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The Historiography of Scientific Revolutions: A Philosophical Reflection

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Abstract

Scientific revolution has been one of the most controversial topics in the history and philosophy of science. Yet it has been no consensus on what is the best unit of analysis in the historiography of scientific revolutions. Nor is there a consensus on what best explains the nature of scientific revolutions. This chapter provides a critical examination of the historiography of scientific revolutions. It begins with a brief introduction to the historical development of the concept of scientific revolution, followed by an overview of the five main philosophical accounts of scientific revolutions. It then challenges two historiographical assumptions of the philosophical analyses of scientific revolutions.

Key words

scientific revolution; theory; paradigm; exemplary practice; unit of analysis; scientific development; web model

1. A Brief History of the Concept of Scientific Revolution

Before the seventeenth century, "revolution" was primarily an astronomical or astrological concept, referring to the motion of celestial bodies turning through 360°.[1] At the time, the general or non-scientific sense of the word "revolution" was pretty close to its astronomical meaning. For example, in *A Table Alphabeticall*, the first monolingual dictionary in the English language, 'revolution' was defined only as "turning back to the same place" (Cawdrey, 1604). In Queen Anna's New World of Words, an Italian/English Dictionary published in 1611, "revolution" was defined as "a turning backe to the first place, a revolution of celestial bodies or spheres" (Florio, 1611, p. 449). I. Bernard Cohen (1985, p. 66) also notes that "in a dictionary of 1611, 'revolution' was defined only as "a full compassing, rounding, turning backe to its first place, or point; the accomplishment of a circular course'".

Since the second-half of the seventeenth century, "revolution" had been gradually used to describe some political changes such as what we now call "the glorious revolution" in 1688. For example, Dr Johnson's Dictionary of the English Language (1755) offers three definitions of revolution, one of which is in a political sense: "1. Course of any thing which returns to the point at which it began to move"; "2. Space measured by some revolution"; "3. Change in the state of a government or country. It is used among us κατ' ἐξοχὴν [par excellence], for the change produced by the admission of king William and queen Mary". Similarly, in the first

[1] A well-known example is the title of Copernicus's book *De Revolutionibus Orbium Coelestium* (On the Revolutions of the Celestial Spheres), published in 1543.

edition of Encyclopaedia Britannica, three senses of revolution are identified: political, geometric, and astronomical.

> REVOLUTION. in politics, signifies a grand change or turn in government. In which sense, the revolution is used, by way of eminence, for the great turn of affairs in England, in the year 1688, where king James II. abdicating the throne, the prince and princess of Orange were declared king and queen of England. In geometry, the revolution of any figure, is its motion quite round a fixed line, as an axis.
>
> The revolution of a planet, or comet, round [the sun], is nothing but its course from any point of its orbit till its return to the same. (*Encyclopaedia Britannica*, 1771, p. 550)

Arguably, the first time that the term 'revolution' was borrowed to describe scientific change was also in the seventeenth century. Sir William Temple (1628-1699), in an essay entitled "Of Health and Long Life", regards the development in the history of medicine from Hippocrates to William Harvey's work on the circulation of blood as the "great changes or revolutions in the physical empire" (Temple, 1731, p. 280).[2] Since the eighteenth century, it has become more and more popular that breakthroughs in science are characterised in terms of revolution.

> The famous book *Principia Mathematica* [by Newton] marked the beginning of a great revolution in physics. (Le fameux livre des Principes mathematiques de la Philosophie naturelle [de Newton] a été l'époque d'une grande revolution dans la Physique.) (Clairaut, 1754, p. 465)[3]
>
> We are on the verge of a great revolution in the sciences. Given the taste people seem to have for morals, belles-lettres, the history of nature and experimental physics, I dare say that before a hundred years, there will not be more than three great geometricians remaining in Europe. The science will stop short where the Bernoullis, the Eulers, the Maupertuis, the Clairauts, the Fontaines and the D'Alemberts will have left it…. We will not go beyond. (*Nous touchons au moment d'une graunde révolution dans les sciences. Au penchant que les esprits me paroissent avoir à la morale, aux belles-lettres, à l'histoire de la Nature & à la physique expérimentale, j'oserois presque assurer qu'avant qu'il soit cent ans, on ne comptera pas trois grands géomètres en Europe. Cette science s'arrêtera tout court, où l'auront lassé les Bernoulli, les Euler, les Maupertuis, les Clairaut, les Fontaine & les d'Alembert... On n'ira point au-delà*.) (Diderot, 1754, p. 5)
>
> It is not worth while, nor of use for our purpose, to trace the history of learning thro' its various revolutions in the later ages. (MacLaurin, 1748, p. 39)
>
> The very high character of Mr Lavoisier as a chemical philosopher, and the great revolution which, in the opinion of many excellent chemists, he has effected in the theory of chemistry, has long made it much desired to have a connected

[2] It is argued that the essay was probably written before 1686 (Woodbridge, 1940, p. 212).

