Revolutionary Analogies

Francesco Nappo (Politecnico di Milano)

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Abstract: This paper discusses the role of analogy in scientific revolutions. A challenge to Kuhn's philosophy of science emerges from this study. On the one hand, the fact that many new paradigms (understood here as 'exemplars') have borne out of analogies is partly explained by the fact that, compatibly with Kuhn's requirement of 'open-endedness' or 'fruitfulness' on paradigms, they contained immediately graspable indications as to their extensions to novel domains of phenomena. On the other hand, the consideration of scientific exemplars possessing an analogical origin also suggests that new exemplars may be successful partly as the result of drawing upon a common background of familiar experiences and training among competing scientific parties. It follows that, for reasons possibly endemic to Kuhn's conception of science, scientific revolutions are unlikely to display the forms of incommensurability that Kuhn famously ascribed to them.

Keywords: analogy; scientific revolutions; normal science; incommensurability.

1. Introduction

The role of analogy in science is one of the central themes of Kuhn's *The Structure of Scientific Revolutions* (*SSR*). Kuhn develops this theme specifically in relation to his discussion of 'normal science'. On Kuhn's account, scientific research properly so called begins with the establishment of a "paradigm" or "exemplar" (1970:187): a scientific achievement whose problem-solving capacity was so striking to be elevated to the status of "shared example" (187). Scientific training and research then consist mainly, on this view, in acquiring and refining a capacity to extend the exemplar to new problem situations (1970:36). Such extensions are not rule-based. They depend, in other words, not on the algorithmic application of some precisely formalizable principle, but on a more visceral and hard-to-analyze capacity to recognize similarities between cases already covered by the exemplar and others not yet covered. This essentially analogical way of proceeding, which Kuhn (1970:189) compares with learning to solve an exercise at the end of a textbook chapter, is at the heart of scientific training and research during normal science.

A theme that is less prominent in Kuhn's works, and that this paper aims to analyze, is the role of analogy in scientific revolutions. Newton's analogy between the physics of terrestrial and celestial bodies, which gave rise to his law of gravitation, J. Clerk Maxwell's analogies with hydrodynamics and the mechanics of wheels, which gave rise to the new 'field' conception of electromagnetic and optical phenomena, and Darwin's analogy between artificial selection and the workings of nature, which gave rise to the hypothesis of natural selection, are just a few notable examples. In each case, the questions and puzzle-solving methods originating from the study of a familiar source domain eventually replaced the scientific community's old questions and methods in a given target system.[1] In short, the analogy gave rise to a new paradigm – understood here not in the broad sense of what, in the *Postscript*, Kuhn calls a "disciplinary matrix" (1970:182), i.e., a community's shared set of "symbolic generalizations", "values", "beliefs in… models", and "exemplars" (181-6), but in the strict sense of "exemplar" (186), i.e., an instance of puzzle-solving that sets the standard for future research in a given field.[2]

[1] Further notable examples include Huygens's light-sound analogy in the wave theory of light and Carnot's hydraulic analogy to heat engines laying the foundations of thermodynamics. See also Nappo (2020) for a discussion of Francis Galton's work on population biology and the role of his 'quincunx' machine.

[2] The distinction between paradigm as disciplinary matrix and as exemplar is from Kuhn's Postscript to *SSR*. By "belief in… models" there, Kuhn mainly refers to those shared ontological commitments that, in *SSR*, he had called "the metaphysical parts of paradigms" (1970:184): e.g., the view of heat as molecules in motion.

This paper argues that 'revolutionary analogies' (viz., analogies that successfully gave rise to new exemplars) present a challenge to Kuhn's account of scientific change. In support of this claim, the case-study of Maxwell's revolution in electromagnetism will be analyzed in detail.

The challenge can be briefly stated as follows. On one hand, the fact that so many new exemplars were borne out of analogies may be partly explained by the fact that they provide immediately graspable indications for extending exemplars to novel domains of scientific interest. As will be discussed, this account of what tends to lead scientists away from old exemplars and towards new ones resonates well with Kuhn's requirement of "open-endedness" (1970:10) or "fruitfulness" (1977:322) on scientific exemplars. On the other hand, the consideration of scientific exemplars possessing an analogical origin also suggests that new exemplars may be successful partly as the result of their drawing upon a common background of experience and training – making it possible for the reasons of the revolutionary faction to be *accessible* to their opponents. Reflection on the role of analogy in successful exemplar replacement therefore imposes a considerably more careful attitude than Kuhn's in associating scientific revolutions with the phenomenon of incommensurability. This holds not only for ascriptions of incommensurability in meaning, already discussed in Kitcher's (1983) and Sankey's (1994) works, but also for those of incommensurability in standards that some recent authors (e.g., Bird 2008) view as a fallout of the role of analogical thinking in science.

The discussion below is organized as follows. Section two considers Kuhn's claims and omissions regarding the role of analogies in scientific revolutions, drawing a link to his claims on the value of fruitfulness. Section three presents the paper's main case-study: the use of physical analogies in Maxwell's articles 'On Faraday's Lines of Force' and 'On Physical Lines of Force' (1890a). This will help illustrate how, by using familiar physical domains as models, Maxwell succeeded in establishing a new way of looking at electromagnetic phenomena and of solving problems in that science. Section four and five provide a critical assessment of Kuhn's account of scientific revolutions, casting doubt on the idea that the establishment of Maxwell's electromagnetic theory occurred as the result of a fight of incommensurable viewpoints. Section six clarifies the depth of the proposed challenge to Kuhn's account of scientific change and connects the present discussion to the broader question of Kuhn's philosophical legacy.

2. The Analogical Route to Normal Science

SSR is a landmark of twentieth-century philosophy of science. The analysis of scientific revolutions is widely regarded as one of its most novel and influential aspects (Shan 2020). On *SSR*'s account, the "defining characteristics of scientific revolutions" (1970:6) are that they: *i)* "necessitated the community's rejection of one time-honored scientific theory in favor of another", *ii)* "produced a consequent shift in the problems available for scientific scrutiny", and *iii)* "transformed the scientific imagination" (6). This account covers what Newton did to physics and what Darwin did to biology – although, as several readers have noted, *SSR* does not discuss *Origin of Species* at length. The same account also covers Maxwell's electromagnetic equations, which, Kuhn argues, "for the far smaller professional group affected by them, …were just as revolutionary as Einstein's" (Kuhn 1970:7). In *SSR*, Kuhn uses Maxwell's example to specifically illustrate how the theoretical development of a paradigm – the Newtonian in this case – "ultimately produced a crisis for the… paradigm from which it had sprung" (1970:74).

However, one aspect that is rather systematically omitted from *SSR*'s treatment concerns the origin of replacing exemplars in a scientific revolution: as Nickles (2012) puts it, "where do new [exemplars] initially come from?" (120). In *SSR*, only a brief comment is made that "new paradigms are born from old ones” (1970:149) but no elaboration of the claim is provided. Later works by Kuhn similarly contain little or no indications on the genesis of successful exemplars.

