The Kuhnian mode of HPS

Abstract

In this article I argue that a methodological challenge to an integrated history and philosophy of science approach put forth by Ron Giere almost forty years ago can be met by what I call the *Kuhnian mode of History and Philosophy of Science (HPS)*. Although in the *Kuhnian mode of HPS* norms about science are motivated by historical facts about scientific practice, the justifiers of the constructed norms are not historical facts. The *Kuhnian mode of HPS* therefore evades the naturalistic fallacy which Giere's challenge is a version of. Against the backdrop of a discussion of Laudan's normative naturalism I argue that the *Kuhnian mode of HPS* is a superior form of naturalism which establishes contact to the practice of science without making itself dependent on its contingencies.

Keywords: history and philosophy of science, naturalistic fallacy, normativity, normative naturalism, rationality, counterfactuals.

1 Introduction

History of science without philosophy of science is blind, philosophy of science without history of science is empty. This paraphrase of Kant's famous dictum by N. R. Hanson (1962)—sometimes wrongly attributed to Lakatos (1970) or Feigl (1970)—is often cited when motivating the combination of philosophical with historical methods in the attempt to better understand science. Roughly, this seems to be right. A meaningful philosophy of science should be informed by scientific practices. One way of doing that is by studying the history of science. Yet there are several methodological concerns that have been raised about the integrated history and philosophy of science (HPS) approach. The concern I want to focus on in this paper was raised about forty years ago by Ron Giere (1973) in a widely-received review article:

> If one grants that epistemology is normative, it follows that one cannot get an epistemology out of the history of science-unless one provides a philosophical account which explains how norms are based on facts. [….] The general problem [in HPS therefore] is to show that philosophical conclusions [about what is rational, i.e. norms] may be supported by historical facts and just how this comes about. Until this is done, the historical approach to philosophy of science is without a conceptually coherent programme (Giere 1973, 290-2).

Effectively, Giere is worried about HPS practitioners committing what (in the realm of ethics) is also known as the *naturalistic fallacy*. That is, Giere is skeptical that historical *facts* about science can support philosophical *norms* about science. In other words, how can 'what is' have any bearing on 'what ought to be'?[1] Yet, Giere's challenge presupposes an idea that I shall deny in this paper, namely that historical facts

[1] Giere, in his review, stated that to "raise this issue is not necessarily to hold dogmatically to a distinction between the descriptive and the normative". And yet his challenge has been customarily interpreted along these lines. In fact, Giere himself gave this interpretation recently when revisiting his review (Giere 2011). Giere later in his work took (Giere 1989) a naturalistic turn similar to Laudan's normative naturalism, the latter of which will be discussed in detail in Section 4. Giere raised a further issue back in 1973 that has subsequently been addressed. Giere argued that history of science is not necessary for studying science philosophically. Contemporary science might do just as well. As several

indeed *are* the justifiers of all historically motivated norms. On the contrary, I shall argue that there is a class of historically motivated methodological norms about science whose justifiers are not historical facts. I shall refer to the mode of history and philosophy of science that results in this class of norms as the *Kuhnian mode of history and philosophy of science (HPS)* not because I believe that it really captures Kuhn's actual thoughts about HPS methodology, but rather because it makes best methodological sense of some of Kuhn's central ideas about science. Although I give plenty of argumentation for why historical facts are not the justifiers of historically motivated norms, I will have not so much to say about what the justifiers of such norms are. Although my project, in this regard, is therefore largely negative, I hope that it will clear the ground for a future positive account of the justifiers of norms constructed in the *Kuhnian mode of HPS*, which I believe, will be an *a priori* account.

The plan for the paper is as follows. In Section 2 I review some prominent reflections on the philosophical norm—historical fact relationship. I conclude that none of these reflections helps us to meet Giere's challenge. In Section 3 I introduce the *Kuhnian mode of HPS*, which, I argue, successfully evades the naturalistic fallacy. In Section 4 I argue that the *Kuhnian mode of HPS*, in many respects, is superior to Laudan's much discussed *normative naturalism,* i.e., its most natural competitor. Section 5 concludes this paper.

2 Reflections on norms and facts in HPS

Hitherto methodological reflections on the relationship of Philosophy of science (PoS) and History of science (HoS) that engage with the issue of normativity can be grouped into two broad approaches, with normativity running from HoS to PoS or from PoS to HoS, respectively:

- The *factive* view: HoS ought to inform philosophical theorizing about science;
- The *normative* view: PoS ought to inform HoS.

Before I shall proceed to give examples for these two approaches a couple of clarifications are in order. First, the factive and the normative view are not mutually exclusive; they may be held at the same time. It just so happens that the workable accounts that I will consider here fall in either camp. Second, as Laudan (1977) pointed out, the term History of Science is ambiguous. In the English language it may relate to either to "the actual past" or to the writings of historians about the past. I will be concerned only with the latter sense.

It is widely acknowledged that historians' writings are inextricably informed by philosophical presuppositions which historians all too often make only implicitly. For instance, a historian may believe that experiments are the only decisive grounds for rejecting a theory. Her research may then tend to focus on crucial experiments rather than on other factors (Laudan 1977, 156-8).[2] Furthermore, the very categories used by historians, such as "confirmation", "observation", "measurement", "explanation", "simplicity", etc. are philosophically charged (ibid. Hanson 1962; Chang 2011). Philosophical rigor in defining those categories may therefore be indicated in order to avoid pitfalls. Some philosophers,

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authors have pointed out, however, many questions about the nature of science (such as theory appraisal) do require the study of the diachronic dimension of science (McMullin 1974; Burian 1977).