[3] Clairaut's paper was read on 15 November 1747.

> account of his discoveries, and of the new theory he has founded upon the modern experiments written by himself. (Lavoisier, 1790, p. v)
>
> There have been few, if any, revolutions in science so great, so sudden, and so general, as the prevalence of what is now usually termed the new system of chemistry , or that of the Antiphlogistians, over the doctrine of Stahl, which was at one time thought to have been the greatest discovery that had ever been made in the science. (Priestley, 1796, p. 35)
>
> For we by no means find, even in those practical discoverers to whom, in reality, the revolution in science, and consequently in the philosophy of science, was due, this prompt and vigorous recognition of the supreme authority of observation as a ground of belief; this bold estimate of the probable worthlessness of traditional knowledge; and this plain assertion of the reality of theory founded upon experience. Among such discoverers, Copernicus must ever hold a most distinguished place. (Whewell, 1847, p. 208)

In the nineteenth century, scientific revolutions and political revolutions were often analysed and examined together. For example, Henri Saint-Simon (1858) suggested that scientific revolutions often alternate with political revolutions in history, while Charles Renouvier (1864) argued that scientific revolutions and political revolutions are analogous in the sense that both occur in order to clarify social contracts. As Warren Schmaus (2021, p. 2) points out, we can often find "the analogy between scientific revolutions and political revolutions" in the nineteenth century writings.

In the first half of the twentieth century, the concept of the Scientific Revolution was introduced to designate a series of scientific changes in the sixteenth and seventeenth century (Burtt, 1925; Koyré, 1939; e.g. Robinson, 1921; Smith, 1930). Martha Ornstein was probably the first to explicitly use the term "the scientific revolution" in such a way.

> The first half [of the seventeenth century] seems more like a "mutation" than a normal, gradual evolution from previous times. It accomplished through the work of a few men a revolution in the established habits of thought and inquiry, compared to which most revolutions registered in history seem insignificant. (Ornstein, 1913, p. 30)
>
> I spoke of two groups of reformers who produced the scientific revolution of 1600-1650, the scientists and philosophic propagandists. (Ornstein, 1913, p. 51)

In the following four decades, there was an increasing interest in the Scientific Revolution among historians, especially historians of science. Herbert Butterfield's *The Origins of Modern Science 1300 – 1800* (1949) and A. Rupert Hall's *The Scientific Revolution 1500 – 1800* (1954) were among the most influential works on the Scientific Revolution at the time. Meanwhile, the concept of scientific revolution (or, "revolutions in science" in Cohen's words) was slowly developed in various case studies (Dampier, 1929; Koyré, 1939; e.g. Randall, 1926). Nevertheless, it is worth noting that, as Cohen (1985, p. 400) indicates, "[d]espite the frequent occurrence of the theme of revolution, it should not be concluded that, during the first half of the twentieth century, historians, historians of science, and scientists generally came to recognize the existence of the Scientific Revolution and to use it as an organizing principle, or that they all conceived of scientific change in terms of revolution".

It is Thomas Kuhn's *The Structure of Scientific Revolutions* (1962) that made the concept of scientific revolution generally accepted within and beyond the history of science. Kuhn's concept of scientific revolution is broader than his predecessors', such as Butterfield's and Hall's. 'Scientific revolution', for Kuhn, refers to any radical scientific changes in history rather than a particular historical episode. This is one of Kuhn's important contributions to the historiography of science: "transforming a growing scholarly concern for a single-scale Scientific Revolution into a research program directed toward individual smaller-scale revolutions in the sciences" (Cohen, 1985, p. 403).

2. An Overview of the Main Philosophical Analyses of Scientific Revolutions

One of the most influential philosophical accounts of scientific revolutions was developed by Karl Popper in his *The Logic of Scientific Discovery*.[4] For Popper, a scientific revolution is a process of the falsification of a theory and its replacement by another. Scientific theories here are defined as "universal statements" (Popper, 1959, p. 59). In other words, a scientific revolution is basically a shift from a "theoretical system" (i.e. a system of universal statements) to another (Popper, 1959, pp. 71–72, 86–87). Popper (1963) maintained that a scientific revolution from a theory (t_1) to another (t_2) marks a better approximation to truth, in which t_2 has a greater verisimilitude than t_1 in one of the following senses.