Margaret Masterman’s ‘The Nature of a Paradigm’ (1965) was one of the first to delve into Kuhn’s omission. On Masterman’s view, the fact that many successful exemplars have emerged from suggestive analogies is no mere historical curiosity. For an emerging exemplar to successfully replace old scientific habits and cognitive structures, she argued, a change in the material conditions of scientific activity is required. Exemplar replacement is therefore more likely to be successful when new exemplars provide what she calls a “crude analogy” (1965:79): “either, literally, a model; or, literally, a picture; or, literally, an analogy-drawing sequence of word-uses in natural language” (79). The fact that old habits are more likely to change as the result of the introduction of a material artefact or of some other easily perceivable construct is what allows crude analogies to be “the trick, or device, which starts off any new science” (73). Pursuing Masterman’s argument, Pickering (1980) has proposed a new characterization of scientific exemplars that aims to refine Kuhn’s own: “an exemplar”, Pickering writes, “should be seen as the concrete embodiment of an analogy. […] The characteristic which distinguishes exemplars from other scientific achievements is their concrete demonstration of the possibility of reconceptualizing one area of research in terms of some other field of discourse” (490).

Readers sympathetic to Kuhn’s ideas may note that there is a way of reading Masterman’s and Pickering’s remarks as compatible with Kuhn’s account of scientific change. For an instance of problem-solving becomes exemplar, on *SSR*’s account, only if it shares two features: first, its achievements are “sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity”; second, it is “sufficiently open-ended to leave all sorts of problems for the… group of practitioners to resolve” (1970:10). The second condition, *open-endedness*, is relevant here. One reading of Kuhn's point is that new exemplars can only get established if they can make realistic promises to their adherents of success along the path indicated by them (cf. Hoyningen-Huene 1993:193). And while in principle any theory, including abstract mathematical formalisms having no analogies with any known system, can serve as an open-ended source of new puzzles to solve, in practice it may well be true that an exemplar originating in a familiar analogy may contain more immediately graspable indications as to its further extensions.[3] Kuhn's requirement of open-endedness may therefore help explain why many successful exemplar replacements in the history of science have analogical origin.

To illustrate this idea with historical examples, consider how naturally Descartes' analogy between light and the motion of projectiles indicates new experiments to be conducted on the properties of reflection and refraction, as well as on their mutual connections (cf. Sabra 1963). Or, to use an example from biology, consider how promptly Darwin's analogy between natural and artificial selection indicates both a new question – the nature of the mechanism of evolution – and the way to investigate it – study the effects of interventions on bred plants and animals (Kitcher 1993). In each case, the analogy can be regarded as an effective cognitive vehicle, during times of crisis, for pointing potential newcomers towards those questions that the exemplar raises but does not yet settle. In other words, there is a plausible (though by no means necessary) connection between the required function of emerging exemplars to indicate puzzles for the community to resolve and the analogical origins of new scientific exemplars. To put it in the language of the later Kuhn, insofar as the acceptance of a new exemplar may depend partly on its capacity to display its "fruitfulness" (1977:322) to potential adherents, it is to be expected that many successful exemplar replacements in history may crop up from familiar analogies.

[3] On the cognitive basis of the 'intuitiveness' of analogy in relation to scientific investigation, see Gentner & Jeziorski (1989), who appeal to an in-built cognitive bias for *systematicity* and quote James (1890).

Interestingly, the link between analogy and open-endedness figures explicitly in the writing of many notable scientists. James Clerk Maxwell discusses it with outstanding clarity in the case of theoretical physics. A recurrent critical target in his early electromagnetic works is the "mathematical method" (1890a:155) undertaken by many of his contemporaries in exploring electromagnetism. It consists in guessing, from the known laws and experiments about a given domain, the form of the equations from which those laws and results can be deduced. As Maxwell explains, this way of proceeding rarely brings fruit when applied to an immature stage of science (such as it was for electromagnetism at his times). For by trading all physical intuition for formal rigor "we entirely lose sight of the phenomena to be explained" (156). Hence, when a novel result in the domain of interest is discovered – one that cannot be accounted for by an application of the operations on symbols already devised –, one is left with no indication as to how to extend the old mathematical formalism to it. Lacking a physical example that guides the interpretation of the operations performed on the mathematical symbols, the researcher is therefore likely to get saddled in a "system of truth strictly founded on observation, but probably deficient both in the vividness of its conceptions and the fertility of its method" (156).

The alternative approach that Maxwell advocates for the early stages of scientific research is (as it happens) the method of "physical analogy" (156). It consists in borrowing the equations and problem-solving methods adopted in a familiar science to use as a kind of 'proto-theory' in a less familiar scientific domain. Maxwell's claim is that, by adopting a familiar physical model as a 'way of seeing' the phenomena regarding a domain of interest, the researcher can "present the mathematical ideas to the mind in an embodied form… and not as mere symbols" (175). This feature offers a key advantage when a physical theory has not been fully worked out that can cover the variety of observations and phenomenological laws already collected in the target

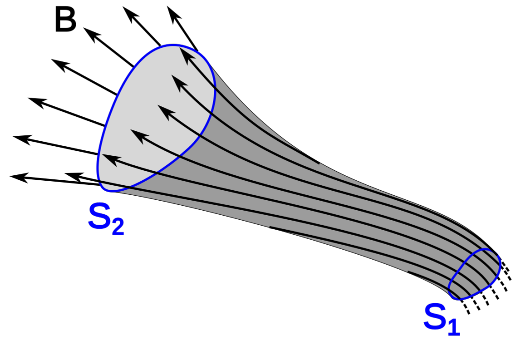

Figure 1 (left): Iron filings on paper surrounding a magnet display Faraday's 'lines of force'.
Figure 2 (right): A line of electric force viewed as a tube carrying an incompressible fluid.

system. For by "allowing the mind at every step to lay hold of a clear physical conception" (1890a:156), the analogy can serve as a guide in distinguishing physically meaningful from physically meaningless extensions of a mathematical formalism. Compared to the purely mathematical approach, then, by the method of physical analogy "the connexions of very different orders of phenomena may be clearly placed before the mathematical mind" (156).

Although (as stated above) these considerations might seem to support Kuhn's account of scientific change, and particularly his requirement of open-endedness on emerging paradigms, there is another sense in which the analogical origins of many new scientific exemplars pose a problem for Kuhn's philosophy of science – one which the requirement of open-endedness only contributes to sharpening. It takes off from the observation that many scientific exemplars have historically replaced old scientific habits partly as the result of exploiting analogies with systems that were eminently familiar to adherents of competing paradigms. The practice of breeding, for instance, was well known to defenders of creationism in Darwin's times (cf. Kitcher 1993:20); and so were the sciences of hydrodynamics and mechanics of wheels that, as we will see, Maxwell used as a model in developing his new electromagnetic equations. The recurrence of these historical examples suggests a general conclusion: that part of the success of new scientific exemplars may reside in their capacity to attract a highly diverse set of potential adherents, by relying on a common background of training and experience. In short: the closer the analogy is to home, the easier it becomes for a prospective exemplar to attract potential adherents.

