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Scientific revolutions, specialization and the discovery of the structure of DNA: toward a new picture of the development of the sciences

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Abstract In his late years, Thomas Kuhn became interested in the process of scientific specialization, which does not seem to possess the destructive element that is characteristic of scientific revolutions. It therefore makes sense to investigate whether and how Kuhn's insights about specialization are consistent with, and actually fit, his model of scientific progress through revolutions. In this paper, I argue that the transition toward a new specialty corresponds to a revolutionary change for the group of scientists involved in such a transition. I will clarify the role of the scientific community in revolutionary changes and characterize the incommensurability across specialties as possessing both semantic and methodological aspects. The discussion of the discovery of the structure of DNA will serve both as an illustration of my main argument and as reply to one criticism raised against Kuhn—namely, that his model cannot capture cases of revolutionary yet non-disruptive episodes of scientific progress. Revisiting Kuhn's ideas on specialization will shed new light on some often overlooked features of scientific change.

Keywords Thomas Kuhn · Specialization · Scientific revolutions · Incommensurability · Molecular biology

1 Kuhn on specialization

In *The structure of scientific revolutions* (Kuhn 1996 [1962], from now on *SSR*), Kuhn describes the historical development of science as being characterized by occasional disruptive episodes, called scientific revolutions. The majority of philosophers

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who have either praised or challenged Kuhn's views has focused almost exclusively on *SSR*. With the remarkable exception of Hoyningen-Huene (1993), for a long time, philosophers have not paid enough attention to Kuhn's post-*SSR* works—almost as if, after *SSR*, he had nothing interesting to say, or just nothing else to say. In reality, Kuhn clarified and even reformulated, in significant ways, some of his early views, in a number of papers published in the 1980s and 1990s (some of which are collected in Kuhn 2000c). Only recently have philosophers started to analyze Kuhn's more mature philosophy and assess his post-*SSR* thought (Andersen et al. 2006; Kuukkanen 2008; Wray 2011).

One of the issues Kuhn begins to explore in his late writings is the phenomenon of *scientific specialization*, that is, the proliferation of "cognitive specialties or fields of knowledge" (Kuhn 2000c, p. 97). New specialties emerge by splitting from one 'parent-discipline' or through the convergence towards an apparent area of overlap between multiple disciplines. Although the second type of specialty formation looks like an instance of unification, rather than specialization, Kuhn explains that the specialty created from more disciplines does not represent the actual unification of its parent-disciplines—which, in fact, continue to persist independently—but is a separate discipline, with its own domain and methodology.

> Either a new branch has split off from the parent trunk as scientific specialties have repeatedly split off in the past from philosophy and from medicine. Or else a new specialty has been born at an area of apparent overlap between two pre-existing specialties, as occurred, for example, in the cases of physical chemistry and molecular biology. At the time of its occurrence this second sort of split is often hailed as a reunification of the sciences, as was the case in the episodes just mentioned. As time goes on, however, one notices that the new shoot seldom or never gets assimilated to either of its parents. Instead, it becomes one more separate specialty, gradually acquiring its own new specialists' journals, a new professional society, and often also new university chairs, laboratories, and even departments (Kuhn 2000c, p. 97).

Kuhn's view of specialization can be said to be both descriptive and prescriptive. On the one hand, scientific specialties proliferate as a matter of fact: "[t]he point is empirical and the evidence, once faced, is overwhelming: the development of human culture, including that of the sciences, has been characterized by a vast and still accelerating proliferation of specialties" (Kuhn 2000c, p. 250). On the other hand, Kuhn regards the proliferation of new specialties as an essential process for increasing the problem-solving power of science: the more specialties there are, the more the general scientific enterprise increases its breadth. Therefore, science (as a whole) ought to aim at the proliferation of narrowly specialized disciplines. In short, "[p]roliferation of structures, practices, and worlds is what preserves the breadth of scientific knowledge; intense practice at the horizons of individual worlds is what increases its depth" (Kuhn 2000c, p. 250).

The creation of a new specialty is a process of isolation: only if scientists focus on a narrower domain, without being distracted by the problems pertaining to the parent and neighboring disciplines, can the new specialty progress. For Kuhn, such a process of isolation is driven by a type of incommensurability. In his view, therefore, specialty-incommensurability plays a positive, ‘generative’ role: it is thanks to incommensurability that the newly formed group of specialists becomes more and more segregated from the pre-existing discipline(s).

The phenomenon of specialization does not seem to have the same ‘destructive character’ of a scientific revolution. While the latter represents a rupture with the scientific tradition, the emergence of a new specialty does not discard its parent-discipline(s). It therefore makes sense to investigate how, and whether, Kuhn’s insights about specialization are consistent with, and actually fit, his model of scientific progress through revolutions.

The aim of this paper is to revisit Kuhn’s ideas in order to develop a more robust view on scientific specialization and to shed new light on some often overlooked features of scientific change. In Sect. 2, I argue that specialization and revolutions are not two different kinds of scientific change, since the transition toward a new specialty corresponds to a revolutionary change for the group of scientists involved in such a transition. In Sect. 3, I characterize specialty-incommensurability as a complex mixture of both semantic and methodological elements, which do not necessarily pose a problem to inter-specialty communication. In Sect. 4, I synthesize the claims made in the previous two sections by discussing the discovery of the structure of DNA and the creation of molecular biology. Finally, in Sect. 5, I explain how the view on specialization developed in this paper derives from an attentive analysis of some Kuhnian premises, which entail some conclusions that perhaps Kuhn himself could not see with enough clarity, or are even at odds with what he actually thought. The directions for some future work on the study of the development of the sciences will also be indicated.

2 Revolutions, scientific communities and specialization

2.1 Scientific revolutions as community-changes

Before trying to understand whether the creation of a new specialty corresponds to a Kuhnian revolution, it is necessary to understand what a Kuhnian revolution is. One possible way to understand the notion of a Kuhnian revolution consists in defining *what* changes in a revolution. Over the course of his career, however, Kuhn changed his mind on precisely this point.

In *SSR*, the growth of science is described as the historical alternation of periods of normal science and scientific revolutions. Normal science is a relatively long and stable period of cumulative research, which is made possible by the consensus of the members of the scientific community upon a dominant *paradigm*. The paradigm dictates how to interpret evidence; it incorporates a set of scientific achievements, or ‘exemplars’, which tells scientists what problems should be considered scientific and therefore solved, how to solve them and what the acceptable problem solutions should look like; it also provides the theoretical language and a largely unquestioned world-view. Normal scientists pursue the paradigmatic agenda by applying the paradigm to a wide number of scientific problems (or even to smaller-scale scientific ‘puzzles’). The wider the number of problems scientists try to solve, the higher the chance of encountering particularly hard problems. When faced with too many recalcitrant problems, or *anomalies*, the scientific community may enter a state of *crisis*. The response to the crisis is a period of *extraordinary science*, during which potential alternatives to the dominant paradigm are taken into consideration and developed. A *scientific revolution* occurs when the pre-existing paradigm is overthrown by a new paradigm, which is capable of solving the old anomalies and which lays the foundations for a new period of normal science. The pre- and post-revolution paradigms are *incommensurable*: there exists no common measure for the comparison of their theoretical languages, methodological standards and world-views. Furthermore, although the post-revolution paradigm recovers much of the (empirical or theoretical) successes of its predecessor, some of the problems that the pre-revolution paradigm attempted to solve are no longer regarded as genuinely scientific: a scientific revolution comports a restriction in the number and type of scientific questions which can be asked, a sort of *loss*. Following this view, science does not progress steadily and cumulatively towards the ultimate truth, but it is driven from behind: from old problems to an increased problem-solving power. This is, in a nutshell, Kuhn's model in *SSR*.

The model of *SSR* revolves around the concept of a *paradigm*: normal science is defined as the cumulative period in which scientists work in the light of a dominant paradigm, while a scientific revolution is defined as a paradigm-shift. A revolution, in other words, is a change of at least some, if not all, of the things a paradigm provides. The problem is that, as pointed out by Masterman (1970), 'paradigm' in *SSR* is a rather polysemous term. Perhaps convinced by Masterman's analysis, in some post-*SSR* writings (Kuhn 1977a, c, 2000b) as well as in the *Postscript* to the second edition of *SSR* (published in 1970), Kuhn distinguishes between *disciplinary matrix* and *exemplar* and seems to restrict his attention to the latter.