> (a) the truth-content but not the falsity-content of t_2 exceeds that of t_1,
>
> (b) the falsity-content of t_1, but not its truth-content, exceeds that of t_2. (Popper, 1963, p. 233)

As a contemporary of Popper, Ernest Nagel also viewed a scientific revolution as a shift from a theory to another. He also regarded scientific theories as systems of statements (Nagel, 1961, pp. 88–89). In addition, both Nagel (1961) and Popper (1963) contend that in a scientific revolution from a theory (t_1) to another (t_2), t_2 typically has a greater explanatory power than t_1. Nevertheless, Nagel's account of scientific revolutions differs from Popper's in one crucial aspect: Nagel argued for the reductive nature of scientific revolutions.

> [I]n any case, the phenomenon of a relatively autonomous theory becoming absorbed by, or reduced to, some other more inclusive theory is an undeniable and recurrent feature of the history of modern science. There is every reason to suppose that such reduction will continue to take place in the future. (Nagel, 1961, pp. 336–337)

For Nagel, a scientific revolution is basically a process of the reduction of a theory (t_1) to another (t_2) in the sense that all the laws of t_1 are logically derivable from the laws of t_2. Nagel distinguishes two types of inter-theoretic reduction: homogeneous reduction and heterogeneous reduction. The former happens when the reduced theory does not contain any term which is not employed in the reducing theory (e.g. the shift from Kepler's law to Newton's laws), while the latter occurs when the reduced theory contains some terms which are not employed in the reducing theory (e.g. the shift from thermodynamics to statistical mechanics).

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[4] The book was originally published by Springer in German in 1935, which was entitled *Logik der Forschung. Zur Erkenntnistheorie der modernen Naturwissenschaft*. It was translated into English and published by Hutchinson & Co. in 1959.

Kuhn (1962, 1970b) challenged these theory-based analyses and developed an alternative approach to scientific revolutions.[5] He paid more attention to the detail of the history of science. He characterised scientific revolutions as paradigm-shifts. A paradigm, in a broad sense, is defined as a disciplinary matrix shared by a scientific community, usually encompassing universal generalisations, models, values, and exemplars.[6] The main task for scientists working in a paradigm is puzzle-solving. Kuhn (1970a) argued that the new paradigm typically has a greater puzzle-solving power than the old one in a scientific revolution. Kuhn argued that different paradigms often differ radically in their universal generalisations, models (or ontological commitments), values, and exemplars as well as research problems and methods. Due to these differences, there is a difficulty of comparing two successive paradigms in a scientific revolution. (This is Kuhn's incommensurability thesis.[7]) Thus, an important consequence of the incommensurability thesis is that the new paradigm sometimes loses certain puzzle-solving capacities, even though it in general solves more puzzles than the old one. For example, in the chemical revolution, the oxygen theory failed to offer a good explanation of the common feature of metals, which could be well accounted for by the phlogiston theory. (Such a phenomenon is called Kuhn loss.)

As Yafeng Shan argues (2020b, pp. 383–386), Kuhn's approach to scientific revolutions was novel at the time in at least two ways. First, it was really novel to analyse and examine the history of science in a way which is not framed by theories. Without argument, philosophers such as Popper and Nagel, used to analyse scientific knowledge and the history of science in terms of theories. Theories were taken for granted as a unit of analysis to examine scientific revolutions. It is Kuhn who first made philosophers seriously reconsider the legitimacy of the use of theory as the unit of analysis in the philosophical examination of the history of science. Kuhn (1970b, p. 182) insightfully pointed out that "scientific theory" is not an ideal conceptual tool to study the history of science, because it "connotes a structure far more limited in nature and scope than the one required". Second, it was novel to highlight the discontinuous and non-rational elements of scientific revolutions in terms of incommensurability. Kuhn challenged the once received view that two successive paradigms in a scientific revolution can be simply comparable. For example, Popper (1963, p. 233) assumed in the definition of verisimilitude that "the truth-content and the falsity-content of two theories t_1 and t_2 are comparable", while Nagel (1961, p. 345) was explicit on the point that theories in a reduction are comparable in the sense that they "must be available as explicitly formulated statements, whose various constituent terms have meanings unambiguously fixed by codified rules of usage or by established procedures". However, these claims were questioned by Kuhn. He doubted that scientific terms (e.g. caloric) in an old paradigm can be neatly translated into a new one without any loss. Kuhn was also sceptical of the view that scientific revolutions can be simply explained

[5] It should be noted that neither Popper nor Nagel explicitly spoke of "scientific revolution" despite their important work on the nature of scientific change. The concept of scientific revolution achieved general acceptance in the thinking of philosophers of science only after the publication of Kuhn's book.

