1939). Koyré was the scholar who Kuhn deemed, more than any other, "responsible for (…) the historiographical revolution" (Kuhn 1962, p. 67). For Koyré's significant influence on Kuhn, see, e.g., Omodeo (2016). 1947 was also the year that James B. Conant, president of Harvard University, invited Kuhn to become an assistant for his course on the history of science for upper-level undergraduates. For the special relationship between Kuhn and Conant (and the Cold War political/ideological background of *Structure*), see, e.g., Fuller (2000), Reisch (2016) and Omodeo (2016).

21. As Bird (2000, p. 33) puts it: "It is appropriate then that despite the title, *The Structure of Scientific Revolutions*, half of that book does not concern revolutionary science at all." One can indeed state that Kuhn's title is misleading. Scientific revolutions may be the

draft of the first chapter of *Structure*, he even introduced his main argument by making a comparison with revolutions in the history of art.[22] This is a second reason why our application of *Structure* is unusual: most applications have focused on the question whether Kuhn's all-important description of the structure of scientific revolutions fits a particular revolution (e.g., the Darwinian revolution, the chemical revolution or the plate tectonics revolution), rather than on the question whether a particular science follows his developmental scheme in its historical development (see Marcum 2015, ch. 7).

A second goal of Kuhn was to delineate the philosophical consequences of the new image of science for he believed that the old, positivist philosophy that had been associated with the Whiggish, development-by-accumulation historiography had to be replaced with a new philosophy of science.[23] He even thought of *The Structure of Scientific Revolutions* (1962) "as addressed primarily to philosophers" (Kuhn 1993, p. xii).[24] It is also generally deemed to

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quintessential part of *Structure* and many of its chapters (5 out of 13) are indeed devoted to their analysis, but his title does, nevertheless, not reflect very well the true purpose and content of his book. Consequently, we don't entirely agree with Hacking's claim that "Structure and revolution are rightly put up front in the book's title" (2012, p. x). Nevertheless, it may have contributed to its success. See, in this respect, Gavroglu (2016). He believes that Kuhn's title, and more particularly "what 'happened' to" the words 'science' and 'revolution' was "absolutely decisive for the book's success" (p. 51) outside academia. See also Nye (2016).

22. See Pinto de Oliveira (2017). It differed significantly from the first chapter of *Structure*, though. The title was: 'What are Scientific Revolutions?'. He eventually abandoned the art/science analogy (as a central element of his argument) because he was afraid he knew too little of art as activity. That is a shame because as Kuhn ([1969] 1977, p. 340) put it himself: *Structure* must be seen as a "belated product" of his "discovery of the close and persistent parallels" between science and art. Pinto de Oliveira (2017, pp. 758–60) discusses two passages in *Structure* that highlight this analogy. Kuhn also referred to it in the postscript to the second edition of *Structure* ([1962] 1970, p. 208) (see also Kuhn [1969] 1977). It helps to explain why his book was so influential outside the domain of the history and philosophy of science. As Pinto de Oliveira puts it: "Kuhn gives back to the other disciplines, with 'added value', the idea of development with breaks" (2017, p. 761). The analogy also seems to have influenced Feyerabend, "the other main name of the contemporary historically oriented philosophy of science (…)" (Pinto de Oliveira 2017, p. 762). It is probably no coincidence that it was also the two fathers of this analogy, Kuhn and Feyerabend, "who put the word *incommensurable* on the table" (Hacking 2012, p. xxx).

23. As late as 1991, Kuhn pointed out, in his Robert and Maurine Rothschild Distinguished Lecture ("The Trouble with the Historical Philosophy of Science") that "the problem with the historiographic revolution was that it was unable to provide a philosophy of science to replace the one it demolished and to account for the growth of scientific knowledge" (Kuhn, cited in Marcum 2015, p. 25).