Later on, however, Kuhn drops the concepts of paradigms, disciplinary matrices and exemplars to focus on the conceptual structure of scientific theories, which, in his view, respects a taxonomic hierarchy (Kuhn 1983, 1991, 2000a). As a result, Kuhn's whole model of science is redefined. Anomalies and crises are now caused by the discovery of an entity which violates the so-called 'no-overlap principle'; an entity, that is, which is a member of two unrelated kinds in the pre-existing conceptual taxonomy (for an early analysis of Kuhn's notion of scientific taxonomy, see Hacking 1993). A scientific revolution is not a 'change of paradigm' any longer, but a 'change of taxonomic conceptual structure'. Such a taxonomic change consists in both a change in the criteria for determining the membership to a kind and a redistributions of referents among preexisting categories (Kuhn 2000c, pp. 28–32).

One of Kuhn's favorite case studies, the Copernican revolution, shows nicely how the concept of a scientific revolution can be reinterpreted as a taxonomy change. The conceptual core of the Ptolemaic cosmology is a taxonomy counting three kinds of celestial body—stars, meteors and planets—in which the Moon and the Sun are classified as planets, whereas the Earth, being the center of the universe, is neither a planet nor any other kind of celestial body. The Copernican taxonomy possesses a fourth kind of celestial body, the satellite, and classifies the Sun and the Moon as a star and a satellite respectively, and the Earth as a planet. What happened during the so-called Copernican revolution, therefore, was not just an 'improvement' of the old taxonomy—for example, through the addition of a new kind. What happened, rather, was the replacement of one conceptual system with another. In the transition from the Ptolemaic to the Copernican taxonomy, the criteria for determining the membership to the celestial kinds were deeply altered, and the referents of such kinds were redistributed.

Although some philosophers have shown a rather dismissive attitude toward Kuhn's late 'linguistic turn' (Bird 2002), others have vindicated the taxonomic-conceptual model of scientific revolutions by recurring to some theories and findings from the cognitive sciences (Andersen et al. 2006). Here, I will not examine Kuhn's mature 'taxonomic model' in more details, for a number of reasons. To begin with, although most of the literature on Kuhn's mature philosophy focusses on the concept of a taxonomy, in his late writings, Kuhn actually uses several different terms—not only taxonomy, but also 'lexicon' and 'conceptual network'. It is not entirely obvious that all these terms are synonyms and, therefore, whether a scientific revolution should be defined exclusively in terms of a 'change of taxonomy'.

Even if it was the case that Kuhn really intended to focus exclusively on conceptual taxonomies, many scientific theories either do not possess such a rigid hierarchic structure, as in the case of the chemical table of elements (McDonough 2003), or they are constituted by a plurality of overlapping taxonomies, as in the case of the equally valid but inconsistent classifications of stellar kinds (Ruphy 2010). Furthermore, some scientific revolutions were not preceded by the failure of the dominant conceptual taxonomy to accommodate a new kind of entity; in fact, revolutionary changes may occur because of changes in the conceptualization of processes and events (Chen 2003a, b, 2005, 2010). In short, the history of science is full of revolutionary changes that cannot be described as changes of conceptual taxonomies (Bird 2012).

Instead of arguing whether a scientific revolution is best described as a 'change of paradigm' or as a 'change of taxonomy', here, I will adopt a different approach. Following Demir (2008), who has shown how the notion of incommensurability can be understood differently depending *whom* it may pose a problem for (i.e., scientists, historians of science or philosophers), a better understanding of Kuhn's notion of a scientific revolution will be provided by examining *for whom* revolutionary changes occur. In order to do so, it is first necessary to explain a crucial concept of Kuhn's philosophy, namely the concept of a *scientific community*.

In various works throughout his career, Kuhn explains that: the members of the community posses a special knowledge (they are *experts*); such a community is distinguished, or even isolated, from the non-expert public; and membership to the scientific community is acquired through a special training (see also Nickles 2003, pp. 146–147). In Kuhn's view, the scientific community is both the *agent* and the *locus* of scientific activity. This means that the scientific community is also the agent and the locus of scientific revolutions. In other words, the scientific community is the unit undergoing a revolution and a revolution always affects the pre-existing structure of the scientific community. Although, in *SSR*, scientific revolutions are defined as changes of paradigm, it is crucial to understand that a paradigm is something that the members of a scientific community have reached a consensus upon and which guides their research. In his post-*SSR* writings, Kuhn drops the notion of a paradigm but maintains his view of the scientific community as the agent and locus of scientific change. In short, whether it is defined as a change of paradigm, of lexicon or of conceptual taxonomy, in every formulations Kuhn gave throughout his a career, a scientific revolution always involves and is completed by and within a scientific community. In a sense, scientific revolutions are a type of 'social change'.

With this in mind, it is possible to see why some changes which are revolutionary within a community may not be perceived as such by the members of other communities (or may not be noticed at all).[1] Asking whether the event *X* was revolutionary in itself, without further qualifications, makes little sense. Even in his late works, Kuhn stresses the importance of asking *for whom* an episode of scientific change actually counts as revolutionary.[2]

Scientific communities exist at different 'levels'. As Kuhn writes in the *Postscript*:

> [the] most global is the community of all natural scientists. At an only slightly lower level the main scientific professional groups are communities: physicists, chemists, astronomers, zoologists and the like. For these major groupings, community membership is already established except at the fringes. Subject of highest degree, membership in professional societies, and journals read are ordinarily more than sufficient. Similar techniques will also isolate major subgroups: organic chemists, and perhaps protein chemists among them, solid-state and high-energy physicists, radio astronomers, and so on. It is only at the next lower level that empirical problems emerge (Kuhn 1996, p. 177).

Leaving aside the problem of isolating scientific communities,[3] Kuhn's reference to their multi-level structure solves a problem raised by several critics. It has been said that Kuhn oscillates between *gradualism* (when he stresses small incremental changes in the scientists' activity) and *discontinuism* (when he speaks about revolutionary

1 As Hoyningen-Huene clarifies, "[t]he agent of a scientific revolution is, like that of a tradition of normal science, a scientific community. […] [The] question of whether a given episode in scientific development should properly be ascribed to revolution or to normal science can only be answered relative to particular communities. Since some developments have revolutionary character only for the group immediately involved but are cumulative for some more distant group, this point isn't trivial." (Hoyningen-Huene 1993, pp. 200–201)

2 "[It is] with respect to *groups* that the question 'normal or revolutionary?' should be asked. Many episodes will then be revolutionary for no communities, many others for only a single small group, still others for several communities together, a few for all the sciences" (Kuhn 2000c, p. 148, *my emphasis*).

3 In *SSR*, a scientific community is a group of specialists who has reached a consensus upon a paradigm and a paradigm is something which a group of specialists has reached a consensus upon. There is a clear circularity here, and understanding what a scientific community is without a prior recourse to paradigms (or taxonomies, etcetera) is a non trivial problem. In the *Postscript*, Kuhn attempted to solve it by relying on some scientometrical methods, such as the examination of scientists' communication networks and the counting of citations linkages. Kuhn's approach is criticized by Musgrave (1971), who rightly points out that scientists from different communities may nevertheless cite each others' works for various reasons, and that therefore counting citations and compiling bibliometrical indexes 'mechanically' is not sufficient to determine with precision scientists' membership to one scientific group rather than another. With hindsight, however, Musgrave's criticism seems too harsh. When Kuhn started to write about scientific communities, scientometrics was still in its infancy. Furthermore, Kuhn was suggesting one possible way to isolate communities without recurring to paradigms, not *the* only way. The fact that Kuhn was not able to find a precise method to define a scientific community does not imply that any talk about scientific communities is impossible in principle.

breaks). This double attitude towards scientific change does not help in understanding why some episodes of scientific change are revolutionary, whereas something like Maxwell's electromagnetic theory is regarded as a 'normal change' within the wider paradigm of classical mechanics (Nickles 2013, p. 118).