[2] The biasedness of historical case studies has become a common theme in recent debates about HPS methodology (Burian 2001; Pitt 2001; Chang 2011). See BLINDED for the outline of a defense of the case-study approach.

however, envisage an even stronger role for philosophy: they claim that philosophical analysis may challenge scientists' judgments in the past as recorded by HoS. An early proponent of such an approach is Norwood Russell Hanson. Assessing the 'formalistic' philosophy by his contemporary Rudolf Carnap and others, Hanson (1962) wrote in accord with his abovementioned rendering of the Kantian idiom:

> To the historian such philosophy is often unilluminating because it does not enlighten one about any thing: nothing in the scientific record book is treated in such symbolic studies. (Hanson 1962, 582)

But Hanson was not happy with the work of historians either:

> To the philosopher, histories of science are often unilluminating because, as a result of their chaotic diffuseness, they never reflect monochromatically: only spectra of concepts and arguments result. (ibid.)

As alternative to these approaches, Hanson advocated a combined HPS method. As he saw it, philosophers of science should be concerned with the assessment of the "logical cogency" and the justifiedness of scientific claims. Hanson for instance suggested that the philosopher should determine, by their critical methods, whether the positron was indeed supported by the evidence in 1931 and whether it was steady state or rather big bang cosmologists who had better arguments for their respective theories. As inspiration for his approach Hanson explicitly cites Pierre Duhem's showing that Galileo's argument for the acceleration of a body being proportional to the duration of its fall being unsound. As later descendants of this approach to HPS one can cite Earman and Glymour (1980) on the confirmation of general relativity in 1919 by Eddington's star light bending expedition. Earman and Glymour concluded that Eddington and his collaborators threw out one data set for no good reason.[3] This and other examples were later picked up by the proponents of the so-called Strong Programme of the Sociology of Scientific Knowledge, which sought to explain also true justified beliefs in terms of sociological causes (Collins and Pinch 1998). Many works in the SSK tradition, although portraying themselves as following a purely descriptive approach (e.g. Bloor 1999), indeed make *normative* judgements about certain theories not being as well supported by the evidence at the time as scientists had it. Collins and Pinch (1998), for instance, build on Earman and Glymour's abovementioned work and follow their normative judgment that the theory of relativity was not well supported by the British eclipse data in 1919. They then go on to argue that scientists' reasons for acceptance were sociological rather than epistemic. Pickering (1984), likewise, judges normatively that the epistemic reasons for accepting the weak neutral current as being real were not strong and argues that the reasons for acceptance were rather of a sociological nature. It goes without saying, however, that this argumentative strategy originally formulated by Hanson, although clearly central to the work of the SSKers, does not exhaust their argumentative arsenal.[4]

The perhaps most prominent and probably most controversial reflection on the philosophical norm—historical fact relationship comes from Imre Lakatos. Lakatos (1970) viewed the relationship between philosophy and history of science thus:

> (a) philosophy of science provides normative methodologies in terms of which the historian reconstructs 'internal history' and thereby provides a rational explanation of the growth of objective knowledge; (b) two competing methodologies can be evaluated with the help of (normatively interpreted) history; (c) any

[3] This has been contested recently (Kennefick 2009). See BLINDED for a reply.

[4] Other strategies are the exploitation of the thesis of the underdetermination of theories by evidence and the fact that the evidence does not *determine* the choice of a certain theory (Burian 1990).

> rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) 'external history'. (91)

For Lakatos “history without some theoretical bias is impossible” (107). Any history of *science*, Lakatos argued, would, in the selection of facts, presuppose some definition of science (if even only implicit) and therefore normative judgments. For the sake of transparency such definitions and normative principles should be made explicit. But for Lakatos the writing of internal history was not just the selection of historical facts on the basis of norms. Rather, internal history “may, on occasions, be their radically improved version [of the facts]” (106). Famously, Lakatos proposed to ban all historical facts that threatened to undermine the rationally reconstructed internal histories (i.e., the ‘external’ history) into the footnotes of the texts containing the internal histories (107). Worse, Lakatos thought that the normative methodologies developed by philosophers ought to be evaluated on the basis of *internal,* i.e. rationally reconstructed, histories. But, as many writers pointed out immediately (Kuhn 1970b; McMullin 1970), this is either circular (when the evaluated norm is the same that was used to reconstruct the internal history) or question-begging (when the evaluated norm is different from the one used to reconstruct the internal history). Apparently, Lakatos wanted to have his cake and eat it too. He wanted an account that would combine the factive and the normative approach. That miserably failed.

