Kuhn vs. Popper on Criticism and Dogmatism in Science: A Resolution at the Group Level

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Popper repeatedly emphasised the significance of a critical attitude, and a related critical method, for scientists. Kuhn, however, thought that unquestioning adherence to the theories of the day is proper; at least for 'normal scientists'. In short, the former thought that dominant theories should be attacked, whereas the latter thought that they should be developed and defended (for the vast majority of the time).

Both seem to have missed a trick, however, due to their apparent insistence that each individual scientist should fulfil similar functions (at any given point in time). The trick is to consider science at the group level; and doing so shows how puzzle solving and 'offensive' critical activity can simultaneously have a legitimate place in science. This analysis shifts the focus of the debate. The crucial question becomes 'How should the balance between functions be struck?'

1. Introduction – *Criticism and the Growth of Knowledge*

Criticism and the Growth of Knowledge was the flashpoint for a well-known debate between Kuhn and Popper, in which the former emphasised the importance of 'normal science' *qua* puzzle solving and the latter (and his supporters) questioned the very idea that 'normal science', so construed, could count as good science at all.

Kuhn's basic idea was that science would hardly get anywhere if scientists busied themselves with attacking what they already had, rather than accepting and refining it. But Popper instead emphasised the importance of striving to overthrow theories that might very well, for all their designers and users knew, be false (or otherwise unfit for purpose). The debate was inconclusive because both sides clearly had a perfectly reasonable point.

The natural solution would have been to take the middle ground – i.e. to suggest that scientists should feel free to attack or to uncritically accept – but both Popper and Kuhn appear to have avoided this option because they ultimately considered the matter, I will contend, only at the level of the individual scientist. By this, I mean that each thought about how one particular scientist ought to behave, and then extrapolated from this to determine how they thought a group of scientists should behave. And on the one hand, it is hard to see how it can be right for an individual (and by extrapolation every) scientist to blithely assume that what everyone else is doing is basically right, and that the theories she is given are fit for purpose. On the other, it is difficult to see how it could be right for a (and by extrapolation every) scientist to treat the canon of her science as fundamentally wrong, and to spend all her days objecting to the basic metaphysical principles underlying it.

In this paper I will explain how thinking at the level of the group, using a functionalist picture of science, provides a means by which to resolve the tension. I do not claim that this notion is entirely new – Hull (1988), Kitcher (1990) and Strevens (2003) all discuss the importance of the division of cognitive labour, although not with emphasis on attitudes – but I would venture to declare that the subsequent analysis is *far* more penetrating than that recently offered by Domondon (In Press) in this journal, and previously by Fuller (2003).[1] In fact, it shows precisely how we can resolve, and move beyond, the kind of 'Kuhn vs. Popper' problems with which they are concerned. I shall begin by explaining the positions of Popper and Kuhn in greater depth.

2. Popper on Criticism

From the beginning of his career, Popper pushed the idea that a critical attitude is at the heart of the scientific persona, and that a critical method is its proper counterpart. Despite his well-known emphasis on the importance of falsifiability, he acknowledged in the original version of *The Logic of Scientific Discovery* (i.e. in 1934 but only translated into English in 1959) that:

> A system such as classical mechanics may be 'scientific' to any degree you like; but those who uphold it dogmatically – believing, perhaps, that it is their business to defend such a successful system against criticism as long as it is not *conclusively disproved* – are adopting the very reverse of that critical attitude which in my view is the proper one for the scientist. (Popper 1959, p.50)

Just a little later, in an article in *Mind*, he declared:

[1] Forgive me if this sounds immodest. My view may simply be a result of the gulf between the philosophy of science and science studies. No doubt the reader can reach a judgement as to the relative accuracy and depth of our respective treatments.

> [I]t is the most characteristic feature of the scientific method that scientists will do everything they can in order to criticize and test the theory in question. Criticizing and testing go hand in hand: the theory is criticized from very many different standpoints in order to bring out those points which may be vulnerable... (Popper 1940, p.404)

And throughout the rest of his career, at least until *Criticism and the Growth of Knowledge* appeared, Popper adhered to this line. In *The Open Society and its Enemies*, he wrote: '[R]ationalism is an attitude of readiness to listen to critical arguments and to learn from experience.' (Popper 1945, vol. II, p.249). In the preface to the first English edition of *The Logic of Scientific Discovery*, he wrote: 'I equate the rational attitude and the critical attitude. The point is that, whenever we propose a solution to a problem, we ought to try as hard as we can to overthrow our solution, rather than defend it.' (Popper 1959, p.16). Sure enough, the message is the same in *Conjectures and Refutations*: 'The critical attitude, the tradition of free discussion of theories with the aim of discovering their weak spots so that they may be improved upon, is the attitude of reasonableness, of rationality.' (Popper 1963, p.67) And even in more obscure places, e.g. in Popper's response to a critique of his views on demarcation by his ex-student W. W. Bartley, we find passages such as the following:

> [W]hat characterizes the scientific approach is a highly *critical attitude* towards our theories rather than a formal criterion of refutability: only in the light of such a critical attitude and the corresponding critical methodological approach do 'refutable' theories retain their refutability. (Popper 1968, p.94)

The point behind all this textual evidence is not to belabour the point that Popper was pro-criticism. Rather it is supposed to throw the following passage, in *Criticism and the Growth of Knowledge*, into sharp relief:

> I believe that science is essentially critical... But I have always stressed the need for some dogmatism: the dogmatic scientist has an important role to play. If we give in to criticism too easily, we shall never find out where the real power of our theories lies. (Popper 1970, p. 55)