The last observations put pressure on Kuhn's philosophy of science. Specifically, they indicate a tension between plausible constraints on successful exemplar replacement and the *incommensurability thesis*: the claim (which will be further specified in what follows) that there is no common language or standard by which one can neutrally justify preference of one paradigm over competing ones (cf. Kuhn 2000a:31). This is because, as the next sections will help illustrate, precisely the same analogies that can be invoked to display a new exemplar's open-endedness are often such as to bridge the conceptual and methodological differences between rival scientific schools. Consequently, it becomes questionable that, as Kuhn once wrote, "in a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds" (1970:150). The role played by analogies with eminently familiar domains in scientific revolutions rather suggests that, in at least some such occasions, it matters to full historical understanding that competing scientists practiced their trades in the same arena, knowing full well the exchange value of the goods each had to offer.

It may be worth clarifying how the challenge just outlined differs from the considerations that Masterman (1965) and Pickering (1980) advanced. Their point was that it is to be expected that new exemplars would emerge from analogies, since an exemplar must provide a concrete 'way of seeing' the phenomena. As discussed, Kuhn can account for (what may be right about) this observation by his condition of open-endedness. The argument proposed here is different: at least some of the revolutions that have occurred in history managed to attract adherents partly by appealing to a shared background of scientific training and experience. This observation challenges Kuhn's account insofar as it suggests that successful replacements of one exemplar by another are less likely to display the forms of incommensurability that Kuhn ascribed to them.

To summarize, we have now the outline of an argument for why scientific revolutions and incommensurability may come apart, based on the role of revolutionary analogies. The main burden now is to show that there are historical examples possessing the above features. To this effect, the next section will start delving into the case of Maxwell's electromagnetic revolution.

3. Crisis and the Emergence of Maxwell's Theory

Maxwell's first article on electromagnetism ,'On Faraday's Lines of Force' (1855-56), begins with the observation that the present state of the electromagnetic science "seems peculiarly unfavorable to speculation" (1890a:155).[4] Indeed, already by the end of the eighteenth century it was widely accepted that electrostatic forces among two charges at rest have repulsive or attractive character depending on the valence; moreover, that charges obeyed a law analogous to Newton's law of gravitation, in that the forces among them are inversely proportional to the square of their distances. According to one of the predominant theories at the time, electric charges are traversed by fluids that bring about differences in valence among charges.

However, electrostatics was not the only domain of phenomena to require an explanation. The problem was to find a theory that would encompass all known phenomena. For instance, electrostatic induction seemed to resist formal treatment. It was known that, if one placed a charged body near a metallic body sitting on an insulating stand, the metallic body would get charged. However, the distribution of charges in the body could only be quantified for certain

[4] The case-study to be discussed, Maxwell's early work in electromagnetism, is the subject of considerable historical disagreement. The presentation below is based on the reconstruction defended in Nappo (2021a).

shapes of bodies. Similarly, the connection between an electric current and the action of magnetism was only partially understood. André-Marie Ampère had shown that, for closed circuits, a stable relation obtains between magnetic force and electric currents. That was his law that the integral over a magnetic loop is equal to the sum of the electric currents that intersect that loop. However, the generalization to the case of open circuits was far from obvious.

Aiming for a fresh look at the subject, 'On Faraday's Lines' proposes to mathematize the notoriously un-mathematical ideas on electromagnetism by Michael Faraday. The main idea is that electrical and magnetic action unfolds along "lines of force" – as nicely illustrated by Faraday's experiment with iron filings (*Fig. 1*). As Maxwell notes, we can imagine electric and magnetic forces as if acting through a dense series of tubes filled with an incompressible fluid, where a source of fluid is the equivalent of an electric or magnetic point-charge. Of this imaginary system, we *stipulate* that it obeys mathematically analogous laws as those of electromagnetism. In particular, we know that the velocity of a fluid varies inversely as the cross-section of the tube. Hence, we can conceive of a series of special tubes, behaving as mere surfaces, such that the velocity of the fluid in the tube just happens to obey the same relation whereby electric and magnetic forces vary inversely as the *square* of the distance (*Fig. 2*).

The aim of 'On Faraday's Lines' is to show that many properties of electromagnetism can be accounted for under the assumption that electric and magnetic forces behave *as if* they were macroscopically equivalent to the system of tubes above. For instance, the quantity of electrostatic induction had been observed to vary depending on the interposed body: a metallic body is more prone to induction than an insulator. This property can be accounted for if one imagines that the nearby charged body generates lines of force carrying an incompressible fluid; and that, when the fluid encounters the interposed body, it passes through a medium of different resistance. In particular, the quantity of this resistance will be much greater in an insulator, causing the fluid to slow down in agreement with hydrodynamic laws. By means of a physical analogy, one could therefore "obtain physical ideas without physical theory" (1890a:156).

Having identified a macroscopic analogue of electromagnetic forces, the obvious next step is to identify an analogue at the micro-level: a conceivable mechanical system such that electrical and magnetic forces behave *microscopically* as if they were that system. That is precisely the task that Maxwell addresses in 'On Physical Lines of Force' (1861-63).[5] Maxwell starts from another experiment by Faraday, which indicated that magnetic action was responsible for rotating the plane of polarization of light. This suggested a representation of the cause of magnetic action as molecular vortices – the hexagonal cells in *Fig. 3*. The first part of 'On Physical Lines' shows that the micro-level description of the action of molecular vortices is compatible with the macroscopic description of magnetic action outlined in 'On Faraday's Lines'. The main expedient is to note that the system of molecular vortices can satisfy the laws of the system of lines of force if one assumes that each molecular vortex moves rapidly in the direction of its axis and that the centrifugal force accompanying the vortex motion generates a pressure in the equatorial direction, producing mechanical tension with nearby vortices.

In the second part of 'On Physical Lines', Maxwell addresses the problem of including the action of electricity into his model. The solution is suggested by the mechanical conditions of the system of vortices. In short, Maxwell notes that colliding molecular vortices would generate friction with one another, eventually setting all rotation to rest. To keep the mechanical system in motion, then, "idle wheels" (1890a:468) would be needed in the interstices among the vortices.

[5] A rival tradition reads in 'On Physical Lines' an attempt at articulating a mechanical hypothesis concerning the cause of electromagnetism (Kuhn 1970:74 also hints at this interpretation). As discussed in Nappo (2021a), the hypothetical methodology is foreign to Maxwell's approach. Cf. Achinstein (2018) and Bokulich (2015).

Maxwell's idea is that the motion of these idle wheels (represented by the small circles in *Fig. 3*) corresponds to the action of electricity. Whereas in an insulator, when the medium's resistance is high, the idle wheels can only rotate on themselves, in a conductor, when the resistance is low, they are free to travel from one place *A* to another *B* – the equivalent of a passage of electric current. Among other things, the model gives a straightforward representation of the fact that the passage of electric current produces heat: when the idle wheel particles are displaced, they cause some of the rotating vortices to collide against one another, generating friction and thus heat.

The most creative and revolutionary move occurs when, in the third part of 'On Physical Lines', Maxwell sets out to account for electrostatic attractions and induction. For this, he must allow for molecular vortices model to accumulate and release electric charge. Drawing from the treatment of transverse waves in the wave theory of light (1890a:489; cf. Nappo 2021b), and partly from mechanical considerations regarding the motion of his vortex model, Maxwell notes that, whereas he had previously assumed the vortices and electric particles rigid rotating bodies, it was most natural to assume that they had an elastic nature. In this way, the motion could be transferred from their external to their internal parts, and vice versa. The accumulation of electric charge by a given body could then be conceived as a kind of stress or tension in the medium.