24. He admitted, though, that "limitations of space have drastically affected my treatment of the philosophical implications of this essay's historically oriented view of science" (Kuhn 1962, p. xliv). This also explains why his later attempts to develop it further were directed "exclusively to the book's philosophical aspects" and particularly to the "underpinnings of incommensurability (…)" (Kuhn 1993, p. xii). For Kuhn's philosophical goals, see Bird (2012a).

have been more influential among philosophers than among historians (Reingold 1980; Cohen 1985, p. 27; Wray 2012; Bird 2012a; Marcum 2015). This is a third and last reason why a Kuhnian interpretation of the history of evolutionary biology can be said to be unusual or unorthodox.[25]

4.3. An Odd Kind of Historiography

Of course, that does not mean that our historiographical use or application of Kuhn's historical developmental model is completely new. It has, for example, already been pointed out that it "fits the history of the development of geology quite well" (O'Hara 2018, p. 258). Frankel (1978), for example, interprets the history of geology through the prism of Kuhn's model. In a similar vein, Melchert observes (or hopes) in a paper, entitled "Leaving behind Our Preparadigmatic Past: Professional Psychology as a Unified Clinical Science" that "[t]he regular introduction of new theoretical orientations was a main feature of the pre-paradigmatic era in psychology, but that era has come to an end" (2016, p. 21).

Unfortunately, though, scholars who investigate whether Kuhn's historical developmental model can be discerned in the history of a specific science, often misunderstand it. Méthot, for example, points out that the science of virology "developed its own paradigm in the mid-twentieth century by breaking with bacteriology, along the lines van Helvoort suggested" (2016, p. 7). This was preceded by "deep controversies in virus research in the first half of the twentieth century" (2016, p. 7) and followed by a period of normal science. However, he does not seem to understand that this implies that the science of virology broadly followed Kuhn's developmental model (pre-paradigmatic phase, paradigm, normal science) since he next wonders whether this process

25. It should be added, though, that *Structure* was highly instrumental in the historical turn in philosophy of science. As Michael Friedman (1993, p. 37) put it: since the publication of *Structure*, "careful and sensitive attention to the history of science must remain absolutely central in any serious philosophical consideration of science" (see also Bird 2000, p. viii). As a matter of fact, Kuhn was professor of history of science at Berkeley when he published *Structure*, even though he taught in both the philosophy and the history department. Most of Kuhn's own publications before 1962 were in the history of science. In 1968, six years after the publication of *Structure*, he still called himself "a practicing historian of science (…) I am a member of the American Historical, not the American Philosophical, Association" (Kuhn, cited in Hacking 2012, p. x). This explains why, of the 150 footnotes in the first edition of *Structure*, only 13 include references to philosophers, the vast bulk of the other references are to historians (Bird 2000, p. x). To be precise, sixty percent of the sources (76/127), cited in *Structure*, and ninety percent of the most cited sources, are works in the history of science (Wray 2015). There are only 13 citations of philosophical sources. *Structure* also became "the most widely read book on the history of science" (Blum et al. 2016, p. 1) and thus "the second major milestone, after the first publication of the journal *Isis* in 1912, to mark the rise of the history of science to a field enjoying broad recognition beyond the narrow community of its practitioners" (Blum et al. 2016, p. 1).

(i.e., the establishment of virology as a mature science) can "really be called a 'revolution' (…) in the Kuhnian sense?"[26]

A second, and more fundamental problem, associated with using Kuhn's historical developmental model in the study of the history of specific sciences, is that this is very much at odds with the modern historiography of science. As Arabatzis (2016, p. 196) puts it, "Kuhn's grand narrative of scientific development was not well received by historians of science (…)" (see, e.g., also Kitcher 2012, pp. 532–3). Daston explains why: the very idea of "looking for overarching regularities in the history of science seems bizarre (…)"(2012, p. 498). And: "Since roughly the 1990s, the focus (…) has shifted from the streamlined to the dense and detailed; the professed aim has been to 'complexify' rather than simplify and to reveal variability rather than uniformity" (2012, p. 498). That is why "Most historians of science no longer believe that *any* kind of structure could possibly do justice to their subject matter" (2016, p. 117).

Ironically, this is partly due to the influence of *Structure*.[27] Its publication was "a signal event leading to the widespread rejection of presentism in the history of science" (Kremer 2013, p. 301) and "the starting point for a long conversation that contributed to the view held by many historians of science today, that science is a historically rooted and culturally embedded set of practices" (Smocovitis 2012, p. 569). Nye (2012, p. 560) speaks of a shift, particularly in the decades after the publication of *The Structure*, to the study of the history of "the fine structure of scientific practice and scientific ideas (…)." Kuhn had always been "unwavering in his conviction that the future of the history of science lay in history departments. (…) It would become historicist, in the sense of situating science in its specific context, just as the history of art (…) embedded styles and genres in particular periods and places" (Daston 2016, pp. 117–118).[28] It is in this sense that he left a

26. See, for example, also Weimer and Palermo (1973) or Bowler (1983, p. 11). Bowler calls *Structure* the best-known attempt to create a "general scheme describing the emergence of a new theory," sketches the emergence of a paradigm and its replacement with a new theory and subsequently points out that there has already been some debate wether the Darwinian revolution fits this pattern.