[6] Kuhn (1970b, pp. 181–191) distinguished two senses of paradigm. A paradigm in a narrow sense means an exemplar, which is defined as a puzzle-solution.

[7] For an in-depth analysis of Kuhn's incommensurability, see Sankey (1994).

by some universal standard of rationality (e.g. Popper's falsifiability criterion and Nagel's criteria of reduction).[8]

Under the influence of Kuhn's work, philosophers began developing more historically-informed accounts of scientific revolutions. Imre Lakatos (1968, 1978) developed an account of scientific revolutions in terms of research programmes, illustrated by two historical examples. For Lakatos, a research programme basically consists of a (theoretical) hard core and a set of auxiliary hypotheses. A scientific revolution is just a process of "one research programme superseding (overtaking in progress) another" (Lakatos, 1970, p. 99). Lakatos argued that the superseding research programme (P_1) should be more progressive than the superseded one (P_2) in the sense that P_1 generates more novel and corroborated predictions than P_2. Thus, a scientific revolution, for Lakatos, is a both rational and progressive move.

To some extent, Lakatos (1978, pp. 89–93) synthesised the Popperian falsificationism with the Kuhnian historiography of science. On the one hand, he defended the Popperian view that any scientific revolution has been and should be fundamentally a rational shift. On the other hand, he agreed with Kuhn on the role of non-rational factors in the history of science. He also shared with Kuhn the view that the unit of analysis in the philosophical examination of scientific change is something much more complicated than a theory (as a set of universal statements). In his words, "the basic unit of appraisal must be not an isolated theory or conjunction of theories" (Lakatos, 1970, p. 99).

Despite the substantial differences between their philosophical analyses, there are still two central theses concerning scientific revolutions shared by Popper, Nagel, Kuhn, and Lakatos.

> T1. The nature of a scientific revolution is a process of the replacement of some scientific consensus by another.
>
> T2. A scientific revolution is *ipso facto* a progressive process.

Larry Laudan (1977) rejected both by offering a novel account of scientific revolutions in terms of research tradition.

> A scientific revolution occurs when a research tradition, hitherto unknown to, or ignored by, scientists in a given field, reaches a point of development where scientists in the field feel obliged to consider it seriously as a contender for the allegiance of themselves or their colleagues. (Laudan, 1977, p. 138)

A research tradition is defined as "a set of general assumptions about the entities in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing in theories in that domain" (Laudan, 1977, p. 81). Laudan's "research tradition" differs from Kuhn's "paradigm" and Lakatos's "research programme" in one significant way: even some of the most basic elements of a research tradition can change. For both Kuhn and Lakatos, if there are some fundamental changes of a paradigm or of the hard core of a research programme, it leads to the establishment of a different paradigm or research programme and symbolises the emergence of a scientific revolution. However, for Laudan (1977, p. 98), it is a

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[8] It is worth highlighting that one should not confuse irrationality with non-rationality. Kuhn (1970b, p. 175) explicitly denied that he tried to argue that science is 'a subject and irrational enterprise', though he highlighted the role of non-rational factors in the history of science.

“misleading” way to characterise these changes as examples of scientific revolutions. Laudan believed in “a natural evolution in the research tradition”: “the core assumptions of any given research tradition are continuously undergoing conceptual scrutiny” (Laudan, 1977, p. 100).[9]

Contra his predecessors, Laudan did not construe the nature of a scientific revolution as a shift of scientific consensus. For Laudan, a scientific revolution means that there is a new research tradition which cannot be ignored and has to be taken seriously by the scientific community. But it does not imply that the old research tradition must be abandoned or replaced by the new one. In addition, Laudan doubted that a scientific revolution is inherently progressive. For him, whether a scientific revolution is progressive is historically contingent: “Scientific revolutions can occur even when it is entirely irrational or nonrational considerations which bring a new research tradition to everyone’s attention” (Laudan, 1977, p. 138).

The 1960s and 1970s are the heyday of the philosophical debate over scientific revolutions. Since the 1980s, philosophers of science have become more interested in other topics such as scientific realism and scientific explanation. That said, scientific revolutions is still an important issue in contemporary philosophy of science. There have been some attempts to examine and explore the pattern, nature, and implications of scientific revolutions for the past few decades (Chang, 2012; Darden, 1991, 2005; e.g. Kitcher, 1984).[10] As yet it has been no consensus on what is the best way to characterise the unit of analysis in the historiography of scientific revolutions.[11] Nor is there a consensus on what best explains the pattern and nature of scientific revolutions. The historical and philosophical implications of scientific revolutions are under debate.[12] Despite the substantial differences, these philosophical analyses share some historiographical assumptions about the unit of analysis and the pattern of scientific development. In the remaining of this chapter, I would focus on scrutinising these historiographical assumptions.