Since scientific communities exist at different levels, scientific revolutions (which are caused by and occur within scientific communities) occur at different levels too. An example of a 'high-level revolution' is the Copernican revolution, which not only changed astronomy but also had shattering implications for the general metaphysical view of its time. If Kuhn was interested only in this type of revolutions, his model would only capture some extremely rare episodes in the history of science. This was not what he had in mind:

> "[a] few readers of [*SSR*] have concluded that my concern is primarily or exclusively with *major revolutions* such as those associated with Copernicus, Newton, Darwin, or Einstein. A clearer delineation of community structure should, however, help to enforce the rather different impression I have tried to create. A revolution is for me a special sort of change involving a certain sort of reconstruction of group commitments. But it need not to be a large change, nor need it seem revolutionary to those outside a single community." (Kuhn 1996, pp. 180–181, *my emphasis*)

By considering that revolutions can occur at different levels of the scientific community, one can see how, for example, Maxwell's electrodynamic theory both was and was not revolutionary. Before Maxwell's theory, there were indeed *two* distinct disciplines—electric physics and magnetic physics. After Maxwell, the electric and the magnetic forces, once believed to be different, became the 'electromagnetic force' and the two different sub-communities of scientists became a single sub-branch of classical physics. For electric and magnetic physicists, Maxwell's electrodynamic theory was indeed a revolution: they had to re-conceptualize old phenomena in new ways, the communities they once belonged to no longer exist and the old division of knowledge they were accustomed to died off. At the high-level view of classical mechanics as a whole, however, there was not such a big change: electric and magnetic phenomena kept on being regarded as scientific problems pertaining to classical physics in general, both before and after Maxwell.

Kuhn's view on scientific communities and revolutions can be summarized as follows:

1. scientific communities are the *agents* and *loci* of science: normal activity is carried out within a community and, similarly, a scientific revolution also happens within, and involves the members of, the community
2. there are high-level scientific communities (e.g., the communities of 'physics', 'chemistry', 'biology') and low-level scientific communities (e.g., the communities of 'quantum mechanics', 'organic chemistry', 'molecular biology')
3. for 1 and 2, there can be high-level revolutions (occurring in communities at the high-levels) and low-level revolutions (occurring in lower-level communities)
4. a revolutionary change occurring in a scientific community may not be noticed by the members of other scientific communities; or, if it happens at a low-level

> community, or in a sub-community, it may not be perceived as revolutionary by all the members of the rest of the wider community

It remains to be seen whether specialization fits Kuhn's model of scientific progress through revolutions.

2.2 Revolutions and specialization

The process of specialization does not look as 'destructive' as scientific revolutions. After a scientific revolution, the old scientific tradition is discarded once and for all. By contrast, a new specialty does not replace its parent-discipline(s). Nevertheless, Kuhn sometimes speaks of revolutions and specialization as if they were somehow associated.

> After a revolution there are usually (perhaps always) more cognitive specialties or fields of knowledge than there were before. [...] [R]evolutions, which produce new divisions between fields in scientific development, are much like episodes of speciation in biological evolution. The biological parallel to revolutionary change is not mutation, as I thought for many years, but speciation. And the problems presented by speciation (e.g., the difficulty in identifying an episode of speciation until some time after it has occurred, and the impossibility, even then, of dating the time of its occurrence) are very similar to those presented by revolutionary change and by the emergence and individuation of new scientific specialties (Kuhn 2000c, pp. 97–98).

> [T]he episodes that I once described as scientific revolutions are intimately associated with the ones I've [...] compared with speciation. [...] Thought the process of proliferation is often more complex than my reference to speciation suggests, there are regularly more specialties after a revolutionary change than there were before (Kuhn 2000c, pp. 119–120).

Although suggestive, Kuhn's view on specialization is rather underdeveloped. Recently, Wray (2011) has examined and expanded upon Kuhn's original insights. For Wray, Kuhn's mature philosophy has the merit of examining a type of scientific change—namely, the proliferation of specialties—which has been mainly discussed by sociologists and historians, but not philosophers. Wray maintains that sociological and historical explanations of scientific specialization tend to be 'mono-causal': they explain the creation of new specialties as the result of just one sociological cause. Such mono-causal explanations are based on the assumption that scientists create new sub-disciplines in order to be able to 'migrate' toward them. In this way, they can leave an older and overcrowded field, which would offer fewer chances of a good career. These socio-historical accounts revolve around the personal motivations scientists may have to work in a more rewarding and less competitive discipline. They fail, however, to explain *how* new specialties come into being in the first place. As Wray suggests, Kuhn's mature work, by contrast, helps us to see how, although sociological factors may play some role in accelerating it, specialization happens for epistemic reasons.

For Wray, the epistemic reason for why groups of scientists branch off from their parent-disciplines in ways which fit Kuhn's description is the failure of the pre-existing disciplines to solve some persisting problems. Sometimes, such persisting problems are provided by the discovery of new kinds of entity which cannot be accommodated within pre-existing conceptual structures (Wray 2011, pp. 118–122). Wray's chief examples are the creation of *endocrinology* and *virology*. In both cases, the new specialty was created as a response to the discovery of a new kind of entity that conflicted with the pre-existing classification systems. In the first case, the discovery of 'hormones' led some physiologists to re-conceptualize the co-ordination of certain body functions in terms of chemical transmission, rather than nervous mechanisms. In the second case, sub-groups of bacteriologists and bio-chemists realized that some microorganisms were relevantly different from bacteria and toxins and, as a consequence, they converged toward an independent, new specialty, in order to study the properties of the newly discovered 'virus' (Wray 2011, pp. 127–130). The discoveries of these entities have the same, complicated 'historical structure' described by Kuhn (1962): they could not be predicted in advance by the pre-existing conceptual systems; they were met with resistance from many members of the scientific community, who were not entirely persuaded about *what* had been exactly discovered; they led to 'priority disputes' about *who* actually made the discovery first. Rather than being innocent additions to knowledge, the discoveries of hormones and viruses brought a conceptual shift. Through these examples, Wray shows how discoveries and conceptual changes, and not just sociological factors, are at the basis of specialization.

Wray, however, seems to ignore Kuhn's (sparse) hints at a possible connection between revolutions and specialization. In his view, Kuhn's philosophy describes two distinct kinds of scientific change: on the one hand there are scientific revolutions (disruptions with the normal tradition, leading to the abandonment and replacement of an old paradigm), on the other there is specialization (which is not destructive). What emerges from Wray's discussion is a picture of scientific development which resembles a tree, the branches of which gradually grow (normal science), break (revolutions) and split in sub-branches (specialization) (Wray 2011, p. 125, Fig. 3), without any further investigation on the potential link between the two types of scientific change.

Wray's distinction between revolutions and specialty-creation is problematic. If, on the one hand, Wray uses the examples of endocrinology and virology to "illustrate the important role that conceptual changes can play and have played in the creation of new scientific specialties" (Wray 2011, p. 130), on the other, it is not entirely clear *why* he believes that the conceptual changes behind the emergence of a new specialty are fundamentally different from the conceptual changes which trigger a revolution.

Behind the phenomenon of specialization there are sociological, psychological and epistemic reasons, which are intertwined in complicated ways. As a result, there are different ways in which the story of the creation of a new specialty can be told. For example, by looking at the histories of virology, it appears that historians like van Helvoort explicitly use the Kuhnian model of revolutions to describe the emergence of the new discipline: although the bacteriological paradigm was not replaced, the development of the concept of virus, with all the controversies associated with it, violated many important expectations and theoretical assumption and represents a case of revolutionary epistemic rupture with the pre-existing tradition (van Helvoort

1991, 1992, 1993, 1994). This is not to say, of course, that the Kuhnian model is the *only* possible historiographical approach to describe specialization. As Méthot (2016) points out, not everybody agrees with van Helvoort's 'Kuhnian reading' of the creation of virology; but Méthot also points out that not every historians agree on how to tell the story of the creation of virology: while many narratives focus on the development of the concept of virus, others are more concerned with the development of the experimental practice which made such a discovery possible in the first place. In summary, if one claims that specialization is driven by the discovery of something which violates the normal expectations, that such discoveries create controversies and debates which are hard to solve and that the result of such discoveries is a profound conceptual change, then one should also explain why such profound conceptual change is not the same as the conceptual change which drives a scientific revolution. Wray does not elaborate such an argument.