A genuine example for the factive approach is the programme set up by Donovan, Laudan, and Laudan (1992) in their book *Scrutinizing Science*. Donovan et al. “take the analogy between science and science studies seriously and see no reason why science itself should not be studied scientifically” (8). By that they essentially mean that scientific methodologies, or, in their words, “theories” about science need to be subjected to an empirical test. The test that Donovan find most appropriate is a test on the basis of historical case studies. Effectively, their approach is a naïve falsificationist approach: although they realize that their approach is subject might be undermined by the Duhem-Quine thesis, they opt to simply ignore this threat (13). What is more, Donovan et al.’s project leaves one flabbergasted as to how one is supposed to test a norm on the basis of facts. Among the norms they seek to so test is the norm that new theories should conserve their predecessor’s empirical success (31). But if one were to find that a successor theory does not conserve the empirical success of their predecessor, should one not conclude that the proposed theory is flawed rather than concluding that the norm is? Again, Donovan et al. mention this problem, but choose to ignore it (xv). Donavan et al. do refer the reader to Laudan’s much more sophisticated normative naturalism (in a footnote). Later in this essay I will discuss Laudan’s view at some depth, but suffice it to say that at the pinnacle of Laudan’s campaign, he held that norms were to be tested “against the historical record in the same way that any other hypothesis about the past can be tested against the record” (Laudan 1987, 27).

The most recent view on HPS that I want to mention is Hasok Chang’s idea of HPS as ‘complementary science’ (Chang 2004). Chang wishes to treat HPS as an integral discipline in which “it becomes difficult to see where philosophy ends and history begins or vice versa” (240). In complementary science “[p]hilosophy and history work together in identifying and answering questions about the world that are excluded from current specialist science” (ibid.). In this endeavor philosophy endows us with the “criticism” and “skepticism” needed for realizing that science might leave a number of interesting (and scientifically researchable) questions unanswered. History supplies us with the tools needed to unearth the relevant episodes in which these questions were raised before they were later forgotten (for one reason or another). As examples Chang mentions the (apparent) radiation of cold (analogous to the radiation of heat), and the superheating of water (i.e. boiling of water slightly below the boiling point). As Chang notes himself, HPS as complementary science is a (weakly) normative approach. That is, complementary science deems certain research questions worthy of pursuit and thereby makes a value judgment. Nevertheless, Chang is keen to stress that complementary science is not prescriptive: it does not tell scientists that they *ought to* have researched those questions, but rather recognizes the legitimacy of "specialist science" focusing on certain topics at the disadvantage of others. Like in Hanson's approach we are being invited to study historical facts about science (i.e. forgotten research questions and problems) with the help of philosophical tools. In contrast, just as in Hanson's approach, Chang does not identify history as constraining philosophy in any way. Chang's approach is thus clearly an instance of the *normative view*.

In taking stock we realize that none of the above reflections on the HPS relationship helps us answer Giere's challenge. To Hanson's strong normative approach and to Chang's weakly normative approach, Giere's challenge is simply irrelevant. Whereas Hanson and Chang advocate that PoS ought to inform HoS, Giere's challenge concerns those approaches that believe that HoS should inform PoS. On the other hand, although Lakatos's and Laudan's approach have the relevant directionality of normativity (HoS ought to inform PoS), they are severely flawed. So how can Giere's challenge then be answered? Or can it be answered at all? After all, the norm and fact divide is not only a problem for philosophy of science but a general problem for any naturalistic approach towards norms. Perhaps, this challenge, just as another famous problem highlighted by Hume, may simply not have a solution. Regardless of the prospects on other fronts of the norm-fact divide, however, I want to argue in the next section that at least in the context of HPS, there is an approach that does successfully address Giere's challenge. The approach in question I will refer to as the *Kuhnian mode of HPS.*

3 The *Kuhnian mode of HPS*

Kuhn himself never extensively commented on his own methodology. However one does find relevant remarks interspersed throughout his work. To my knowledge the most informative methodological statement to be found by Kuhn is the following, which is worth quoting in full:

> *I am no less concerned with rational reconstruction, with the discovery of essentials, than are philosophers of science. My objective, too, is an understanding of science, of the reasons for its special efficacy, of the cognitive status of its theories.* But unlike most philosophers of science, I began as an historian of science, examining closely the facts of scientific life. Having discovered in the process that much scientific behavior, including that of the very greatest scientists, persistently violated accepted methodological canons, I had to ask why those failures to conform did not seem at all to inhibit the success of the enterprise. *When I later discovered that an altered view of the nature of science transformed what had previously seemed aberrant behaviour into an essential part of an explanation for science's success, the discovery was a source of confidence in that new explanation.* My criterion for emphasizing any particular aspect of scientific behaviour is therefore not simply that it occurs, nor merely that it occurs frequently, but rather that it fits a theory of scientific knowledge. *Conversely, my confidence in that theory derives from its ability to make coherent sense of many facts which, on an older view, had been either aberrant or irrelevant.* (Kuhn 1970c, 236-7 added emphasis; see also Kuhn 1970b)

Clearly, Kuhn has a factive HPS approach in mind: HoS ought to inform PoS. Further, Kuhn is not willing to give up on the idea that science is in fact a rational enterprise. On the contrary, he thinks that important historical facts ought to inform what we take to be rational, scientific behavior. But then Kuhn of course needs to address Giere's challenge. Although Kuhn never attempted to do this I think Kuhn's philosophy of science does implicitly contain an answer. In the following I want to erect a sound methodological footing that makes good sense not only of Kuhn's but also of other, more recent, philosophical concepts about science, and that evades the naturalistic fallacy. That methodological footing I want to refer to as the *Kuhnian mode of HPS* which is characterized thusly:

> *Discovery of norms:* Historical facts about scientists' actions motivate the construction of scientific norms under which those actions come out as rational.
>
> *Justification of norms*: Historically motivated scientific norms provide constraints for rational behavior even in worlds in which the facts motivating those norms are different. Historical facts are therefore not the justifiers of historically motivated norms.