Table 1. Main Accounts of the Unit of Analysis in the Philosophical Analyses of Scientific Revolutions
Thought styles (Fleck, 1935)
Theory (Darden, 1991; Nagel, 1961; e.g. Popper, 1959)
Paradigm-as disciplinary matrix (Kuhn, 1962, 1970b)

[9] Note that Laudan did not deny that a research tradition has some “essence” or “nonrejectable elements”, but he tried to emphasise that “the elements constituting this class can shift through time” (Laudan, 1977, pp. 99–100). This view, he argued, better captures the history of science.

[10] I do not have room to discuss these accounts in detail here, unfortunately. Most of these new accounts can be construed as revised or integrated versions of some earlier accounts discussed in this section. Philp Kitcher (1984, 1989), for example, developed a reductionist account of revolutions in genetics by incorporating some elements of Kuhn’s approach (e.g. the significance of problems). Lindley Darden’s analysis of the molecular revolution (2005) can be viewed as a variant of the application of the Laudan’s account of scientific revolutions, while Hasok Chang’s account of the chemical revolution (2012) is to a great extent Kuhnian.

[11] For a list of main philosophical accounts of the unit of analysis, see Table 1.

[12] For a list of main philosophical accounts of scientific revolutions, see Table 2.

Research programme (Lakatos, 1968; Musgrave, 1976)
Research tradition (Laudan, 1977)
Field (Darden, 2005; Darden & Craver, 2002; Darden & Maull, 1977)
Practice (Kitcher, 1984, 1989)
Style of reasoning (Crombie, 1994; Hacking, 1994)
System of practice (Chang, 2012, 2014)
Scientific perspective (Giere, 2006; Massimi, 2018)
Exemplary practice (Shan, 2020a, 2020b)

Table 2. Main Philosophical Accounts of Scientific Revolutions

Consensus-shift	Theory-shift	Theory falsification (Popper, 1959)	Rational and progressive
		Theory reduction (Nagel, 1961)	Rational and progressive
	Paradigm-shift	Incommensurable replacement (Kuhn, 1970b)	Rational in general but with some non-rational factors and progressive
	Research programme-shift	Programme replacement (Lakatos, 1978)	Rational and progressive
	Explanatory schema-shift	Explanatory extension/unification (Kitcher, 1984, 1989)	Rational
Consensus-recognition	Research tradition-recognition	Tradition acceptance (Laudan, 1977)	Contingently rational
	Field-recognition	Field-discovery (Darden, 2005)	N/A

3. The Unit of Analysis Reconsidered

Despite the substantial differences, the philosophical accounts all assume that scientific revolutions are basically about the changes of scientific consensuses: either the replacement of a scientific consensus by another or the recognition of a scientific consensus. In other words, the unit of analysis in the philosophical examination of scientific revolution is a scientific consensus. Thus, in order to examine the nature and pattern of scientific revolutions, it is an indispensable task to provide an account of scientific consensus. As I have shown in the previous section, theory (as a unit of analysis) was taken for granted to describe scientific consensus until the 1960s when philosophers began realising that scientific revolutions also involve significant non-theoretical changes (e.g. problems, methods, and experimental procedures). Thereafter, new units of analysis have been developed in order to capture the non-theoretical elements of a scientific consensus: paradigm, research programme, research tradition, field, system of practice, etc. It is worth noting that all these units of analysis all assume that the essential elements of a scientific consensus consist of something general or universal, invariantly shared by the members of a community. For example, a paradigm consists of some universal generalisations, while a research programme has a hard core. I call these essential elements "macro-scientific consensus".

However, there is a persistent problem for these units of analysis, namely, the problem of identification: it is a difficult task to identify macro-scientific consensus in the history of science. Let us consider an example. It is widely received that Mendelian genetics is the first consensus in the history of modern genetics (e.g. Darden, 1991; Shan, 2021; Waters, 2004), but it is not easy to identify the essential elements of Mendelian genetics. As Darden (1991) and Shan (2020a) have shown, there were so many radical theoretical and non-theoretical developments that very few was invariantly shared from Mendel's theory of hybrid development (1866) to Morgan's theory of the gene (1926). Thus, it is difficult to identify the macro-scientific consensus that essentially constitutes Mendelian genetics, whether it is characterised in terms of theories, paradigms, or research programmes.