To reproduce electrostatic induction by means of his mechanical analogy, Maxwell imagines that a charged body causes an acceleration or deceleration of the idle wheels in the medium and that this change in velocity produces a form of tangential tension on the nearby molecular vortices. Such tension can be perceived by the vortices and can make a difference to their velocity. The result is an accumulation of charge deriving from an initial change in velocity of the idle wheels. This gets translated mathematically into the celebrated term that Maxwell adds to Ampère's law: to the original law Curl B = 4πJ (where B stands for a magnetic field and J for

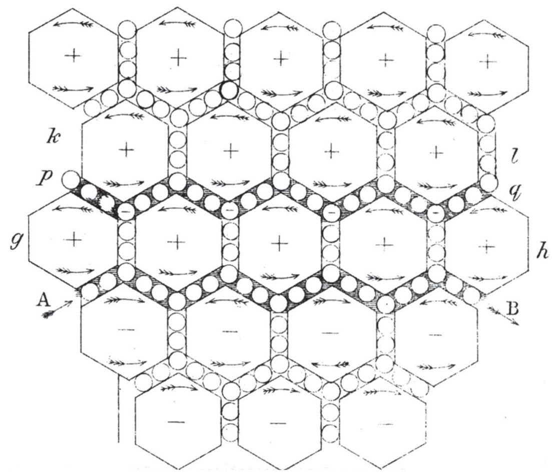

Figure 3: The molecular vortices model of 'On Physical Lines of Force'

a current), Maxwell adds + $\delta E / \delta t$ for the "displacement current" – not so much an actual current (in spite of its name) as much as a measure of the rate of change of an electric field (E) over time (*t*). Most surprisingly, the velocity of the waves through the elastic medium, as calculated from Maxwell's vortex model, turned out to be very close to the measured velocity of light.

In later works, culminating with the *Treatise on Electricity and Magnetism*, Maxwell attempted a reformulation of his electromagnetic theory by a route squarely different from the use of a mechanical analogy, by means of Lagrangian and Hamiltonian formulations of dynamics. Still, his faith in the displacement current and in the subsequent possibility of unifying optical and electromagnetic phenomena was not shaken. While still lacking in the decisive confirmatory evidence and in a precise literal description of the mechanism underlying electromagnetism, with the *Treatise* Maxwell could be sure to have accomplished his principal aim: to provide a physically coherent account of the phenomena of electromagnetism in accordance with Faraday's conception of 'local action' – the notion whereby electromagnetic action unfolds through an intervening field and not (as in many rival theories) at a distance. Here we have a clear illustration of how analogies with familiar domains gave rise to a new exemplar.

4. The Resolution of Maxwell's Revolution – Part I

Although revolutionary, the reception of Maxwell's electromagnetic theory is hard to square with Kuhn's account of scientific change. The aim here is not to recount the full story of this reception (which has been discussed extensively in Buchwald 1984, Hunt 1991, and Darrigol 1993, among others) but to stress, more than has been done before, on the role that analogy has played in making Maxwell's revolution 'commensurable' – and therefore also successful. The arguments below will serve to justify at least a working presumption of commensurability in Maxwell's case, leaving the defense of some historical details to a separate occasion.[6] In particular, this section casts doubt on the idea that Maxwell's revolution was marked by significant incommensurability in standards (or 'methodological') – a form that plays a major role in *SSR*'s early exposition and that has found a recent revival in Bird (2002; 2008). The historical basis for ascribing so-called 'semantic' forms of incommensurability, which occupy a much larger part of Kuhn's concerns in his later works, will be discussed in the next section.

It will be useful to offer a brief reminder of *SSR*'s main claims regarding methodological incommensurability. On *SSR*'s account, scientific revolutions resemble political ones not only in that, when successful, they replace old institutions with new ones. The deeper resemblance concerns the failure of extra-institutional recourse: in evaluating the puzzle-solving capacity of two candidate exemplars, scientists cannot but use the criteria for identifying research problems and for determining the acceptability of proposed solutions that they have been trained to use; and "when paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular" (1970:94). So, while defenders of the revolutionary approach can "provide a

[6] Nersessian (2003) is a notable precedent for the claim that Maxwell's case illustrates the commensurability of consecutive electromagnetic theories. However, Nersessian is concerned with the diachronic relations between Faraday's, Maxwell's and Lorentz's theories; she does not address the question of interest here, viz. whether incommensurability may affect the relation of Maxwell's theory with those of his contemporaries.

clear exhibit of what scientific practice will be like for those who adopt [it]" (94), the individual decision as to the choice of paradigm cannot be based on logic and experience alone. As we read in *SSR*: "like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life" (94).

As it emerges from *SSR*'s discussion, Kuhn sees in the lack of a mechanical rendition of Maxwell's equations the most revolutionary aspect of his electromagnetic theory (see, e.g., 1970:48; 74). This account is in line with a comment that Einstein once made about Maxwell's physics: "Before Maxwell people thought of physical reality — in so far as it represented events in nature — as material points, whose changes consist only in motions which are subject to total differential equations. After Maxwell they thought of physical reality as represented by continuous fields, not mechanically explicable, which are subject to partial differential equations." (1931:69). On a standard account, supported by the sources that Kuhn quotes (see 1970:48), the absence of a mechanical realization is precisely what prevented Thomson (Lord Kelvin) from embracing Maxwell's revolution (cf. Thompson 1910: 1021-7); as Max Planck (1931:58) once added, the same "impossibility of devising a visualizable [mechanical] model" was also (allegedly) the main cause of resistance to Maxwell's new theory in Germany. This would seem to be a perfect illustration of *SSR*'s notion of incommensurability in standards, where conservative and radical scientists diverge on the very nature of physical explanation.

At a closer look, the historical record reveals a different picture. That there was no lack of proposed mechanical realization of Maxwell's theory is shown by the fact that, as Einstein himself noted, "Maxwell did try to find a way of grounding or justifying these equations through mechanical thought-models" (1931:67), such as the mechanical analogy of 'On Physical Lines' discussed above. As it emerges from his later works, such as the 1870 Address to the British

Association, Maxwell never entirely gave up hope of grounding his equations on a molecular vortex theory much like Thomson's: "In the vortex theory we have nothing arbitrary, no central forces or occult properties" (1890b:223). Far from expressing a new attitude toward physical explanation, Maxwell's reluctance to present his theory mechanically after 'On Physical Lines' was likely due to an epistemological concern – one nicely expressed by his friend Monro: "The coincidence between the observed velocity of light and your calculated velocity of a transverse vibration in your medium seems like a brilliant result. But…a few such results are wanted before you can get people to think that, every time an electric current is produced, a little file of particles is squeezed along between rows of wheels" (in Campbell and Garnett 1882:330).