The *Kuhnian mode of HPS* clearly evades the naturalistic fallacy, since '*what is*' is justificatorily irrelevant to '*what should be'*. To further elucidate the discovery and justification of norms in the *Kuhnian mode of HPS* one can formulate the following two counterfactual conditionals.

> *D-counterfactual:* Had there not been historical fact *h*, there would have been little reason to propose norm *n* under which *h* comes out as consistent with rational behavior.
>
> *J-counterfactual*: Had there not been historical fact *h*, norm *n* would still provide constraints for rational behavior.

The following examples will illustrate the *Kuhnian mode of HPS*.

Normal Science. Kuhn can be interpreted as proposing as the main goal of normal science the *efficient* solution of puzzles of different kinds (experimental, conceptual, instrumental, theory-evidence fit, determination of constants, etc.) within a certain paradigm. It is interesting to note that Kuhn actually quite explicitly uses axiological terminology when characterizing normal science (Kuhn 1996, 24). But again, my purpose here is not exegetical. Even if Kuhn hadn't formulated his concept of normal science in axiological terms, it makes good sense to do so.

On the basis of historical work, Kuhn pointed out that puzzles that a certain paradigm struggles to solve are either ignored or left to one side for them hopefully to be solved at a later time. But are those "anomalies" not to be viewed as falsifiers of the relevant paradigm? Would it not be *irrational* (and dogmatic) for the scientist to hold onto her paradigm in the face of those (apparent) falsifiers? This is a view that Popper took (Popper 1970). But Kuhn did not simply report that in the historical development of science there were those apparent falsifiers that did *not* lead to an overthrow of the relevant paradigm (e.g. the recalcitrant orbit of the moon and the advance of Mercury's perihelion in the Newtonian paradigm). Rather, Kuhn sought to provide a *rationale* for apparent falsifiers that are being left aside by the scientific community. On the Kuhnian idea of normal science it is rational not to have just *any* recalcitrant puzzle overthrow a paradigm. Why is it not? Because it is the goal of normal science to solve puzzles *efficiently*. If science were to stop or even give up on the relevant paradigm any time it were to encounter puzzles that resist resolution, science, so Kuhn had it, would be terribly inefficient. Clearly then, the concept of normal science is *motivated* by Kuhn's historical findings about apparent falsifiers not falsifying paradigms. However, the norm implied by normal science is not justified by the historical facts. This can be seen by posing the appropriate J-counterfactual. Imagine a world in which science were not to encounter any puzzles that were to resist resolution (within a paradigm). Even in such a world the norm of leaving aside puzzles that resist solution in order to increase the efficiency of science, even though, as a matter of fact, there are no such puzzles in that world, would be a perfectly rational norm. Kuhnian anomalies, although motivating the concept of normal science, do not justify it. The concept of normal science is not the only example there is for the *Kuhnian mode of HPS*. Let us consider two other examples.

Novelty. Novel empirical success plays an important role in the realism debate (see above). Historically, it was highlighted probably first by Imre Lakatos, who had it that only those research programmes are progressive that produce successful *novel* predictions. Soon, however, it was realized that scientists often give no more credit to theories than they do to the accommodation of already known facts. Worrall (1989)'s study of Fresnel's successful prediction of the bright spot is a case in point. Although Fresnel's successful prediction is as impressive a prediction as one can get, it was not given more attention than Fresnel's successful explanation of the straight edge diffraction patterns. Rather than rejecting the relevant practices as being irrational (which they are on the view that novelty must be *temporal* novelty), Worrall redefined the concept of novelty in accordance with those practices. In Worrall's account, a theory need not produce temporally novel evidence but merely use-novel evidence, where use-novel evidence is defined as evidence that was not used in the construction of the theory entailing it (for various complications see BLINDED). The idea of use-novelty, just as the concept of normal science, is perfectly *rational* concept: it (reasonably) deems any *ad hoc* modifications of a theory illegitimate. Because Fresnel, for instance, did not use the straight edge diffraction phenomenon in the construction of his theory, or so Worrall argues, his explanation of that phenomenon must be counted in that theory's favor. Not so, if Fresnel had indeed used that phenomenon in his theory-construction. As a rational concept, use-novelty (just like normal science) is inspired by the historical facts, but the rationality of action implied by that concept does not depend on the historical facts. On the contrary, use-novelty would provide constraints for rational action even in a world in which theories only made temporally novel predictions, i.e. a world in which the historical facts motivating the introduction of the use-novelty concept were different. Even in such worlds, the norm not to use the evidence that one seeks to accommodate in the construction of one's theory would be a perfectly rational norm. In other words, the absence of the historical facts motivating the introduction of the use-novelty concept take nothing away from the rationality of the norm implied by the use-novelty concept.