It should be highlighted that the problem here is not that it is misleading to characterise scientific revolutions by examining the changes of scientific consensuses. Rather, I argue that it is misleading to characterise scientific revolutions by focussing on macro-scientific consensuses. A simple solution to the problem of identification is to shift attention from macro-scientific consensuses to, what I call, micro-scientific consensuses, which is something local and context-dependent. Mendel's work on the development of pea hybrids (1866) is such a good example of micro-scientific consensuses. It was widely accepted by early Mendelians in the 1900s and 1910s. As Shan (2020b, 2020a) argues, what was accepted is Mendel's particular way of problem-defining, problem-refining, problem-specification, experimentation, conceptualisation, hypothesisation, and reasoning. All the early Mendelians accepted that Mendel's work provided a reliable framework for further investigation of the problem of heredity, though many of them were still sceptical of the generality of Mendel's laws of development or his concept of dominance. And I argue that most scientific consensus is in this micro sense rather the macro sense. I would like to emphasise that I am not denying that there are some maro-scientific consensuses in the history of science. Newton's three laws of motion are such examples. However, I have to note that in most cases, it is difficult to formulate a version of macro-scientific consensus which is widely accepted by the members of a scientific community. For example, it is plausible to identify the theory of natural selection by evolution as a marco-scientific consensus in the twentieth century evolutionary studies, but it is extremely difficult to articulate or formulate the theory of natural selection by evolution in a way that biologists all accepted throughout time. As Laudan (1977, p. 74) indicates, it is a historical fact that "the fundamental assumptions of every [scientific consensus] are debated within the scientific community". Even if in some cases, we can roughly distinguish two macro-scientific consensuses in a scientific revolution, we still have to the distinction is not a clear cut. For example, as Chang (2012, pp. 19–22) points out, the phlogistonist and oxygenist systems share some important research problems, though they differ in some. Therefore, I argue that a unit of analysis focussing on marco-scientific consensus is not very helpful to analyse and examine the nature and pattern of scientific revolutions in history.

In contrast, I argue that that one ought to analyse and examine the nature and pattern of scientific revolutions by focussing on micro-scientific consensuses rather than macro-scientific consensuses. As Shan (2022) argues, scientific change is better analysed in terms of exemplary practices. An exemplary practice is defined as a particular way of problem-defining and problem-solving, typically by means of problem-refining, conceptualisation, hypothesisation, experimentation, and reasoning (Shan, 2020b, 2020a). Accordingly, I propose that exemplary practice can be adopted as a new unit of analysis in the philosophical examination of scientific revolutions. Such an approach has an obvious advantage over the traditional ones with a focus on macro-scientific consensuses: it better captures the complexity and nuance of the history of science. In particular, it is not undermined by the problem of identification. As I have shown in Mendel's case, it is not very difficult to identify the micro-scientific consensus among the members of Mendelian genetics. Early Mendelians did differ in the formulation and interpretation of the Mendelian laws, but they all accepted Mendel's exemplary practice, which provides conceptual tools, experimental guidelines, and research problems for the study of heredity. Thus, I call for a modification of the historiographical assumption concerning the unit of analysis: we ought to shift our attention from macro-scientific consensuses to micro-scientific consensuses.

4. Scientific Development Reconsidered

There are two models of scientific development underlying the philosophical analyses of scientific revolutions. In this section, I argue that neither provides a good historiographical framework to examine scientific revolutions.

4.1 Single-line Vs. Multi-line Models of Scientific Development

Most philosophical analyses of scientific revolutions (Kitcher, 1989; Kuhn, 1962; e.g. Popper, 1959) assume a particular model of scientific development, what I call the single-line model. According to the single-line model, the development of science is a process of alternating one episode with another, and scientific revolutions are just episode-shifts.[13] As illustrated by Figure 1, the development of science is a lineage of episodes E_0, E_1, E_2, …, E_n. For instance, the development of genetics in its early period is often characterised as the

[13] Many discussions in philosophy of science rest on this model. An obvious example is the debate between scientific realism and anti-realism. One of the central issues in that debate is whether there is any theoretical component which is preserved throughout theory-change (e.g. Laudan, 1981; Psillos, 1999; Stanford, 2006; Worrall, 1989). Both realists and anti-realists implicitly agree on a historiographical assumption that scientific development is a process of alternating one theory with another, though differ in the question of whether there is something substantial preserved in theory-change.

lineage from Mendel's theory (1866), de Vries' theory (1900a), Bateson's theory (1902), to Morgan's theory (1926).