When considering *who* the agents involved in the breaks and splits of the tree of science are, it becomes difficult to distinguish different types of scientific change as Wray does. Scientists always create a new specialty from within their parent-discipline(s): no new specialty comes into being without the direct involvement of scientists who already practice in pre-existing disciplines. What happens is that, first, these specialists recognize the inability of their discipline to solve some recalcitrant problems. Scientists' dissatisfaction with the methods and proposed solutions of their own discipline looks similar to a perceived sense of *crisis*. It is because they are dissatisfied with some concepts and methods of their discipline that they begin to consider alternative problem-solving approaches. By doing so, they create new concepts and inventing new strategies to solve some old problems in new ways, thus entering a period which is not too dissimilar from what Kuhn in *SSR* defines as *extraordinary science*. In the transition toward the emerging specialty, the sub-group of special scientists will focus exclusively on problems arising from a restricted domain, abandoning the concepts and methods of the parent-discipline(s) and replacing them with the new ones. This sort of *loss* is reciprocal: with the emergence of the new specialty, the parent disciplines will also loose a fragment of their old ontology. The sub-group of scientists which has migrated from some pre-existing discipline to the new specialty will inhabit (and will have to adapt to) a new 'niche': they will, in other words, live 'in a different world'. Finally, and in a sense which will be explored in the next section, the isolation which consents the establishment of new specialties is driven by a form of *incommensurability*. In short, the process of specialization appears to follow the same steps and to be characterized by the same elements as a scientific revolution. It must be specified, however, that the fact that a sub-group of specialists is dissatisfied with how their discipline deals with some problems arising from a restricted area does not mean that the *whole* pre-existing discipline is in a state of crisis and must be replaced in toto.

As discussed in Sect. 2.1, Kuhn is interested in both *high-level* and *low-level revolutions*, that is, changes affecting *high-level* and *low-level scientific communities* respectively. Simply put, when considering *for whom* scientific changes occur, the creation of a new specialty is a low-level revolution, affecting a sub-group of scientists.

Wray is deeply aware of the centrality of the concept of a scientific community in Kuhn's philosophy, which he even describes in terms of a 'social epistemology of science'. However, he fails to provide an argument for why scientific revolutions and the creation of a new specialty—which both affect (parts of) the scientific community and for similar epistemic reasons—are different kinds of scientific change. On the one hand, he says that "a revolutionary change occurs only when a *research community* replaces the theory with which it works with another theory" (Wray 2011, p. 15, *original emphasis*), on the other, he does not recognize that the creation of a specialty *also* involves a *part* of an existing research community replacing one theory (or paradigm, taxonomy, etcetera) with another. In the view developed here, instead, revolutions and specialization are triggered by the same mechanism, but with different results: paradigm-replacement, in the first case, the creation of a new discipline, in the second.

3 Incommensurability and specialization

3.1 Incommensurability

For Kuhn, specialization is driven by a form of incommensurability. In order to understand what such a claim amounts to, it is, first, necessary to understand Kuhn's notion of incommensurability.

In mathematics, two magnitudes are said to be incommensurable if there is no common measure for their comparison—as in the case, for example, of the radius and the circumference of a circle, the ratio of which cannot be expressed by an integer number, but by the irrational number π. In *SSR*, the term 'incommensurability' is used metaphorically to illustrate a phenomenon intimately associated with scientific revolutions. Through a revolutionary paradigm shift, scientists undergo a *perceptual change* (they see things differently), a *semantic change* (they adopt a new theoretical language), a *methodological change* (they change their standards for evaluating theories, problems, problem-solving methods and solutions) and, in a sense, all these changes correspond to a *world change*. Since they apply different concepts and methods towards the resolution of different ranges of problems, proponents of competing paradigms fail to make complete contact with each other's views. The concept of incommensurability thus describes the lack of absolute extra-paradigmatic principles for the comparison of pre- and the post-revolution scientific traditions.

The curiosity (and the criticisms) of many philosophers has been attracted by the semantic aspect of incommensurability; sometimes, this is the *only* aspect of incommensurability to be discussed at all (Sankey 1994). *Semantic incommensurability* expresses the idea that scientists belonging to incommensurable scientific traditions speak different, untranslatable languages: since they attach different meanings to the same terms, they end up talking at cross-purposes, experiencing occasional 'communication breakdowns'.

One of the most famous arguments against semantic incommensurability is that such a notion is self-defeating: Kuhn, it is said, claims that the scientific theories of the past are expressed in a language 'incommensurable' with respect to the language of our current theories; yet Kuhn himself does exactly what his notion of incommensurability should forbid, when, as a historian, he understands and translates some past scientific theories into our contemporary language (Shapere 1966; Scheffler 1967). Kuhn replies to these criticisms by claiming that incommensurability involves only small parts of the theoretical language of competing theories.

The idea of *local incommensurability* becomes clearer in a number of post-*SSR* papers, where Kuhn explains that different conceptual taxonomies are incommensurable when they have different criteria of classification, that is, different criteria for kind membership assignation. For example, the Ptolemaic and the Copernican taxonomies are incommensurable because there is no *lingua franca* in which the Sun is both a planet and a star, or the Earth both is and is not a celestial body (see above, Sect. 2.1). Since it involves only a relatively small and circumscribed cluster of inter-defined kind-terms, local incommensurability does not imply total incommunicability. The possibility of communicating across revolutions is guaranteed by those parts of the theoretical language which preserve their meanings. Furthermore, scientists can learn how to 'interpret' the parts of their opponents' conceptual taxonomy which are incommensurable with their own (Kuhn 2000c, pp. 33–57).[4]

There is another aspect of incommensurability which, in recent times, has sparked a renewed interest among philosophers of science (see, for example, Chang 2013). As described in *SSR*, a scientific revolution is a change of paradigm, which tells scientists how they should carry out the scientific research. A change of paradigm, therefore, is also a change of what the scientific research is about, of how such a research should be carried out and of how its results should be assessed. Proponents of competing paradigms, therefore, evaluate the weaknesses and strengths of their opponents from their own paradigmatic perspective. Recourse to evidence is to no avail for adjudicating which paradigm is the 'right one', since evidence is always interpreted in the light of a paradigm. Nor can some logically valid argument convince scientists to abandon a paradigms in favor of its competitor, since the very premises which are considered valid from one paradigmatic perspective may be dismissed as 'unscientific' from another. The philosophical literature groups these problems under the label of *methodological incommensurability* (see Hoyningen-Huene and Sankey 2001, pp. 13–15). Methodological incommensurability does not necessarily have to do with meaning variation and incommunicability: the divergence of standards of theory appraisal may arise even when scientists fully understand each other's conceptual vocabulary.

Methodological incommensurability seems to threaten the idea that the progress of science is rational. Since there are no neutral, extra-paradigmatic rules for inter-paradigm comparison, one could fear that the whole process of paradigm choice is

[4] For further discussions on the taxonomic version of incommensurability see, among others, Andersen et al. (2006), Chen (1997), Kuukkanen (2008), Sankey (1998), Wang (2002), Wolf (2007). As already mentioned in Sect. 2, here I will not delve too much into Kuhn's theory of taxonomy, simply because such a theory is misleading. The conceptual structure of many theories simply is not 'taxonomic'. For instance, while one can say that the Newton's and Einstein's theories are incommensurable because, among other things, they attach different meanings to 'mass' and 'force', it would be hard to regard 'mass' and 'force' as being 'taxonomic kind terms'; rather, they look more like 'nodes' in complex, non-hierarchical and non-taxonomic 'conceptual networks'. A proper discussion of the difference between a taxonomic-view and a network-view of the conceptual structure of scientific theories would go far beyond the limited scopes of the present paper. In both cases, however, incommensurability could still be defined as the difference in the criteria for meaning-determination of a cluster of inter-defined theoretical terms.

guided by merely sociological, political or economical or reasons. Kuhn, however, did not intend to claim that science is irrational. Already in the *Postscript*, he says:

> Only philosophers have seriously misconstructed the intent of these parts of my argument. A number of them, however, have reported that I believe the following: the proponents of incommensurable theories cannot communicate with each other at all; as a result, in a debate over theory-choice there can be no recourse to good reasons; instead, theory must be chosen for reasons that are ultimately personal and subjective; some sort of mystical apperception is responsible for the decision actually reached. More than any other part of [*SSR*], the passages in which these misconstructions rest have been responsible for charges of irrationality (Kuhn 1996, pp. 198–199).