Realism in the face of the Pessmistic Meta Induction. The Pessmistic Meta Induction (PMI) has been proposed as an argument against scientific realism. From the fact that past empirically successful scientific theories turned out to be false (in particular: the caloric theory of heat and aether theories), it is argued that the empirical success of our *currently* best theories is no good grounds for inferring that these theories are approximately true. Regardless of what one may make of the force of this argument (see e.g. Magnus and Callender 2004), it has prompted the realist to respond to this challenge not only by refining the meaning of empirical success (an empirically successful theory must have *novel* empirical success), but also by developing some sophistication about what parts of a theory one should take a realist stance. One of the most popular positions employing a "divide et impera" move (though certainly not the only one; see e.g. Psillos (1999)), is *structural realism* (Worrall 1989). The structural realist is committed only to the structure of theories (such as Fresnel's equations) latching onto the world, not their 'content' (such as the ether), for it is the former, not the latter the structural realist singles out as being responsible for empirical success. Structural realism thus allows for a revised picture of *rational* scientific progress whilst accommodating the historical facts highlighted by the PMI. Although it is normally not put that way, that view implies that scientists act rationally when they, slightly simplistically speaking, discard the 'content' of theories and retain only the 'structure' when adopting a new theory. In contrast, scientists would act irrationally, according to this picture, if they were to discard a theory's structure that was responsible for the success of their past theories. Without taking any side in the realism debate (our focus in this paper is on methodology after all), one may recognize structural realism as a result of the application of the *Kuhnian mode of HPS*. Just like normal science and use-novelty, structural realism is clearly motivated by the historical facts that the PMI rests on.[5] But the rationality of the norms implied by structural realism does not depend on any historical facts. That is, if structural realism were the right view to hold about scientific progress, it would be rational for scientists to seek to retain only the structural elements of theories (responsible for empirical success) even in a world in which there would in fact be no radical theory change.

4 The *Kuhnian mode of HPS* vs. Laudan's *normative naturalism*

In order to work out the *Kuhnian mode of HPS* more clearly, it is useful to contrast it to its perhaps most natural competitor, namely Laudan's *normative naturalism*. Central to Laudan's naturalism is an *instrumentalist* conception of methodological norms, which have conditional form: *if* one's goal is X, one ought to do Y, where Y is the best known *means* for achieving *ends* X. Further Laudan, like other instrumental rationalists (e.g. Leite 2007; Brössel et al. forthcoming), is an outright empiricist about instrumental norms:

> the soundness of such prudential imperatives [i.e. methodological norms] depends on certain empirical claims about the connections between means and ends; accordingly, empirical information about the relative frequencies with which various epistemic means are likely to promote sundry [methodological] ends is a crucial desideratum for deciding on the correctness of [methodological] rules. (Laudan 1990b)[6]

Prima facie, the *Kuhnian mode of HPS* seems well compatible with this central component of Laudan's normative naturalism. After all, one of my main examples for the *Kuhnian mode of HPS*, namely normal science, seems to be an example for instrumental rationality: there is a certain goal (efficiency of research) and all actions furthering that goal are deemed rational (focus on solvable puzzles, neglect of recalcitrant ones). Despite the appearances, however, there are important differences between the *Kuhnian mode of HPS* and Laudan's normative naturalism which I want to outline in the following. These differences concern (i) the status of so-called *implicit* methodology, (ii), the question of whether the methodology of science must stay fixed throughout the ages or change, and most importantly (iii) whether methodological norms are to be assessed empirically. Let us consider these components in turn.

First, there is an important distinction to be drawn between scientists' methodological pronouncements and their *actual* methodology, which may very well diverge. In the literature, the former have been summarized under the label *explicit* methodology and the latter has been denoted *implicit*

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[5] For a most recent (non-structuralist) attempt to reconcile realism with the history of science see Harker (forthcoming).

[6] In actual text Laudan speaks of 'epistemic' ends and 'epistemic' rules. However for Laudan the difference between methodological norms, which he construes as having conditional form, and epistemic norms is not substantial for he defends a reductionism of epistemic norms to conditional norms (Laudan 1990a; cf. Kelly 2003; Leite 2007; Brössel et al. forthcoming). See also below for more textual evidence for Laudan's empiricist interpretation of instrumental norms.

methodology (e.g. Worrall 1988). Although I have misgivings about the terminology I shall follow it here for the sake of continuity.[7] In Laudan's early work this distinction and the relationship between scientists' explicit and implicit methodology plays an important role in his 'reticulated' model of methodological change (cf. Laudan 1986, 53ff.). Ideally, Laudan holds, a scientist's explicit and implicit methodology match. Whenever they diverge, this is grounds for questioning either, although normally explicit methodology follows implicit methodology and not vice versa.[8] Since methodological norms are instrumental norms in Laudan's account, a change in the methodological norm implies a change in the goal of the agent. Curiously, however, in his latest work in which he defends normative naturalism Laudan either does not even mention the implicit/explicit methodology distinction anymore (Laudan 1990b), or he clearly commits to the import of explicit methodology by making it a *necessary* condition for an action to be rational that the agent carrying out that action *believes* it to further her goals (1987a).[9] The reason Laudan gives for this commitment of his is that, on the opposite view, agents may promote their ends effectively but be deemed irrational, or conversely, fail to effectively promote their ends and be deemed rational (ibid., 23). However I don't think there is anything wrong with this. A scientific agent might have aims which are very detrimental to the goal of science. The agent's goal might be to increase his scientific standing at any cost so as to fake her experimental results. If she were to manage to publish such results in a highly-ranked journal without anybody realizing their being flawed, she would reach her goal to receive fame but act contrary to the scientific goal of gaining genuine knowledge about the world. With regard to the goal of science—and that's what really matters for any account of scientific rationality—she would act irrationally. Furthermore, making it a necessary condition of an action being rational that the agent believes her actions to further her goal entails that an agent's implicit methodology that the agent is not aware of can never be rational, which is not only absurd but also undermines part of the driving force for methodological change in Laudan's reticulated model.