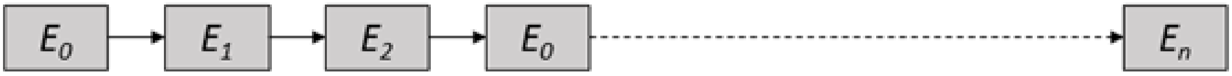

Figure 1. The Single-line Model of Scientific Development

One key feature of the single-line model is the dominance of one consensus in the "normal" period. For example, in the Kuhn's account (1962), when a scientific revolution is over, a paradigm dominates in the sense that most of the scientists in the field tend to work within it and other paradigms either die out or are marginalised. In other words, the single-line model eliminates the possibility of co-existence of two or more consensuses in a long run. However, counterexamples abound in the history of science. For examples, in the late nineteenth and early twentieth century, there were over twenty theories of heredity and none of them dominated the study of heredity (Delage, 1903). As Laudan (1977, p. 74) observes, "Virtually every major period in the history of science is characterized […] by the co-existence of numerous competing [scientific consensuses], with none exerting hegemony over the field."[14]

Other philosophical analyses of scientific revolutions (e.g. Lakatos, 1968, 1978; Laudan, 1977) assume an alternative model, what I call the multi-line model. According to the multi-line model, scientific development is a process of the evolution of multiple consensuses, as illustrated by Figure 2.

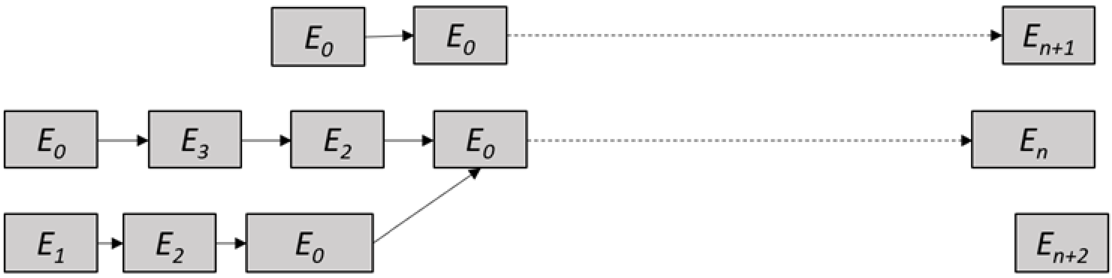

Figure 2. The Muti-line Model of Scientific Development

A crucial difference between the single-line model and the multi-line model is that the multi-line model is open to the possibility of the co-existence of multiple consensuses in a long run. However, the multi-line model is still problematic, from a historical point of view. It is not unusual for co-existent consensuses to interact with each other. The development of co-existent theories is not independent of each other. Sometimes co-existent consensuses integrate, while they even diversify at other times. Consider the case of the origin of genetics.

[14] Lakatos (1978, p. 69) made a similar point: "The history of science […] has not been and must not become a succession of periods of normal science."

As many (Bowler, 1989; Müller-Wille, 2021; e.g. Olby, 1985) have shown, the development of genetics, even if we only focus on the theoretical aspect, is more like a web of interacting consensuses than a lineage of successive consensuses. Although Gregor Mendel is widely regarded as the founder of genetics, his theory of hybrid development is not the only origin. The problem of heredity was Charles Darwin's central concern, because he was looking for a mechanism of heredity to account for the reservation of the favoured traits by natural selection. Darwin's theory of pangenesis was the origin of the mainstream study of heredity in the second half of the 19th century, which heavily influenced Hugo de Vries' work on pangenesis (1889). The development of cytology in the 19th century provides another origin for the study of heredity. Initially inspired by Darwin's theory, August Weismann's germ plasm theory (1892) made an incorporation of the study of cell and of heredity, which influenced Correns' reformulation of Mendel's rule (1900). In the first decade of the 20th century, the newly established Mendelian theory of heredity, mainly developed by Bateson (1902), was rivalled with the Biometrician theory of heredity, initially proposed by Francis Galton (1889) but mainly developed by W. F. R. Weldon (1905) and Karl Pearson (1903), which nevertheless was synthesised into the study of heredity by the modern synthesis decades later. At the same time, the development of the chromosome theory provides another important source for T. H. Morgan and his associates to develop a much more sophisticated theory of inheritance in 1910s and 1920s. Thus, the development of early genetics involves the interaction of various consensuses across different areas. In other words, the multi-line model still fails to capture the complexity and interactivity of scientific development.