Despite all the warnings that Maxwell attached to them, it was mainly through the analogies with hydrodynamics and the mechanics of wheels that his theory was popularized in British physical circles (cf. Hunt 1991:31). The *Treatise* itself contained multiple references to those analogies, by which the theory's main concepts were illustrated (cf. Hon and Goldstein 2020: ch. 7). In light of this, the idea of an irretrievable difference due to a change in the problems being investigated and in the types of solutions accepted becomes questionable. First, insofar as established methods were not abandoned but repurposed, the alleged logical impossibility of finding neutral ground across the revolutionary divide was sidestepped: arguments for the new theory could be provided by appeal to problem solutions already accepted as exemplary. Moreover, it cannot be stressed enough just how, by drawing connections with exemplary solutions in hydrodynamics and mechanics, the work of entire segments of the physics community suddenly emerged as pertinent to electromagnetism, leading to further work being done at the intersection. The ascension of Maxwell's theory may therefore constitute an important illustration of how an exemplar's acceptance and influence in the scientific community could depend at least in part on its fitting with existing knowledge structures and methods.

Retreating to a looser notion of methodological incommensurability, e.g., in terms of a difference in "values" (Kuhn 1977; cf. Hoyningen-Huene 1993:149), brings little relief.[7] It may seem open to the defender of such a version of Kuhn's thesis to concede that, if a reason for the exception of some British scientists to Maxwell's theory is sought, this is to be located, not so much in incommensurable views concerning the scope and nature of physical explanation, but in different expectations regarding the (then uncertain) connection between light and electromagnetism: for a physicist such as Thomson, for instance, the final theory of physics would contain a demonstration of how light and electromagnetism separately reduce to vibrations of the aether, and not of how optics reduces to electromagnetism (cf. Siegel 1991:160). However, the same the above response leads to another problem, viz. that differences in "values" determining divergent expectations about the results of future experiments are all over normal science (cf. Siedel 2018:S6051). The response that in times of revolution values are used in the "global" evaluation of theories (Hoyningen-Huene 1993:148) works only insofar as one neglects the possibility of arguing from shared exemplars in times of revolution, just as it happens in evaluating a theory's "individual applications" (*ibidem*) during normal science, and so fails to resolve the question of why we should reserve the term 'incommensurability' to the former case.

Of course, the case for the commensurability of Maxwell's electromagnetic revolution becomes more complex when we consider how Maxwell's equations were received among Continental physicists – and especially in Germany. While sharing a Newtonian background, the

[7] Siedel (2018) distinguishes the "norm-circularity" (6028) version of the methodological incommensurability argument (what Shan (2020) calls "narrowly scoped" (387) incommensurability) that was just criticized from the "undetermination" version of the argument, which draws upon differences in "values" (Kuhn 1977).

British and German physical schools had evolved into starkly different research traditions (Buchwald 1994). In spite of this, it is interesting to note that the story of Maxwell's reception in Germany again fails to accord with the picture of scientific revolutions that Kuhn's works portray – and for similar reasons as the ones just discussed in relation to the British context.

A crucial figure is Helmholtz. In 1857, the German physicist completed a seminal work on hydrodynamics, focusing on vortex motion; with its help, in 1861 he proposed a formulation of the laws concerning the distribution of electricity around a circular boundary. Having hit upon roughly the same analogical connections that Maxwell had explored in FLF, in 1870 Helmholtz was able to provide a reformulation of Maxwell's electromagnetic equations as an action-at-a-distance theory, ready to be compared to the theories by, respectively, Weber and Neumann then predominant in Germany. Helmholtz's reformulation expedient was to show that a generalized version of Neumann's theory could be formulated, based on the proprietary notion of potential for electrodynamic forces, in such a way as to be neutral about electromagnetic behavior in the difficult case of open circuits, while being consistent with each of Neumann's, Weber's and Maxwell's theory in closed circuits.[8] In "the struggle between the [three] theories", Helmholtz could therefore hope to "remain as close as possible to the ground of facts, and leave undetermined the parts… which could not yet be decided by experiments" (1870:546).

More precisely, Helmholtz's reformulation effort started from a basic action-at-a-distance picture of electromagnetism, with local arrangements of primitive electric charges in space (their relocation equating to an electric current) and with instantaneous, unmediated interactions between magnetic action and electric currents. Maxwell's theory could then be accommodated by assuming that aether and matter were themselves polarizable; for by assuming the

[8] As Buchwald (1994:9) notes, Helmholtz's partiality for Neumann's theory over Weber's is partly due to its avoiding microphysical hypotheses about charges – a methodological stance that Maxwell shared (1890a:156).

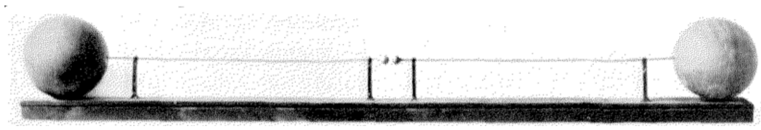

Fig. 4: Hertz's first oscillator, with which he discovered the existence of electromagnetic radiation (as predicted by the Maxwell-Ampère equation when J is set to zero).

polarization to be non-zero, one could derive a time-delay in electromagnetic action (as predicted by Maxwell's original local-action theory). Helmholtz was then able to show that, as this polarization approaches infinity and the value of another free variable *k*, related to the electrodynamic energy of the system, is set to zero, the generalized Neumann's equations imply the Ampère-Maxwell's electromagnetic law; whereas when the polarization does not approach infinity and *k* is set to -1 or 1, the same equations imply, respectively, Weber's and Neumann's alternative generalizations of Ampère's original electromagnetic law to open circuits.

Helmholtz's translation of Maxwell's equations into an analogous theory more accessible to Continental physicists was instrumental in allowing him and his colleagues, in accordance with the experimental tradition common to both Britain and Germany, to devise new tests that could settle the debate over the generalization of the electromagnetic equation. The 1875 experiments on convection currents by Rowland (then a visitor at Helmholtz's laboratory) showed compatibility with Weber's theory and Maxwell's, but not Neumann's. Later, Hertz's oscillators confirmed the existence of electromagnetic radiation, as predicted solely by Maxwell's equations (*Fig. 4*). These facts question Kuhn's idea that "when paradigms enter… into a debate about paradigm choice, their role is necessarily circular" (1970:94). For here we have a case in which a new theory was reformulated in the terms of an old one (i.e., an action-at-a-distance theory) and assessed empirically *by the old standards*. Maxwell's reception in Germany therefore illustrates (once again) how one can rationally come to favor one paradigm over another without shifting the terms of evaluation, simply by appealing to those commonalities in knowledge and standards, as brought out by analogies with familiar scientific domains, that rival schools may share.

Importantly, there is no denying that some differences in scientific “values” (Kuhn 1977) in a broad sense remained between the two schools. As Buchwald (1984, 1994) has noted, laboratory practices were far from uniform even within the Continental tradition. Moreover, as Siegel (1991:123) has argued, diverging attitudes towards physical theorizing and the value of unification may explain why some German physicists, most notably Kirchoff, missed the connection between light and electromagnetism despite hitting upon similar experimental results as Maxwell’s. However, as was already stressed, to speak of incommensurability in this broader sense leads to overshoot, since value divergences determining different expectations in the face of uncertainty are also part of normal scientific activity. Because, as it is illustrated by the reception of Maxwell’s equation, the global evaluation of theories during times of revolution is made against the background of other commonly accepted exemplars, and because (as a result) the appeal to scientific values is not necessary in the global evaluation of theories during such times, all grounds for a distinction seem to fade. At the very least, there is a burden upon defenders of the above response to articulate a plausible criterion that would prevent the notion of incommensurability from extending to disagreements during normal science.[9]

In summary, the reception Maxwell’s revolution (as outlined in this section) does not support the initial diagnosis of incommensurability in standards. In particular, an analysis of the role of his hydrodynamic and mechanical analogies to electromagnetism suggests that the dependence of evaluation standards on the choice of exemplar does not imply the impossibility of a non-

[9] The conclusion that incommensurability in a loose ‘value’ sense is a feature of both normal science and revolutions is a welcome one, but it is hard to square (it is arguably inconsistent) with Kuhn’s position.

circular justification for theory choice.[10] This amounts to denying that the observed trend of practitioners at the turn of the century to embrace Maxwell's theory (and in its purer 'field' formulation, rather than Helmholtz's conservative recasting) must have taken the form of "persuasion" (1970:152).[11] The next section completes the assessment of Kuhn's account.