On the *Kuhnian mode of HPS* scientists' explicit and implicit methodologies can motivate the discovery of norms. Contrary to the later Laudan, it is no requirement of rational action that the agent in question be aware of the goal her actions promote. And contrary to the early Laudan explicit and implicit need not be brought into agreement. In cases of mismatch, however, the HPS practitioner is well-advised to follow Einstein's dictum: "don't listen to their words, fix your attention on their deeds" (Einstein 1982, 270). The priority of implicit methodology can be illustrated with an analogy invoked by van Fraassen (1980). The aim of the game of chess is it to check-mate your opponent. Any chess move that will further the goal of check-mating your opponent in accordance with the rules of chess will be a rational action (conversely, it will be irrational to make moves that threaten your own check-mate). The explicit goals of any individual chess player, however might be quite different and diverse (fame, money, having a good time, socialize, etc.). When we want to understand the game of chess, of course, we better focus on a chess-player's action rather than on their explicit goal pronouncements. Likewise in science. The analogy is of course an imperfect one. Whereas any half-decent chess-player will be aware of the aim of chess, this is not necessarily the case for scientists: scientists may not have any inkling of the goal they are in fact pursuing. Consider the concept of normal science. Very few practicing scientists would recognize efficient puzzle

[7] I think this distinction is slightly misleading since it suggests that the methodology as revealed by a scientist's practices is *never* explicit, which is of course implausible.
[8] Doppelt (1986) has argued that the choice between what needs changing is indeterminate.
[9] Laudan also commits to this idea in his replies (1989, 1990a) to Siegel (1990) and Worrall (1988).

solving as an essential goal of their activity. Nevertheless there may be good grounds for claiming that this is indeed an essential goal of theirs.

Second, Laudan is adamant that the aims and therefore the norms of science have changed throughout the ages. In contrast, the norms considered above as examples of the *Kuhnian mode of HPS*, do not exhibit such context- and time-dependence. They are notions intended as characterizations of science *throughout time* and *regardless of historical context*. This is obvious for structural realism and the methodological requirement of novel evidence, but it is also true of normal science: *any* mature science, according to Kuhn, will exhibit the practices characteristic of normal science. Indeed, as is well known, Kuhn once explicitly defended normal science as a demarcation criterion (Kuhn 1970a). With regard to ancient astrologers, he writes, “they had no puzzles to solve and therefore no science to practice” (9).

The concept of normal science is not the only example in Kuhn’s philosophy for fixed methodology. Other examples are the fixedness of the normal science—crisis—revolution—normal science *sequence* and Kuhn’s five (neither exclusive nor exhaustive) criteria or ‘values’ for theory-choice (Kuhn 1977). At least the former also comes equipped with a plausible rationale, which can be generated entirely from the concept of normal science: (i) the goal of normal science is efficient puzzle solving within a particular paradigm, (ii) as a matter of fact, no particular paradigm solves all puzzles it poses itself, (iii) in order to be efficient, normal science leaves aside puzzles that resist resolution, (iv) a paradigm cannot be said to be efficient if there are too many puzzles that cannot be solved within the paradigm (corollary: the latter will affect scientists psychologically), (v) science can re-gain its efficiency through a paradigm-change, whereby the degree of efficiency of the superseding paradigm (consisting of the right balance between puzzle solving capacity and tolerance of recalcitrant puzzles) will be significantly *different* from the superseded paradigm only if the two are logically incompatible (hence, a ‘revolution’ is required). Kuhn says:

> [I]f new theories are called forth to resolve anomalies in the relation of an existing theory to nature, then the successful new theory must somewhere permit predictions that are different from those derived from its predecessor. That difference could not occur if the two were logically compatible. (Kuhn 1996, 97f.)

If the predictions of the old and the new paradigm *were* logically compatible, the anomalies of the old paradigm would be the anomalies of the new paradigm. The efficiency of normal science would not be significantly altered. Since, as we just saw, the normal science—efficiency crisis—paradigm change—normal science *sequence* can be generated from the concept of normal science, which, as we saw above might be construed as an example of instrumental rationality, it might be said that Kuhn provides an instrumental-rationalist (though no epistemic-rationalist) account of the development of science. Of course not all of Kuhn’s idea fit the bill. Although Kuhn held that superseding paradigms should *generally* increase science’s puzzle solving capacity, not always all puzzle solving capacity of the previous paradigm carries over to the superseding paradigm (1996, 169). The failure to retain puzzle solving capacity across paradigm change is also known as “Kuhn loss”. Regardless, that the normal science—efficiency crisis—paradigm change—normal science *sequence*, too, comes with a rationale compatible with the *Kuhnian mode of HPS* can be seen by the following argument: even if there had been no recalcitrant puzzles within a certain paradigm, it is still true that, had the efficiency of normal science within that paradigm ceded significantly as a result of recalcitrant puzzles, it would have been rational to change paradigms in order to re-establish efficient puzzle-solving activity. Further, it would have been rational to effect a *radical* paradigm change so as to ensure that a high number of recalcitrant puzzles of the old paradigm be turned into solvable puzzles in the new paradigm.