4.2 The Web of Scientific Development

I argue that the pattern of scientific development is more like a synthesising-web. Reconsider the origins of genetics. De Vries' theory (1900b) incorporated the parts of Darwin's and Mendel's theories. Bateson's theory was developed based on de Vries' and Correns' theories. Morgan's theory was somehow a synthesis of the chromosome theory and the Mendelian theory. Overall speaking, the pattern is more like a synthesising web than a lineage or multiple lineages. It should be noted that synthesising is not a simple process of integrating consensuses. As I have shown (2020b), what de Vries learnt from Mendel were the focus on a pair of differing traits, the conceptions of dominance and recessiveness and their statistical relation, the morphological-cellular correspondence, and the mathematical approach. In addition, de Vries' synthesis of Darwin's and Mendel's was more than a theoretical integration. It encompasses a creative and selective attempt to introduce a way of defining and solving the problems of heredity on the basis of Darwin's and Mendel's exemplary practices. Accordingly, as Shan (2020a) argues, Mendel's contribution can be well characterised as the introduction of an exemplary practice, whose components were selectively adopted with modifications by de Vries, Correns, and Tschermak to develop their exemplary practices. Thus, the origins of genetics are better characterised as a web of synthesising exemplary practices, as illustrated by Figure 3. Moreover, I contend that not only is the development of early genetics, but also scientific development in general is better characterised as a synthesising-web.

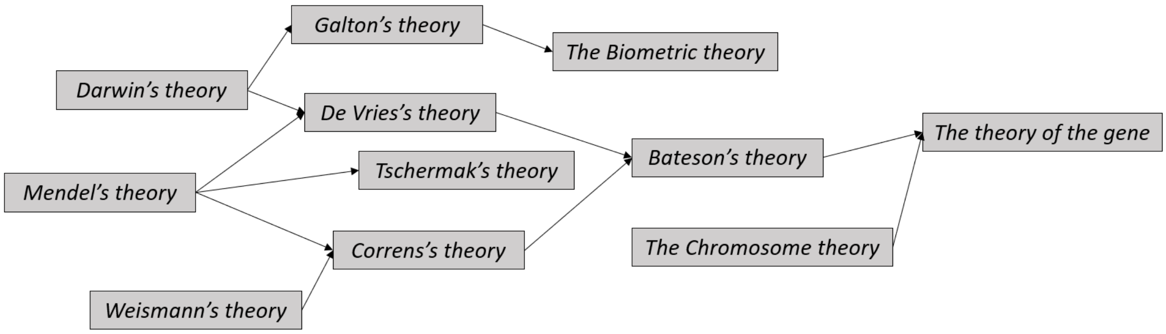

Figure 3. A Partial Picture of the Origins of Genetics

It is clear that the web model is better than the single-line and the multi-line models in the way that its two-dimensional structure better captures the complexity of scientific changes in history. Moreover, it does not assume, like the single-line and multi-line models, a dominating scientific consensus typically prevails in any period of the history of science. Rather the web well characterises the plurality and interactivity of scientific inquiries in any given period.[15] Therefore, I argue that a good philosophical examination of scientific revolutions should assume the web model of scientific development.

5. Conclusion

In this chapter, I have reviewed the historical development of the concept of scientific revolution and the five main philosophical accounts of scientific revolutions. Moreover, I have revisited two historiographical assumptions behind these accounts. I have argued that the historiographical assumption concerning the unit of analysis is flawed in the way that it pays too much attention to macro-scientific consensuses. I have also argued that neither the single-line nor the multi-line models of scientific development provide a good framework to analyse scientific revolutions. I proposed that the unit of analysis should be a description of micro-scientific consensuses rather than macro-scientific consensuses, as the former better captures the complexity of the history of science. In addition, I suggested that a good philosophical examination of scientific revolutions should abandon the single-line and multi-line models of scientific development and assume the web model.

[15] Some may notice that Kuhn's late writings of scientific change also suggest a non-linear model of scientific development. Most famously, as Wray (2011, 124) elaborates, "the history of science consists of periods of normal science punctuated by either (1) episodes of theory change, that is, scientific revolutions, or (2) episodes of specialty formation, where a new branches off from a parent field." Surely this late Kuhnian specialisation model is more sophisticated than both the single-line and the multi-line models. However, the specialisation model has two problems. Firstly, it implies that there is a "common ancestor" of all scientific theories. This is really dubious. Secondly, the specialisation fails to well account for the multiple roots of a historical episode. The late Kuhn is correct that specialisation is an important type of scientific change. However, the specialisation model does not articulate the process and mechanism of specialty formation. In contrast, the web model provides a good framework to analyse the mechanism of specialisation. And the specialisation model can viewed as a special case of the web model.

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