5. The Resolution of Maxwell's Revolution – Part II

Kuhn's thesis of incommensurability in standards is subject to significant deflection in his thinking after *SSR* (Sankey 1994; Bird 2008).[12] A different and more complex story affects, instead, the other version of the incommensurability thesis that (by the lights of many readers of *SSR*) had seemed to be at odds with the rationality of scientific change: incommensurability in meaning.[13] The case of Maxwell's revolution proves to be of guidance here, too, as we attempt to assess the philosophical merits of Kuhn's claims. Although the discussion below will be partial in its coverage of key changes in electromagnetic notions, it will offer reasons to doubt the implication of faultless disagreement owing to how rival scientists assimilate phenomena.

On *SSR*'s narrative, a group of scientists' turn away from an old exemplar causes a "displacement of the conceptual network through which [they] view the world" (102). They not only believe different things, but perceive objects and facts differently: the same swinging stone is perceived differently by Aristotelians and by Galilei (interpreters sometimes refer to this phenomenon as 'cognitive incommensurability'). Since radical and conservative scientists view

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[10] For an argument for the same conclusion but stemming from entirely different premises, see Lange (2009).
[11] See also Darrigol (1993), who notes that Maxwell's original formulation of his theory was mathematically simpler and more 'natural' than Helmholtz's once its assumptions regarding fields were clearly understood.
[12] See Kuhn (1977) for an elaboration of his post-SSR position. Cf. also Kuhn (2000a): "I would no longer [speak of differences in "methods, problem-field, and standards of solution"] except to the considerable extent that the latter differences are necessary consequences of the language-learning process" (34, fn. 2).
[13] Hoyningen-Huene (1993) has argued that incommensurability is necessary for rational disagreement in science. This paper will not take a stance over the conceptual requirements for rational disagreement.

things differently while using the same words (e.g., ‘pendulum’) to denote them, they must also mean different things when they talk about them. As a result, in articulating their diverging accounts of a subject they must “fail to make complete contact with each other’s viewpoints” (148). Considering space, time, and mass in Einstein’s new physics, for instance, Kuhn writes that “the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts” (102). This semantic segregation is common to all scientific revolutions: “communication across the revolutionary divide is inevitably partial” (149).

Judging *SSR*’s claims with an eye to Maxwell’s revolution makes clear the extent of their hastiness. For while Maxwell’s theory imposed a different structure to electromagnetic phenomena from theories based on action-at-a-distance, there was (as we have seen) *de facto* communication between the two traditions. An important aspect is that the British and German schools were in rough agreement with regards to the description of the main electromagnetic effects (electric currents, induction, etc.) at the operational and phenomenological level. Maxwell himself had assumed, in his early works, that the phenomena that both Faraday and German physicists were treating of could be operationally defined, and that the two approaches could be compared in terms of their fit (1890a:365).[14] By the same assumption, and identifying Maxwell’s theory with a version of Neumann’s that yielded specific predictions (e.g., electromagnetic radiation), Helmholtz was able to provide a working translation of Maxwell’s theory. This shows that differences in meaning do not immediately translate into a form of incommensurability.[15]

[14] Cf. Chang (2012:160) on agreement at the “operational” and “phenomenal” level in the chemical revolution.
[15] Incidentally, Maxwell’s case reveals the flipside of Biagioli’s (1990) “anthropology of incommensurability”. Whereas Biagioli identifies a tendency of competing schools to misunderstand one another to retain their identity, here we have an illustration of the opposite tendency, viz. to find points of contact. While trained in the German school, Helmholtz was, in many ways, an outsider; opposing Weber’s theory, he looked in Maxwell’s work the means to overcome it. What we also need, then, is an anthropology of commensurability.

In effect, already in 1970 Kuhn claimed that he never meant to exclude that working translations from one paradigm's language into another's might be available, nor that defenders of rival paradigms could make use of more familiar language and models in the attempt to establish common ground. In the Postscript (a work that Sankey 1994 classifies as belonging to the 'transition period' in Kuhn's thinking about semantic incommensurability), we read that: "The men who experience such communication breakdowns must… have some recourse […] Briefly put, what [they] can do is recognize each other as members of different language communities and then become translators" (1970:201-2). Still, Kuhn insisted that "neither good reasons nor translation constitute conversion, and it is that process that we must explicate to understand an essential sort of scientific change" (204). We find a similar (albeit more confusing) claim advanced in Kuhn (1976): "translation of one theory into the language of another depends… upon compromises… whence incommensurability […]" (191).

Kuhn's remarks are clarified in his late formulation of the semantic incommensurability thesis, via the theory of the "lexicon" (2000a:52). In the briefest terms, this is the view that shifts in exemplars are accompanied by changes in the "taxonomic categories" (52) by which scientists classify objects and phenomena. It follows that not even a recasting such as Helmholtz's can properly be called a "translation" (53) of Maxwell's theory, since the objects being referred to in the passage to the reformulated theory are subject to re-classification: charges and currents, for instance, cease to be different modification of the same medium, as in Maxwell's original theory, to become separate and irreducible phenomena in Helmholtz's (cf. Darrigol 1993:242).[16] What must have happened in the Continent's transition to a field theory must therefore be assimilated,

[16] In other words, there is a violation of the "no-overlap principle": bridging the gap between the languages "would require adding to one lexicon a kind term that… overlaps with one that is already in place" (2000c:93).

on the lexicon theory, to a form of "language acquisition" (2000a:53).[17] For, allegedly, nothing less than learning to speak and reason by Maxwell's exemplars could have achieved that switch.

In partial defense of Kuhn's points, it must be conceded that 'taxonomic' differences are evident on close historical analysis of the electromagnetic revolution. Some of them were clearly recognized: in exposing their diverging accounts of the subject, British and German physicists were aware that 'electric charge' in Faraday's works meant something different from the same term in Weber. But other differences at the margins (so to speak) of the electro-magnetic subject remained unnoticed. Understanding these additional differences may be important for the purpose of ex post historical explanation. For instance, it has been noted (by Buchwald 1994:80) that divergences in the classification of certain electromagnetic objects, such as that of electric dipole, may offer a partial explanation of the Continental physicists' success, and the British physicists' striking failure, to detect electromagnetic radiation by experimental means.