What is one to make of the fixedness of methodology? Laudan takes it to be a key feature of entirely wrong-headed *a priori* accounts of rationality that pay heed to the practice of science. And for Laudan, it is blatantly obvious that scientific methodology has changed throughout the ages. But has it? It all of course depends on what we regard as legitimate candidates for scientific methodology. If any explicit methodology is to be treated as such a legitimate candidate, then it is quite obvious that scientific methodology has changed throughout the ages. Worrall (1988) convincingly denies the antecedent. In the theory-change that took place in the early 19th century from the emission theory of light (worked out by no other than Newton) to the wave theory of light (propelled by the work of Fresnel in particular), Laudan claims, also a methodological change took place from inductivist method that banned all theoretical entities (cf. Newton's 'hypotheses non fingo') to a hypothetico-deductive method with a premium on predictive success. But Worrall doubts that the methodological change was real. Rather, scientists in the 18th and early 19th century were under the *illusion* that the success Newtonian mechanics achieved was based on a positivistic method a la Newton when as a matter of fact that method only insufficiently described what they were actually doing (e.g. postulate the theoretical concept of force!). Again, the explicit espousal of methodological imperatives by scientific actors can be very misleading.

Worrall makes another observation with regard to this important example. If the change from Newton's emission theory of light to Fresnel's wave theory of light was accompanied by a temporally deferred change of methodology (as Laudan claims), and if methodological norms constrain rational behavior (as Laudan is of course happy to accept), then the change from Newton's to Fresnel's theory cannot have been a rational change. Why? Because Fresnel's postulation of the theoretical entity of the aether clearly violated the method associated with Newtonianism (hypotheses non fingo). Fresnel's theory was thus introduced irrationally (by the lights of the Newtonian method) before effecting a change of methodology in its own favor! Worrall also points out that the idea of changing scientific methodology is not compatible with the idea of progress. In a nutshell, Worrall argues that one theory cannot be judged better than another theory if there is no universal set of norms against which this change can be compared to. In unmistakingly Lakatosian terms Worrall asks "[w]hat is the basis for the judgement that the empirical sciences have become increasingly sophisticated as opposed to degenerately baroque?" (ibid., 381). Later Laudan adopted the view that progress is to be judged from our current modern perspective, rather than by the lights of the actors of the scientific period concerned (Laudan 1987a). But also that is of course not enough to fend off relativism, as Laudan later rather teeth-gnashingly admits (Laudan 1987b).

Worrall defends fixed methodology against the background of epistemology: no fixed methodology, no objective preference for theories, no epistemic progress (see also the critique by Doppelt 1986). But fixed methodology can be defended on different grounds. There is a simple semantic argument for this. The view that there are different *scientific* methodologies for different *scientific* fields but no overarching scientific methodology is incoherent: if methodologies of different scientific fields are supposed to be scientific, there must be something that these methodologies share for them all to be *scientific* methodologies. Of course, one may hold that there is no overarching scientific methodology (e.g. Dupré 1995; Cartwright 1999), but at best methodologies for physics, chemistry, biology, etc. (if even that). Such a view, however, is at odds with our common sense notion of different *scientific* fields. Minimally that view would have to be amended with some kind of 'error theory' according to which we are simply mistaken when using the term 'scientific' as picking out a class of methodologies that share important features. Such an account would also have to explain why we customarily refer to the methods used in physics, chemistry, and biology, when we speak of science, rather than to the methods used in, say, astrology, alchemy, creationism, or even sociology.

The *Kuhnian mode of HPS*, it should be noted, is not incompatible with epistemic rationality (although some elements of Kuhn's philosophy clearly are). The notion of structural realism, for instance*, is* indeed an example of an *epistemically* rational concept. It provides an epistemic rationale for theory-change because it has it that false theories which had genuine empirical success contained *true parts* that were subsequently retained. As long as the above two tenets of the discovery and justification of norms are respected also epistemic norms can qualify as results of the *Kuhnian mode of HPS*.