Later on, he actually expressed profound aversion towards the more extreme positions adopted by some sociologists of science (see Kuhn 2000c, pp. 110–111). He even went as far as saying:

> I do not for a moment believe that science is an intrinsically irrational enterprise. [...] Scientific behavior, taken as a whole, is the best example we have of rationality (Kuhn 1971, pp. 143–144).

Kuhn aimed at undermining the neo-positivist ideal of a stable, absolute and unchangeable set of scientific rules, a solid 'Archimedean platform' guiding scientists in the process of theory choice. That Kuhn rejected the neo-positivist view on scientific rationality does not imply that he wanted to dispense with the idea of scientific rationality *tout court*. What incommensurability shows, in Kuhn's opinion, is not that science is irrational but, rather, that we need a more complex and nuanced concept of scientific rationality than a naive faith on an infallible algorithm (Kuhn 2000c, pp. 155–162).

As Kuhn (1977b) explains, scientists agree on which 'values' are necessary for a theory to be considered as scientific—i.e., accuracy, consistency, breadth of scope, simplicity and fruitfulness. All proper scientific theories possess these values, albeit to various degrees. The source of methodological incommensurability consists in the fact that scientists may not agree on how to weight such values: some scientists may prefer the theory which is simpler and more accurate, while others may prefer the most fruitful and promising. A further layer of complication is represented by the fact that, during periods of extraordinary science, scientists have to make a comparative evaluation about two competing paradigms, one of which is the dominant and well-established, while the other is yet to be fully developed. The problem of to the so-called *prospective rationality* posed by methodological incommensurability—the problem, that is, of making sense of how the proponents of an established paradigm could end up choosing in a rational manner to endorse a paradigm which is not even fully developed yet—is only apparent. As explained in Sect. 2, scientific revolutions are community affairs. A scientific revolution, however, is not resolved overnight. This means that the choice between incommensurable theories is not instantaneous: it is, rather, the result of a process taking place within the community. During such a process, as it gets more confirmed and theoretically more refined, the consensus of the majority of the scientific community will shift towards the new theory. It is important to stress that Kuhn's views on methodological incommensurability and the rationality of theory choice, like those on revolutions, are grounded on the idea that the scientific community is both the *locus* and the *agent* of a scientific revolution.

That Kuhn's incommensurability does not imply irrationality has been discussed by, among others, Bird (2000), Brown (1983), D'Agostino (2014), Earman (1993), McMullin (1993), Salmon (1990) and Wray (2011). Recently the application of social choice theorems to the issue of scientific theory choice seems to vindicate Kuhn's intuitions: in fact, some argue that the impossibility of an algorithm for choosing theories does not make theory choice irrational, although they may disagree on how a different, more nuanced model of rational choice should look like (see Okasha 2011; Bradley 2016).

In short, Kuhn's ideas on incommensurability can be summarized as follows. Incommensurability indicates the lack of a set of shared and stable principles for inter-paradigmatic comparison. In its semantic form, incommensurability indicates the lack of a *lingua franca* between restricted parts of competing conceptual systems with different criteria of classification. In its methodological version, incommensurability indicates the lack of independent evaluative standards. Incommensurability does not imply incommunicability. Incommensurability does not imply irrationality.

It remains to be seen in which sense different specialties are incommensurable.

3.2 Specialty-incommensurability

The process of specialization is a process of isolation which, for Kuhn, is driven by a form of incommensurability. The growing insularity, driven by incommensurability, allows the newly emerged specialty to refine and restrict its own domain, to increase in precision and to establish itself as an independent discipline. Kuhn describes specialty-incommensurability as a conceptual disparity which keeps specialties separated by making inter-specialty communication difficult:

> what makes […] specialties distinct, what keeps them apart and leaves the ground between them as apparently empty space […] is incommensurability, a growing conceptual disparity between the tools deployed in the two specialties. Once the two specialties have grown apart, that disparity makes it impossible for the practitioners of one to communicate fully with the practitioners of the other. And those communication problems reduce, though they never altogether eliminate, the likelihood that the two will produce fertile offspring (Kuhn 2000c, p. 120).

Like many of Kuhn's late ideas, the notion of specialty-incommensurability is rather underdeveloped. In his attempt to clarify and assess Kuhn's more mature philosophy, Wray speaks of specialty-incommensurability as being akin to semantic incommensurability. He maintains that "[s]cientists working in neighboring specialties are often impeded in *effective communication* across specialty lines because *they attach different meanings to the same terms* (Wray 2011, p. 75, *my emphases*).

Both Kuhn and Wray focus on semantic aspects, but while, for Kuhn, specialty-incommensurability impedes 'full communication', for Wray, it impedes 'effective communication'. It must be noticed, however, that a lack of full communication does not imply the impossibility of ‘effective (but limited) communication’ de jure: a partial communication could still produce some limited yet valid outcome. Furthermore, the ubiquity of the so-called *inter-disciplinary research*, in which scientists coming from different specialties collaborate, shows that there is indeed a lot of effective communication across specialties de facto.[5]

The rationale behind Wray’s claims that specialty-incommensurability represents an impediment to effective communication may be that, in his account, a lot of emphasis is put on those cases in which a new specialty is created after a ‘significant discovery’: not the simple discovery of a new kind of entity to be simply added to the pre-existing scientific classification, but a discovery which “require[s] radical changes to the taxonomy of a field with the result that a new field [is] born and the domain of the original field [is] subsequently truncated” (Wray 2011, p. 129). So, in his examples, virology and bacteriology are incommensurable because they attach different meanings to the same term, ‘virus’, and this would make the communication between virologists and bacteriologists impossible. The same happens to endocrinologists and physiologists, who experience communication breakdowns because they use the same term, ‘hormone’, in different ways. It is however difficult to agree with Wray on this point. Although it is true that, *before* virology and endocrinology were established as autonomous disciplines, different scientists had different ideas about viruses and hormones, over time, those concepts were removed from the conceptual vocabulary of the parent-disciplines. It is hard to say that bacteriology and virology attach a different meaning to ‘virus’, simply because ‘virus’ is not part of the language of bacteriology anymore. It is even harder to suppose that bacteriologists cannot communicate with virologists because they cannot understand what the latter mean by ‘virus’.

In both the cases discussed by Wray, the newly discovered kind did not result in a radical re-organization of the conceptual taxonomies of the pre-existing mother discipline(s), but only to a ‘loss’ of a part of their ontologies. Therefore, it is not entirely clear whether bacteriology and virology are ‘conceptually incommensurable’ or simply ‘about different things’. The problem is that the concept of incommensurability makes sense only in the context of *competing* paradigms to choose from. If specialties are just about different problems, different domains, and so on, then the concept of specialty-incommensurability risks becoming trivial.

Both Kuhn and Wray speak of specialty-incommensurability as a sort of ‘linguistic barrier’. Although Kuhn and Wray may have different views on what such a linguistic barrier actually impedes—whether ‘full’ or ‘effective communication’—such a characterization of specialty-incommensurability runs against what was said by Kuhn himself: namely, that incommensurability does not imply incommunicability (see Sect. 3.1). It follows that: *either* the linguistic barrier among specialties can always be overcome; *or* that specialty-incommensurability does indeed imply

[5] Neither Kuhn nor Wray explain how inter-disciplinary research is even possible in the face of specialty-incommensurability. Some philosophers use the example of interdisciplinary research to argue against the very existence of specialty-incommensurability (Andersen 2013). Whether specialties can be both incommensurable and capable of generating interdisciplinary research will be investigated in my future work.

incommunicability and, therefore, differs in some relevant ways from the incommensurability between pre- and post-revolution scientific traditions, which does not. In the first case, it is not clear what would be so special about the linguistic barrier to begin with; in the second case, both Kuhn and Wray do not explain why specialty-incommensurability is so different from the other form of incommensurability.

Perhaps the problem with both Kuhn's and Wray's account of specialty-incommensurability lies in the excessive emphasis they put on semantic issues. I am not denying that there is a semantic aspect to specialty-incommensurability: among other things, my example of the discovery of the DNA-structure, in Sect. 5, will also refer to the semantic incommensurability among co-existing disciplines, which attach different meanings to the term 'gene'. What I am saying is that semantic incommensurability across specialty may not always be as strong as Kuhn and Wray seem to imply. Furthermore, by speaking of specialty-incommensurability almost exclusively in linguistic terms, both Kuhn and Wray miss the opportunity to examine the methodological issues arising in the process of specialization.