These concessions notwithstanding, Kuhn's metaphors of "incommensurability" and "language acquisition" (2000a:53) systematically prove too strong for Maxwell's case-study. Analogies play a central role in the explanation.[18] Maxwell's fluid and mechanical models were not only instrumental in grabbing the attention of scientists who, coming from a distinct tradition of physical thinking, could put their work to the service of translation, but also functioned as a third language for facilitating the mutual understanding of competing schools. This is most evident for the fluid analogy of 'On Faraday's Lines'. As we have seen, Helmholtz had studied the same connections between hydrodynamics and electricity that Maxwell had deployed in 1855-6; it was likely this common recognition of the fluid analogy that provided him, in 1870,

[17] Note that Kuhn's lexicon theory is not meant to affect which scientific changes he deems as revolutionary.
[18] Incidentally, this is an aspect that, because of their focus on issues of meaning and reference, fails to figure in standard critiques of the semantic incommensurability thesis, such as Kitcher (1983) and Sankey (1994).

with the means to recast Maxwell's theory into an action-at-a-distance one, via an electric analogue of media density for pressure changes. As a matter of fact, the same analogies kept being invoked throughout the electromagnetic revolution to attain semantic coordination.

To illustrate with a telling example, let's briefly recall the story of one notion in Maxwell's theory that, lacking an immediate operational definition, was bound to generate some misunderstanding across the revolutionary divide: that of *electric displacement*. Insofar as on an action-at-a-distance theory electric charges were conceived as point-like entities immersed in space, Maxwell's use of 'displacement' in his early works and in the *Treatise* immediately suggested the idea of a motion of particles. Inevitably, early Continental readers began complaining that, among other things, Maxwell's notion of polarization was incoherent (cf. Darrigol 1993:202): the required displacement of charges would not amount to a state of polarization, but to something more akin (at least by Continental lights) to an electric current. To add to the confusion, Maxwell frequently recognized the precedent of Mossotti's work on polarization, which was originally presented in terms of primitive electric charges (1890a:491).

As works by his students Lodge (1889) and Fitzgerald (1892) helped clarify, Maxwell's 'electric displacement' should have been read on a more hydrodynamic model: the conveyed meaning is that of a change of state of a field rather than a change of position of electric elements. This leads to a physically coherent account of polarization as a variation of the strain to which a given field is subject. Lodge and Fitzgerald offered additional mechanical models, such as the 'hydraulic Leiden jar' by Lodge and the 'wheel and band' model by Fitzgerald (depicted in Hunt 1991:81), in an attempt to clarify the field conceptions of charge and electric displacement. As discussed in Buchwald (1984:78-9), archival sources indicate that Rowland, among others, found Lodge's clarifications on Maxwell's theory useful. Fitzgerald's expositions of field theory were also important in leading Hertz to the correct interpretation of the oscillator experiment (which he had originally thought of as contradicting Maxwell's equations).

While partial, the above remarks point to a general reason for why, despite their theoretical nature, getting clear on proprietary terms in Maxwell's theory, when the circumstances called for it, did not require learning a new language.[19] Although Continental readers were not trained in British field theory, they did not lack in the idiolects of hydrodynamics and mechanics. By recalling the intermediary analogies, then, scientists such as Weber, Helmholtz, Hertz and others could come to appreciate that Maxwell's notions, while different from theirs, were physically legitimate ones. Kuhn's dichotomy of overlapping versus non-overlapping taxonomic categories accordingly proves far too coarse-grained: while adding (say) Maxwell's term 'electric displacement' into the proprietary language of Continental physicists would have generated inconsistencies with other beliefs of theirs concerning electricity and matter, it was not inconsistent with their beliefs on the type of interactions that may be physically allowed, nor with their beliefs about what physical changes 'displacement' could be used to describe. A proper theory of meaning change ought to track such aspects of proximity in language.

In summary, we have the outline of an argument for why, in the presence of analogies with familiar physical domains, semantic differences 'at the margins' between radical and conservative scientists in Maxwell's revolution could be overcome upon necessity. The prospects for Kuhn's lexicon theory are negative: in attempting to illuminate the changes brought about by Maxwell's new exemplar, the unrelenting description of non-overlapping taxonomies conceals those commonalities in language that are essential for understanding the success of his revolution. Indeed, it is precisely by making salient what was, from Continental physicists'

[19] This is consistent with the idea that, *for the historian*, apprehending early British or early Continental theories of electricity requires learning a new language. See section six on 'time-slice' incommensurability.

viewpoint, an impending but unrealized extension of familiar notions that Maxwell's theory managed to gain their close attention. The metaphors of '*re*-conversion' and 'language *re*-acquisition' would be more appropriate for the conceptual changes that we find instantiated in this historical example, but they imply none of Kuhn's conclusions regarding the alleged failure of "complete contact" (1970:148) to be observed across the revolutionary divide. The next section addresses the debated issue of Kuhn's philosophical legacy in light of the above results.

6. The Nature and Necessity of Scientific Revolutions

By delving into the case-study of Maxwell's theory and its reception, the previous discussion has identified an important tension in Kuhn's account of scientific change. It can be stated as follows. To be successful in achieving a scientific revolution, emerging exemplars must demonstrate their fruitfulness to a wide variety of potential adherents. As section two has discussed, analogies with familiar scientific domains may play an important role in helping display an exemplar's extensibility to new problem situations. But precisely in view of this role, one must also recognize that successful replacement of one exemplar by another is likely to be achieved in ways that are significantly more gradual and less 'gestalt-switch-like' than Kuhn claimed. In other words, the same "acquired similarity relations" (1970:189) whose acquisition and use represent such a crucial part of training and research during normal science also permit the cross-evaluation of competing paradigms during times of crisis. Plausible requirements on successful exemplar replacement are therefore in tension with Kuhn's incommensurability thesis. As the example of Maxwell's electromagnetic revolution plausibly illustrates, the possibility of bridging the gap between distinct research traditions and of reaching out to rival scientists is one of the important factors by which new exemplars sometimes successfully replace old ones.

Because it is concerned with identifying a general tension rather than a specific unfavorable example, easy escape routes for Kuhn's philosophy are to be excluded. To claim, in light of the discussion above, that Maxwell's was not a scientific revolution because incommensurability does not attach to it only saves the letter of Kuhn's position. First, Kuhn's notion of scientific revolution was originally intended to provide insights into the kind of scientific change that Maxwell's electromagnetic theory, among others, brought about (cf. Kuhn 1970:7); to re-classify the episode as non-revolutionary is ad hoc. Secondly, because of the role that analogies with familiar domains have played in other prominent revolutions, such as Newton's and Darwin's, the response may well leave defenders of the Kuhnian approach with a notion of 'scientific revolution' that is, by their own lights, poorly instantiated in the history of science. To defend Kuhn's philosophy, the analysis advanced in this paper must be refuted, not evaded.