Third, and perhaps most importantly, although the *Kuhnian mode of HPS* is compatible with instrumentalist notions of rationality, it is not committed to an *empiricist* interpretation of instrumental rationality, which Laudan holds, as noted earlier. Again,

> Whether our methods, conceived as means, promote our cognitive aims, conceived as ends, *is largely a contingent and empirical question*. What strategies of inquiry will be successful *depends entirely on what the world is like*, and what we as prospective knowers are like. One cannot settle a priori whether certain methods of investigation will be successful instruments for exploring this world, since whether a certain method will be successful *depends on what the world is like*. (Laudan 1987b, 231, added emphasis)

On the *Kuhnian mode of HPS*, by contrast, the justification of norms about science is decidedly not dependent on the *actual* state of affairs. That is, for something to be a norm on the *Kuhnian mode of HPS* it is not required, contrary to Laudan's account, that certain aims (e.g. efficient science) are *as a matter of fact* achieved by actually following certain norms. It is sufficient that it is *plausible* that, counterfactually, the ends *would* be achieved if certain norms (e.g. focus on solvable puzzles) *were* to be respected. The answer to such questions, contrary to Laudan's liking, we do regularly answer *a priori*. Consider once more the concept of normal science. How is the norm to leave aside recalcitrant anomalies in order to ensure the efficiency of scientific work to be assessed? Empirically, as Laudan would have it? Presumably that would involve studying empirically whether or not scientists *frequently* succeed in increasing the efficiency of their work by leaving aside recalcitrant anomalies. Is that at all plausible? I think not. Rather, the normal science norm to leave aside recalcitrant puzzles is to be judged rational because it maximizes utility. And utility maximization which can be understood *a priori* by means of a well worked out mathematical theory.

For illustration, consider a toy example analogous to the normal science concept. Suppose I have several tasks that I want to get finished by the end of next week. I want to tidy up my flat, do some work in the garden, get my car fixed, see my doctor, mark some essays, and finish a paper. I have no preference for any of those tasks; they all need to be carried out as soon as possible. However my paper writing turns out to take longer than anticipated. I have only two options. I either keep working on this but then have less time for the other tasks and run the risk of not finishing those tasks either. Or I leave the paper aside and have a good chance of getting all the other tasks done by the end of the week, just as I had planned. Given that I have no preference for any one of those tasks (they all need to be done as soon as possible) the second option clearly is the more rational one for me. And this is really just analogous to the normal science case. In both cases it is considered what one ought to do when one is trying to tackle a bunch of tasks with a *limited amount of resources* whereby (i) one task is taking longer than anticipated and (ii) the rest of one's tasks being manageable. This seems to be a instance of a classical utility maximization problem. In order to decide on the right rational action to be taken in such problems, no recourse to empirical investigations about actual agents needs to be sought. On the contrary, such investigations might be counter-productive. In the above example, I might fall sick and not reach my goal of finishing as many tasks as possible by the end of the week *despite* following the norm of leaving aside my troublesome paper writing. Again, what underlies our judgment about what's rational and what's not in both cases appears to be some basic utility maximization.

But not all of the examples I mentioned in this paper can be understood in this way. Take the concept of use-novelty. Recall that it comes with the norm not to use the evidence in the construction of theories that are supposed to entail that evidence. Suppose the appropriate instrumental norm is one in which the goal is it not to produce *ad hoc* theories. Thus, if your goal is it not to produce ad hoc theories, do not use evidence in the construction of theories that entail that evidence. But given how *ad hoc* theories are defined on the use-novelty view, namely as theories which have been constructed to entail the evidence, the instrumental norm reduces to an analytic statement: if your goal is it not to produce theories that have been constructed by using the evidence that they entail, do not use evidence in the construction of theories that entail that evidence.[10] Empirical investigations into means-end relationships appear to be entirely misplaced here. But of course this is not to say that the use-novelty concept is correct. In fact, it can be shown—by some good-old conceptual analysis—that it's mistaken (see BLINDED).

5 Conclusion

In this paper I argued that a challenge put forth by Giere (1973) about the fact—norm relationship in HPS does not apply to a particular way of doing HPS, namely the *Kuhnian mode of HPS.* In the *Kuhnian mode of HPS* historical facts, although clearly motivating the construction of norms about science, are not the justifiers of those norms. What this paper decidedly did not do was to provide a fully-fledged out account of what the justifiers of norms constructed in the *Kuhnian mode of HPS* actually are. But as indicated throughout this paper, I believe the right account of the rationality of scientific norms is going to be an *a priori* account of rationality. Indeed the examples considered in the last section suggest that there is not going to be an account in which there will be one-fits-all justifiers of norms but rather a host of different *a priori* methods for assessing scientific norms. And as should have become clear by now, such an *a priori* account would not at all be divorced from the practice of science. On the contrary, the motivation to introduce new norms originates in the study of scientific practice. In this context it is worth noting that the norms constructed within the *Kuhnian mode of HPS* which I discussed in this paper share a common feature: they are all logically weaker notions than the notions they replaced. Normal science is compatible with more possible worlds (namely with worlds where there are apparent falsifiers) than the Popperian norm to reject theories when they face apparent falsifiers. Use-novelty is compatible with worlds in which theories are strongly confirmed by evidence that they did not predict, whereas temporal novelty is not. And structural realism is compatible with worlds in which scientists—again simplistically speaking—discard the 'content' of theories when moving on to the next theory, whereas naïve realism isn't. So it seems as we learn more facts about the history of science (often counterintuitive ones), our view of science becomes

[10] This point I adopted from (Kaiser 1991, 442) who argues this in a very similar context.

not only more nuanced but also more permissive without compromising on the view that science is a rational enterprise. Because these norms constructed in the *Kuhnian mode of HPS* are more permissive, they are compatible with more historical facts. That is why we can say that through the *Kuhnian mode of HPS* we capture ever more historical facts about science in rational terms without those historical facts being the justifiers of the norms we so construct.

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