27. As Findlen puts it, "there is an ironic coda to the inspiration that Kuhn offered to pioneers of microhistory by writing a grand narrative, in light of the function of anomalies in destabilizing the explanatory power of prevalent scientific theories. He inadvertently played a role in inspiring microanalysis as a means of rethinking commonly received historical narratives. By the 1990s the *petite histoire* had replaced the *longue durée*" (2005, p. 233). For an investigation of the possibility of reconstructing new 'big pictures' from the mass of modern, microhistoriographical detail, see Secord (1993).

28. However, as Daston (2016) also points out: this conviction stood in sharp contrast with his "search for structures" (p. 124). The historicisation of the history of science also

"remarkably substantial" (Blum et al. 2016, p. 2) legacy in the history of science or a "stronger" legacy than sometimes now claimed (Nye 2016, p. 292).[29]

By thus helping to push the history of science in an externalist, historicist and *petite histoire* direction, *Structure* and its developmental model became, to modern historians, a relic of a foregone era. Arabatzis (2016) categorizes it as a contribution to "the philosophy of history of science" (p. 192), an endeavor which "has been relatively neglected" (p. 196) by modern historians (see, e.g., also Bird 2000, pp. viii, 29, Bird 2012b; Shapin 2015, p. 11). Bird calls it a work in "theoretical history" (2000) or in the "philosophy of the history of science" (2012b). Consequently, it is in two different ways philosophical (but also historical): it belongs to the archaic field of the philosophy of history of science but also aimed at developing a new philosophy of science (a field that it historicized). It is no wonder, then, that, ever since 1962, scholars have struggled to categorize *Structure* and that even Kuhn (1993, p. xii) himself was "often at a loss for response," when asked what field his book dealt with.

5. Conclusion

There are important reasons why the here presented Kuhnian analysis of the history of evolutionary biology hasn't been done before. Firstly, the match between Kuhn's model and that history is imperfect; secondly, it is, for several reasons, an unusual way of applying Kuhn's theory to the study of (a) science; and lastly and maybe most importantly, modern, complexifying, and *petite histoire* (as opposed to 'big history') historians of science are generally not interested anymore in the search for overarching regularities in the history of science (to the extent that they are still interested in exploring its internal history at all).

It cannot be denied, though, that Kuhn's developmental scheme is to a certain extent discernible in the history of evolutionary biology and that a Kuhnian interpretation of that history yields very intriguing insights. It sheds somewhat new light on *On the Origin of Species* (1859), gives us a better and less misleading term for the so-called 'eclipse of Darwinism', and particularly provides us with a

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resulted in the denial, by historians, of the splendid isolation or autonomy which Kuhn saw as a defining characteristic of science. On Kuhn's internalism, see Bird (2012b).

29. For concrete examples of the sometimes-subtle ways in which Kuhn inspired historians, see Buchwald (2016) and Rudwick (2016). *Structure* also transformed a growing concern for the Scientific Revolution of the seventeenth century into "a research program directed toward individual small-scale revolutions in the sciences" (Cohen 1985, pp. 403, 23; see also Daston 2012, p. 497). Thanks to Kuhn, historians also learned to appreciate the 'losers' of scientific revolutions (they often resisted new paradigms for good reasons) and the tradition, tacit knowledge and rigid forms of training that are antecedent to the practice of science (Arabatzis 2016, p. 196).

new way of thinking about the genesis and nature of the MS, about the reasons for the chronic discontent with this paradigm and about the epistemic status of the EES. Our Kuhnian, comparative analysis has also revealed that the extreme multidisciplinarity and heterogeneity of the science of evolutionary biology is an underestimated *fil rouge* and complicating factor in its rich and complex history. In conclusion and in answer to the question that heads this paper: interpreting that history through a Kuhnian prism may be somewhat unorthodox but it most definitely makes sense.

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