Instead of returning to the historical details, let us here try to sketch how the present analysis, if found ultimately correct, would bear on the question of Kuhn's philosophical legacy (cf. Bird 2008; Shan 2020). A useful starting point here is Bird's (2002; 2005; 2008) recent proposal to recuperate *SSR*'s early "naturalistic" (2002:444) themes, at the expense of the aprioristic "wrong turning" (445) that he reads in the late Kuhn. Specifically, Bird praises *SSR*'s pioneering thesis that actual scientific thinking relies on a set of "quasi-intuitive cognitive capacities", such as "mental schemata, analogical thinking, pattern recognition, quasi-intuitive inference" (2008:2). From that basis, he argues that *SSR*'s thesis of incommensurability in standards can be inferred. As Bird (2008) writes: "it is easy to see how incommensurability may arise from the psychological features of scientific cognition outlined above… [For] scientist A and scientist B who have been trained with different set of exemplars… may not be able to come to the same judgment over the correctness of some putative scientific problem-solution" (7).

While agreeing with Bird that the role of quasi-intuitive cognitive capacities should be regarded as a central element of Kuhn's contribution, the above discussion goes in a rather opposite direction to his. To be clear, on both views incommensurability is likely a rarer phenomenon in the history of science than Kuhn makes it to be (cf. Bird 2008:17). The disagreement concerns whether the role of analogical thinking and other quasi-intuitive capacities is evidence for or against incommensurability. According to the analysis developed above, the prevalence of mental models, analogical thinking, and pattern recognition in the history of science indicates that we are *less* likely to identify historical examples of incommensurability in standards (as well as in meaning) – and precisely as the result of the cognitive and sociological aspects characterizing the way scientific activity unfolds.[20]

More specifically, the above discussion motivates drawing a neater distinction between two notions of incommensurability: *time-slice* versus *historical*. The fact that much of what Kuhn regarded as 'normal science' proceeds by means of "mental schemata, analogical thinking, and pattern recognition" (Bird 2008:2) makes it likely that, if we take two arbitrary time-slices of a given scientific field, we discover research traditions that, in many ways, look nothing like each other. Original models and schemata are superimposed by new ones; disciplinary standards are influenced by new discoveries and by the development of cognate disciplines. It is therefore unsurprising that, to use an example from Kuhn (2000b), Volta's conceptualization of the electric battery would differ incommensurably from ours. As Kuhn notes, "the transition from Volta's viewpoint to the modern one reverses the direction of current flow" (2000b:22) and requires a reconceptualization of "the battery and circuit on a more hydrodynamic model" (24). Differences of this sort are bound to emerge from an accurate analysis of past achievements.

[20] A similar conclusion may extend to the domain of pure mathematics, given the prominent role that analogies and mental models have played in shaping its history. See, e.g., Krieger (2005) and Cangiotti & Nappo (2023)

However, incommensurability in a 'time-slice' form does not alter the fact that, historically, the establishment of a given paradigm over another can typically be traced to a more gradual series of commensurable conceptual and methodological changes.[21] As the case of Maxwell's reception illustrates, the transition from one set of models to another is mediated by a series of familiar images and metaphors that aid conceptualization. The form in which a scientific community apprehends a new exemplar need not, therefore, be that in which a speaker enters a different language community; frequently, exemplar replacement is a matter of getting comfortable using some well-known idiolect in a new territory. If this is correct, we should expect the 'historical' sense of incommensurability – that which occurs as history unfolds and that may be experienced by competing scientists in a dispute– to be a much less likely event.[22]

The conclusion just outlined by no means annihilates the extent of Kuhn's legacy. First, it would be wrong to reduce Kuhn's contribution to his claims on historical incommensurability.[23] Secondly, weaker theses in the neighborhood of the incommensurability thesis are untouched by the previous arguments. One of them is that, to re-adapt a phrase by Kuhn himself, sameness of standards and of meaning in science are not to be assumed as the "given" but the conquered "with difficulty" (1970: 126). Although this weaker thesis, viz., that finding common ground is inherently *troublesome* across the revolutionary divide, does not justify the picture of a faultless disagreement between radical and conservative scientists in revolutions, it suffices to dispel many of the myths that Kuhn ascribed to a "philosophical tradition that took science as a static

[21] Kuhn (2000a) hints at something like the distinction between time-slice and historical incommensurability when he writes: "the historian, at least, does experience revolutions. Whether scientists, moving in a direction opposite to the historian's, also experience revolutions is left open by what I have so far said" (57). Kuhn (2000a) goes on to insist that some "holistic [language] changes" (57) do occur in the history of science.
[22] This view does not entirely rule out cases of methodological incommensurability in science; see, e.g., Hoyningen-Huene (2008) and Chang (2012) for a diagnosis of incommensurability in the chemical revolution.
[23] 'Exemplar' as a central unit of historical analysis is not questioned by the arguments above. Cf. Shan 2020.

body of knowledge” (Kuhn 2000d:111), such as that of a universal standpoint and language from which any scientific claim whatsoever could be assessed, independent of time and context.

In conclusion, let us briefly mention a different route for a naturalistic development of *SSR*’s picture of scientific activity. It takes off from what *SSR* identifies as one of the main engines of scientific change: “arbitrariness” (Kuhn 1970:4). This factor comes in at least at two different points in *SSR*’s account of scientific activity. First, there is arbitrariness in the starting points. A given exemplar’s puzzle-solving methods have developed by reference to a particular area of scientific investigation. Meanwhile, the reward system in place in times of normal science leads many scientists trained under that exemplar to get involved in increasingly bolder extensions; not infrequently, the scientific territories uncovered in this process turn out to resist treatment by the standard methods. Secondly, arbitrariness affects how scientific experts responds to emerging empirical anomalies. When an exemplar’s problem-solving capacity is put under significant pressure, the responsibility for saving it falls on the shoulders of those who are regarded as the experts. However, there is always some degree of chanciness in how those experts are selected and what sorts of decisions they eventually take, if any, to preserve the life of the exemplar.

When understood as a part of naturalistic outlook of scientific change, the aspects of arbitrariness just noted may suffice to vindicate one of *SSR*’s central claims: that scientific revolutions are *necessary* (cf. 1970:93). For the ineludible arbitrariness in the starting points and the incentive to investigate yet unexplored territories of scientific interest generate the conditions phenomena that cannot be explained by a mere extension of some standard problem-solving patterns. As Kuhn writes, “sometimes a normal problem, one that ought to be solvable by known rules and procedures, resists the reiterated onslaught of the ablest members of the group […] On other occasions a piece of equipment designed and constructed for the purpose of normal science fails to perform in the anticipated matter" (1970:6). These reoccurring episodes ensure the conditions for a scientific consensus to be threatened and, in some cases, overthrown. The result is a picture of the history of science that is necessarily marked by time-slice incommensurability.

7. Progress through Revolutions

To sum up, this paper has outlined a challenge to Kuhn's account of scientific change. If the analysis proposed above is correct, any prospects for a future Kuhnian philosophy of science would have to take into account the fact that Maxwell placed at the foundation of the utility of analogical thinking in science: that "the great majority of mankind are utterly unable, without long training, to retain in their minds the unembodied symbols of the pure mathematician…To such men momentum, energy, mass are… words of power, which stir their souls like memories of childhood" (1890b:220). As argued in the preceding sections, this fact places constraints on what candidate exemplars should be like to gain the favor of new adherents, leading to the conclusion that revolutions in science are unlikely to display incommensurability in its *historical* form. A more probable result to expect from "naturalizing Kuhn" (Bird 2005) would be an argument for another distinctively Kuhnian thesis: the *necessity* of scientific revolutions.

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