Although the incommensurability thesis, in general, was never meant as an argument against the rationality of science (see Sect. 3.1), specialty-incommensurability, in particular, seems to dissolve some of the problems arising in theory choice and theory comparison: instead of choosing only one from two incommensurable paradigms, a scientific community can maintain them both and choose to split into two different sub-communities instead. This point has recently been made by Davies (2013), who also criticizes some parts of Wray's interpretation of Kuhn. However Davies, like Wray, puts great emphasis on the semantic aspects of incommensurability in general, and specialty-incommensurability in particular. Furthermore, in his view, specialty-incommensurability has nothing to do with issues of theory appraisal and theory comparison because, Davies seems to suggest, the conceptual languages of different specialties are just about 'different things'. If this was the case, once again, it would not be clear why we should speak about 'incommensurable specialties' rather than, more simply, 'different disciplines'. Unlike Kuhn, Wray and Davies, I want to stress the methodological aspects of specialty-incommensurability.

As described in Sect. 2, a new specialty emerges from within some pre-existing discipline(s), as an attempt to solve some persistent problems in new ways. After they have narrowed down its domain and developed its conceptual language, some scientists migrate toward the new discipline. This whole process, however, is neither sudden nor smooth. Scientists belonging to the parent-discipline(s) may be resistant to accept the new solutions as valid. This has also been illustrated in Sect. 2, through a finer recounting of one of Wray's examples, namely the creation of virology. Before the establishment of virology as a discipline, scientists disagreed on whether the newly discovered entities were bacteria or a different kind altogether. Such a disagreement was not just about the meaning of some terms and the disparity of conceptual languages; one can even hypothesize that the scientists involved in the debate understood each other and were able to communicate effectively. It may be the case, therefore, that another source of irreconcilable disagreement involved the assessment of the potential of the emerging discipline as a whole. In short, specialty-incommensurability may have to do with issues concerning appraisal and choice in periods during which the new specialty is emerging but has not been fully established yet.

Scientists' appraisal of the new specialty is made *before* the new specialty is fully formed and established. On assessing whether an emerging specialty is promising, what scientists take into consideration is not only the consistency of a new conceptual language, but also things like the fruitfulness of a new approach, the applicability of some new methods and their potential problem-solving power. In other words, to be considered worthy of scientists' time and effort, the emerging specialty should offer more than a new theory which solves some marginal issues with some pre-existing classifications; in fact, it should also offer the prospect of future scientific research that is going to be 'scientifically interesting' and 'promising'. In short, the sub-group of scientists who migrates toward the new discipline may decide to do so because of a different way of evaluating and assessing its potential. Such a preventive evaluation of the potential of a new discipline is a necessary condition for its establishment.

The problem of making sense of the evaluation of the 'potential' of new theories and methods has recently generated an interesting philosophical literature about 'heuristic appraisal' (Nickles 2006) and the so-called 'context of pursuit' (Nyrup 2015; Šešelja and Straßer 2013, 2014). A similar study may illuminate the reasons for why some scientists may keep on thinking that the pre-existing, well-established disciplines are better suited to solve the recalcitrant problems, while others decide to move toward an emerging and potentially successful one.

Scientists' assessment of a promising but immature discipline seems, somehow, to mirror somehow what goes on during times of revolutionary change, when methodological incommensurability creates disagreement on how to assess a paradigm which is not fully developed yet. In both cases, methodological incommensurability produces some differences in the evaluation process: in the case of revolutions such a process is completed with the demise of the old paradigm, in the case of specialization, the process leads to a break in the structure of the scientific community. After all, if the creation of a new discipline *is* an instance of Kuhnian revolution, as argued in Sect. 2, then what has been said about scientists' appraisal in the potential of a new paradigm during scientific revolutions can simply be extended to the case of specialties formation.

Since *SSR*, Kuhn associated his incommensurability thesis to his concept of a scientific revolution. If, as I have argued in the previous section, the creation of a new specialty is indeed an instance of scientific revolution, then the rationale behind the idea of 'specialty-incommensurability' becomes more discernible. This is just a first, sketchy step towards a finer characterization of that mechanism of isolation, which, for Kuhn, drives the proliferation of scientific disciplines, and which he identified with incommensurability. Without denying that there can be (more or less strong) semantic aspects to it, specialty-incommensurability may be a complex mixture of semantic and methodological issues. Perhaps, specialty-incommensurability is closer to the polysemous 'incommensurability' discussed in *SSR*. Although a proper discussion of specialty-incommensurability will require a separate, more detailed work, the issue will be clarified by the discussion of a specific *case study*: namely, the discovery of the DNA structure and its role in the emergence of molecular biology.

4 The discovery of the structure of DNA and molecular biology: an example of Kuhnian revolution

In this section, the discovery of the double-helix structure of DNA and its relation to the emergence and establishment of molecular biology is analyzed. This case will be a useful illustration of some of the conclusions drawn in Sects. 2 and 3, namely: that the emergence of a new scientific discipline corresponds to an instance of a Kuhnian revolution for the scientists involved in the process; and that specialty-incommensurability cannot be reduced to a matter of incommunicability. This example will also help in overcoming one of the criticisms raised against Kuhn—namely, that his model of scientific development does not capture some crucial episodes in the history of science, with the discovery of the structure of DNA being one such purpoted instance. As argued by Bird:

> the discovery of the double-helix and the existence of base-pairings, unexpected but not contra-expectation, were clearly revolutionary in their consequence for biochemistry and molecular genetics. A discovery that many regard as the most important of the century 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 thought it brought into existence major new fields of research (Bird 2000, p. 60).

Bird says that the discovery of the double-helix structure of DNA was revolutionary in its consequences for molecular genetics and bio-chemistry; however, he does not say *what* such consequences were, nor does he explain *why* such consequences were indeed 'revolutionary'. My reply to Bird is that the discovery of the structure of DNA could not be revolutionary in its consequences for molecular genetics simply because, before such a discovery, there was not such a thing as 'molecular genetics'. Molecular genetics is a scientific specialty born from genetics and molecular biology. Molecular biology, however, was established as an independent discipline *after* the discovery of the double-helix. In the view which I am defending here, the discovery of the DNA structure is revolutionary not because of its effects on a discipline such as molecular genetics, which had not been established yet, but precisely in virtue of its role in the creation and establishment of molecular biology, which, in turn, represented a revolutionary transition for sub-groups of scientists coming from pre-existing disciplines.[6]

There are several different and equally rich historical accounts of the development of molecular biology. On the one hand, there are histories which draw 'big pictures',

[6] Saying that the discovery of the double-helix contributed to the creation and establishment of molecular biology as a disciplines does not mean saying that the term 'molecular biology' did not exist or was not used before such a discovery. Although Watson's and Crick's discovery was made in 1957 and the *Journal of Molecular Biology* was launched in 1959, earlier uses of 'molecular biology' date back to the 1940s or even the 1930s. The fact that the *term* was used in the 1930s, however, does not imply that the *discipline* had already been created. Understanding the "baptism story" of a new discipline—understanding, for instance, why a new discipline is named with an already available term, rather than another—may shed further light on the institutionalization of new disciplines and in the general process of specialization (Powell et al. 2007).

by telling the story of the connected efforts of geneticists, biochemists and biophysicists in the US, England and France (Morange 1998). On the other, there are more ‘local accounts’, or micro-histories, such as the history of the institutionalization of the Medical Research Council Laboratory of Molecular Biology at Cambridge (de Chadarevian 2002). Some histories are mainly focused on the role of the discovery of the structure of DNA (Olby 1994); others examine the impact of information theories on the methodology of the emerging new discipline (Sarkar 1996). Finally, it is worth mentioning that the history of molecular biology has also provoked historiographical debates on how such a history should be reconstructed (Abir-Am 1985, 2006; Judson 1980).

All of these different histories agree that the discovery of the structure of DNA played a crucial role in the establishment of molecular biology, although it is debatable whether such a discovery was sufficient for the creation of the new discipline. Arguably, molecular biology represents the convergence and merging of (at least) two different ‘schools of thought’, each concerned with specific problems. One school was focused on the problem of the structure of genes; such a problem was mainly tackled by the UK-based biophysicists at the University of Cambridge and King’s College in London. The other school was concerned with the problem of the transmission of hereditary information; scientists interested in the ‘chemistry of information’ were mainly US-based, especially at the Rockefeller Institute. An important role in the institutionalization of molecular biology was also played by the exponents of a French-based ‘third school’, working at the Pasteur Institute in Paris. Although similar in methods and aims to the English school, the French school held different conceptions on how the new discipline should have been conceived (Gaudillière 1993).

The discovery of the double-helix was the first step toward the resolution of the structural problem. The manner in which such a discovery was made is, in itself, an interesting and complex episode in the history of science. The discovery of the structure of DNA is attributed to the Cambridge-based geneticist James Watson and bio-physicist Francis Crick. However, such a discovery did not happen all of a sudden in 1953. The discovery of ‘nuclei’—which was later changed to ‘nucleic acid’ and, eventually, to ‘deoxyribonucleic acid’, or DNA—was made in 1869 by Friedrich Miescher, a physiological chemist. Among the people who investigated its structure there were Phoebus Leven, a Russian biochemist who, at the beginning of the twentieth century, proposed the ‘tetranucleotide structure’ (in which *guanine*, *cytosine*, *adenine* and *thymine* are always ordered in the same way); and Erwin Chargaff, an Austrian biochemist who studied the tetranucleotide structure in further detail and, around the 1940s, established the so-called *Chargaff’s rule*, which states that, in a DNA molecule, the total amount of *purines* (adenine and guanine) is always equal to the total amount of *pyrimidines* (cytosine and thymine).

In short, Watson and Crick’s discovery was anything but abrupt. The results of the work carried out in the previous decades by scientists like Miescher, Leven and Chargaff, together with important data collected by Rosalind Franklin and Maurice Wilkins at King’s College, represented the evidence that was put together and interpreted by Watson and Crick in 1953. It is therefore difficult to pinpoint the exact moment in which the structure of DNA was ‘discovered’. It would be equally difficult to determine *who* made the discovery first: in fact, the relationship between the evidence collected by

Franklin, on the one hand, and Watson and Crick's discovery, on the other, is still a matter of controversy (Sayre 1975; Maddox 2001; Gibson 2012). It should also be assessed up to which point the discovery of the double-helix was a matter of 'pure observation' or was, instead, the culmination of a rather 'theory-laden' process. It has been argued, in fact, that Watson and Crick did not assemble the available evidence in a purely theory-less bottom-up fashion. Rather, Cochran, Crick and Vand's 'helical diffraction theory' played a crucial role in allowing Watson and Crick to interpret the available evidence in the way they did (Schindler 2008). All in all, the discovery of the double helix exhibits the complex 'historical structure' examined by Kuhn (1962). Apart from the historical details of its discovery, however, my main point is that the discovery of the double-helix was crucial for the resolution of the 'structural problem', which was worked on by the scientists of the UK-based school of thought, but was not itself sufficient for the creation and establishment of molecular biology. Missing from this account is a consideration of the problem of the transmission of hereditary information.

The structure problem and the information problem are linked: by studying the structure of DNA, it is also possible to understand how genetic information is transmitted. The problem of transmission could not be solved by looking at the structure of DNA alone. It became clear that, to understand genetic information transmission, it was necessary to analyze the structure of RNA too. It was found, however, that RNA is not as easily observable through the known X-ray crystallographic methods as DNA: the old problem required newer methods. The resolution of the information problem also required the application of theories and models coming from information theory and cybernetics. Indeed, the influence of Wiener and von Neumann's mathematical works on the creation of molecular biology cannot be underestimated. A similarly crucial role was played by the 'decodification' work carried out by Sydney Brenner, who began his collaboration with Crick *after* the discovery of the structure of the DNA. What was 'revolutionary', therefore, was not the discovery of the double-helix per se, but the complex theoretical and methodological adjustments required for the creation of a new discipline.

It is, thus, clear that the creation of molecular biology—for which the discovery of the double-helix was necessary but not sufficient—followed the same steps of a scientific revolution. To begin with, the discovery of the structure of DNA and the establishment of molecular biology as a mature field were preceded by a sense of *crisis*. Such a crisis was felt by the members of the community of classical genetics. Geneticists study the mechanisms of hereditary transmission; in its 'classical' Mendelian form, genetics hypothesizes the existence of entities called *genes* and relies on the observation of hereditary patterns through subsequent generations. The crystallographic methods developed in the first half of the twentieth century helped to offer a more 'observational' basis to genetic studies. However, one of the pioneers of the use of crystallographic methods in genetics and of the study of the effect of X-rays on chromosomes, Hermann Muller, denounced the limits of genetics for the explanation of the properties of the genes: "[the] geneticist himself is helpless to analyze these properties further. Here the physicist, as well as the chemist, must step in. Who will volunteer to do so?" (Muller 1936, p. 214).

Furthermore, by adopting the point of view of the scientists involved in the process, it appears clear in which sense the creation of molecular biology was a *scientific revolution*. Molecular biology emerged and was established as a new discipline thanks to the concerted effort of scientists coming from crystallography, chemistry, information theory and mathematics, to solve some of the problems genetics alone was felt inadequate to solve. By focusing on a restricted range of problems, these scientists not only created a new field of research but they also 'abandoned' the research tradition they belonged to. It is important to stress that the transition from a pre-existing to a newly created discipline is not just a case of professionals moving from one department to another. In their migration toward the new field, scientists undergo a process which appear to be not too dissimilar from a paradigmatic shift. In the case of molecular biology, it was not just the case that bio-chemists began to work with geneticists and information theorists rather than keeping on working with other bio-chemists. By working together with professionals coming from different disciplines, the early molecular biologists had to create a 'new paradigm'—a new conceptual language, new methods, new techniques. Their old paradigms were simply not good enough to solve the problems they were interested in.

An interesting aspect of the discovery of the structure of DNA, and the consequent establishment of molecular biology, is the *semantic incommensurability* of different conceptions of 'gene'. Although I regard the new methodologies and instrumentation for the study of genes to be as important as the redefinition of its concept, there is something similar to the meaning variation across specialties which Wray speaks about. As discussed by Griffiths and Stotz (2008), there are different concepts of 'gene' which are currently used by different disciplines. Classical genetics has not been completely swallowed by molecular biology, and 'genetic analysis'—the study of hereditary patterns through the process of hybridization—is still largely used in several branches of biology, such as population genetics, as well as in actual zoological and agricultural practices. While molecular biology defines 'gene' structurally, classical genetics defines it on the basis of its functional role in the hereditary process. It can be said, therefore, that there is a conceptual divide between classical and molecular biology, which use the same term but with different meanings. This example seems stronger than the ones used by Wray in support of his view of specialty-incommensurability as being akin to meaning-incommensurability.

However, once again, the importance of 'methodological incommensurability' for the creation of a new discipline must be stressed. The new methods and concepts of molecular biology were considered by some to be better suited for the resolution of some old problems, *before* the establishment of molecular biology as a discipline. Not everybody assessed the potential of molecular biology in the same way nor, as my short historical reconstruction has shown, was there unanimous agreement on how to proceed or on what the new discipline should look like. The difficulties and controversies surrounding the assessment of an emerging discipline can be subsumed under the rubric of *methodological incommensurability*.

The analysis of the discovery of the structure of DNA and of its relations with the emergence of molecular biology exemplifies the view on specialization developed in the previous sections. The creation of molecular biology represented a revolutionary paradigm change for the scientists involved in such a process, preceded by a sense of crisis at the inadequacy of the pre-existing disciplines at solving some special problems. The discovery of the double-helix was necessary but not sufficient for the creation of the new discipline. The creation of molecular biology was driven by a form of (semantic *and* methodological) incommensurability. This reading of the discovery of the double-helix, and of its role for the creation of molecular biology, has been made through the lenses of a re-interpretation of Kuhn's observations on specialization. In this way, it has been shown how a case of revolutionary but *prima facie* non-disruptive scientific discovery, such as the discovery of the DNA-structure, fits Kuhn's model of scientific development.

5 Towards a new approach to scientific change

Philosophers of science have generally paid little attention to scientific specialization and, at the same time, they have also underestimated Kuhn's late comments on the topic. This is why revisiting Kuhn's post-*SSR* works is still meaningful and fruitful today. Although suggestive, however, Kuhn's late observations about specialization are rather underdeveloped. In this paper, a Kuhnian model of specialization has been developed, by examining Kuhn's own insights, and also through some comparison and contrast with some recent interpretations of his philosophy, such as Wray's.

This approach is not limited to showing that Kuhn was right about some issues, or that Kuhn's critics are either wrong or mistaken. This paper did not merely aim to show that Kuhn's model of revolutions works to describe scientific specialization. Rather, Kuhn's premises were developed in order to reach conclusions which Kuhn himself could not see with enough clarity. Kuhn hinted at some possible link between revolutions and the proliferation of specialties but without delving into this issue with enough depth. However, if one accepts that the scientific community is both the *agent* and the *locus* of a revolution, as Kuhn did (see Sect. 2.1), then one must consider the particular change in the structure of a scientific community which occurs with the creation of a new specialty as a revolutionary change (unless, of course, one provides an argument for why the two changes should be regarded as two different kinds of change—an argument which Wray, who claims exactly this, does not provide). It is also possible to show how, for the people involved in such a process, the creation of new specialties follows the same pattern of Kuhnian revolutions, that is: normal science—anomalies—crisis—extraordinary science—revolution.

It may be the case that some of these conclusions are at odds with what Kuhn himself thought. Contrary to what Kuhn writes, specialty-incommensurability should not be described as a linguistic barrier which impedes communication across specialties. The view of specialty-incommensurability delineated in this paper is, in a sense, more Kuhnian than Kuhn's. It is so because it is rooted on Kuhn's argument that incommensurability does not imply incommunicability (Sect. 3.1). Saying that specialty-incommensurability impedes communication would conflict with such an argument. Semantic aspects of specialty-incommensurability may not always be as strong as Kuhn (and Wray) seem to suggest and, moreover, so-called methodological incommensurability may play some role in the creation and establishment of a new specialty. This view on specialty-incommensurability is, therefore, akin to Kuhn's early concept of incommensurability, as presented in *SSR*: a complex mixture of semantic and methodological elements deriving from the lack of an external, super-paradigmatic set of criteria for theory choice.

There are still several problems to discuss and conclusions to be drawn from the study of specialization. The first, and perhaps more obvious, is a finer characterization of specialty-incommensurability as the mechanism which drives specialization. In this paper, claims about specialty-incommensurability are linked to the view of specialization as a case of revolutionary scientific change. Neither Kuhn nor Wray explain *why* specialties are to be considered incommensurable, rather than just about different things, nor do they investigate the tension between the notion of specialty-incommensurability (which, in their view, is a linguistic barrier) and the existence of so-called interdisciplinary research. In *SSR*, the concept of a revolution and the concept of incommensurability are strongly linked: Kuhn speaks of incommensurability between the pre- and post-revolution paradigm. Therefore, talk about specialty-incommensurability is consequential to, and in a sense justified by, the argument that the creation of a new specialty is an instance of Kuhnian revolutions. In Sect. 3.2, the methodological aspects of specialty-incommensurability have been described: scientists belonging to the same discipline may evaluate the potential of an emerging but not fully established field of research in different ways. It remains to be seen whether the values which are taken into consideration in theory choice are the same for field choice. It also remains to be seen whether anything like ‘incommensurability’ persists after the establishment of a new specialty, or whether incommensurability is what drives a ‘process of differentiation’, resulting in two different specialties. These are rather thorny issues that go beyond the scope of this work. Nevertheless, the idea that new specialties come about through a process of revolutionary scientific change is the first step toward a more detailed analysis of the role of incommensurability in specialty formation.

Another problem which both Kuhn and Wray overlook is whether what they describe is actually ‘scientific specialization’. Kuhn mentions physical chemistry as an example of a new specialty, which emerges from the partial convergence and partial overlapping of physics and chemistry, but which, with time, becomes an entirely independent discipline. In which sense, then, can physical chemistry be considered as a ‘sub-branch’ of either physics or chemistry? Which discipline is physical chemistry a specialty of? Wray’s example, as well as my own, are even more problematic. Wray speaks of the creation of virology from bacteriology and biochemistry, and of the creation of endocrinology from physiology. However, it seems clear that virology is not a ‘sub-branch’ of either bacteriology or biochemistry, and endocrinology is not a ‘sub-branch of’ physiology. It would be more correct to classify virology as a sub-branch of micro-biology (alongside bacteriology) and endocrinology as a sub-branch of medicine (alongside physiology). Molecular biology, which I used as a case study in Sect. 4, is clearly not a ‘sub-branch’ of crystallography or chemistry, but a discipline on its own. In other words, Kuhn and Wray (and, up until now, myself) use the term ‘specialization’ in a rather loose sense. Unless we stretch the concept of ‘specialization’ to a considerable degree, it seems that what is at stake here is the creation not of new specialties, but of new *disciplines*.

Talking about the creation of new disciplines in their own right, rather than sub-branches gemmating from their parent-discipline(s), is not just a terminological matter and may also have some interesting repercussions for the analysis of scientific change. In this paper, I have defined the creation of a new discipline as a scientific revolution for the scientists migrating towards it; but, in some cases, the establishment of the new discipline may have some important consequences for the pre-existing disciplines and for the scientists who do *not* migrate. At the end of Sect. 4, it was mentioned how the 'gene' of old genetics survives in some scientific contexts and remains crucial for more applied research. It is fair to say, however, that 'old genetics', as such, did not survive as a research field in its own right or, at least, that it underwent some profound modification after 'losing' some of its concepts to molecular biology. This point, which it is not possible to explore here in more details, shows the limitations of the analogy of the 'tree of scientific knowledge'. In particular, such an analogy risks obscuring, instead of clarifying, the relation between mother-disciplines and new disciplines. The creation of a new discipline is not an event as innocent and innocuous as the addition of a new sub-branch to some pre-existing trunk; rather, a new discipline may supersede some of the existing branches. Science, therefore, may not develop by simple 'proliferation' of new disciplines; rather, it appears to grow through fragmentation and dissolution: in some cases, though not necessarily every time, the creation of a new discipline does not change just the number of the branches of the tree, but the very structure of the tree.

If this is the case, then low-level revolutions, which occur with the creation of a new discipline, may actually have some large-scale effects. This point may shed some light on one of the issues with the so-called 'historical philosophy of science': the issue of whether the development of science through history is *evolutionary* (driven by small incremental changes) or *revolutionary* (characterized by epistemic ruptures). Following what I have just hinted at above, it may be possible to say that science (as a whole) evolves *through* the revolutions occurring within the sciences. This is, of course, a rough sketch of a view which would deserve a more extensive treatment.

Was this what Kuhn had in mind? It is impossible to answer such a question with certainty. However, in several of his late writings, Kuhn seems to flirt with similar ideas. He observes, for example, that one of the results of the process of specialization is that "[t]he older, more encompassing modes of practice simply die off: they are the fossils whose paleontologists are historians of science" (Kuhn 2000c, p. 120). He also seems interested in studing the issue of scientific change not only from an exclusively intra-disciplinary and intra-field perspective—not only, that is, by looking at what goes on inside single scientific disciplines—but also by considering the evolution of groups and families of sciences (Kuhn 1976). In the end, that questions such as "Was this what Kuhn really meant?" are not always worth asking. Irrespectively of what he himself may have thought about some issues, the development of some of Kuhn's original insights may still be fruitful today.

6 Conclusions

The process of scientific specialization and Thomas Kuhn's late writings have one thing in common: the fact that they have not yet received enough attention from philosophers of science. Although suggestive, however, Kuhn's post-*SSR* observations about scientific specialization are rather underdeveloped. This paper has developed a neo-Kuhnian model of specialization by examining Kuhn's late writings, as well as some recent interpretations of Kuhn's philosophy, such as Wray's. It has been shown how the creation of new specialties fits Kuhn's model of scientific revolutions. An account of specialty-incommensurability, that takes into consideration both its semantic and methodological aspects, has been outlines. This claims on specialization have been integrated and elucidated through the example of the discovery of the DNA structure, and of its relation to the creation and establishment of molecular biology.

As Kuhn says, it is a simple matter of fact that the progress of science is linked to a proliferation of specialties. This is why scientific specialization represents an interesting type of scientific change, which philosophers should consider with more attention. The present paper is just a small contribution to the philosophical analysis of such a complex topic. Kuhn himself could not develop his insights in more details. Nevertheless, his ideas, if properly analyzed and developed, can lead to conclusions that Kuhn himself was not able to see with enough clarity.

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