What happened? Only at two other points before 1970, in Popper's published works, did he suggest that there is a need for dogmatism in science. One key passage is as follows:

> [D]ogmatism allows us to approach a good theory in stages, by way of approximations: if we accept defeat too easily, we may prevent ourselves from finding that we were very nearly right. (Popper 1963, p.64)[2]

However, the extent to which dogmatism is useful, according to this view, is only in so far as we are fallible. That is to say, dogmatism will only prove useful on those occasions where we are 'very nearly right' despite evidence to the contrary. But how about when we are wrong? And furthermore, might we not accept a methodological

[2] This passage is from a chapter based on a lecture delivered in 1953, but I do not know if it appeared in the original. (The other place is a footnote in Popper's revised version of 'What is Dialectic?', again in *Conjectures and Refutations*. In the original paper in *Mind* – Popper 1940 – the relevant footnote did not appear.) It would be interesting to see if this is an addition made after 1961, when Kuhn presented his paper on the function of dogma in Oxford (later published as Kuhn 1963). Gattei (2008, p.40) notes that Lakatos attended Kuhn's talk, and also that it caused somewhat of a stir. It is therefore safe to assume that Popper knew about it.

rule such as ‘Do not accept that a theory is falsified too easily’ without being dogmatic at all? We will return to these questions when we have looked at what Kuhn had to say about dogmatism.

3. Kuhn on Dogmatism

In contradistinction to Popper, Kuhn suggested that adherence to the *status quo* was characteristic of actual ‘normal’, and derivatively good, science.[3] Infamously, Kuhn (1996, p. 80) claimed that an experiment which backfires is normally taken, *and should normally be taken*, to reflect badly on the scientist that performs it: ‘Failure to achieve a solution discredits only the scientist and not the theory … “It is a poor carpenter who blames his tools”...’

Moreover, Kuhn (ibid., p. 80) suggested that normal science can enable us ‘to solve a puzzle for whose very existence the validity of the paradigm must be assumed’. So in short, he thought that work within a paradigm (*qua* disciplinary matrix) is possible only if that paradigm is taken for granted. Later in *The Structure of Scientific Revolutions*, he expressed this view at greater length:

> [T]rial attempts [to solve puzzles], whether by the chess player or by the scientist, are trials only of themselves, not of the rules of the game. They are possible only so long as the paradigm itself is taken for granted. (Kuhn 1996, p. 144–145)

[3] In fact, Kuhn (1970b, p.233) suggested that ‘the descriptive and the normative are inextricably mixed’.

As I have argued elsewhere (Rowbottom 2006 and Rowbottom Forthcoming A), this is an elementary mistake. On the contrary, such puzzles 'exist' *whether or not* the paradigm is assumed to be valid, because *we only need to consider what would be the case if the paradigm were valid in order to examine them.* By way of analogy, we can consider whether *p* & *q* follows from *p* and *q* in classical logic without assuming that both *p* and *q* are true, or indeed assuming that classical logic is fit for purpose (whatever our stance on psychologism in the philosophy of logic happens to be).

Kuhn (1996, p. 24) used similarly strong language elsewhere:

> By focusing attention upon a small range of relatively esoteric problems, the paradigm *forces* scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable...' [My emphasis]

However, we might interpret this a little more loosely by thinking about what is 'forced' in order to demonstrate one's proficiency in, and even to remain a recognised worker in, a discipline. In essence, Kuhn appears to have thought that scientists would not be *motivated* to tackle such esoteric problems (or puzzles) without rigid belief – or even faith – in the paradigm.

This claim may also be somewhat dubious, however, because it is possible to do things for extrinsic reasons. I could learn to recite a poem in order to impress a prospective lover without having any interest in verse or metre, just as a scientist could be content to solve puzzles, in the short term, in order to support himself and slowly build a reputation which would lead to some of his potentially revolutionary ideas being taken more seriously by his peers.

Nevertheless, Kuhn had a valid point to the extent that he was worried about scientists becoming *hypercritical*: 'The scientist who pauses to examine every anomaly he notes will seldom get significant work done.' (Kuhn 1996, p.82) And this, as we will see in the next section, is where Kuhn's objection to Popper's emphasis on criticism has some bite. If high confidence (even if not blind faith) in the paradigm *qua* disciplinary matrix did not put some areas 'off-limits' for legitimate criticism, then scientists would be stuck arguing over fundamentals.

4. Kuhn vs. Popper in *Criticism and the Growth of Knowledge*

The stage has now been set for an examination of the exchange between Popper and Kuhn in *Criticism and the Growth of Knowledge*. We have seen that the former emphasised the importance of criticism and non-conformity in science, whereas the latter thought that conformity and focused puzzle-solving are essential (at least in 'normal science'). It is therefore clear that they were set for a collision course when they were brought together, as Gattei (2008, ch.2) illustrates.[4]

[4] For example, Gattei (2008, p. 40) notes that the provisional programme for the Colloquium on which *Criticism and the Growth of Knowledge* was based: 'describes the session as follows:

July 13, Tuesday	*Criticism and the Growth of Knowledge I*
Chairman:	Sir Karl R. Popper
9:15–10	T.S. Kuhn: *Dogma versus Criticism*
10:15–11	P. Feyerabend: *Criticism versus Dogma*
11:15–12:45	Discussion'

He adds: 'Fundamental here are the contrasting words "criticism" and "dogma", chosen in order to emphasize the differences and characterize the two opposing positions – two diametrically opposed positions.' Gattei (2008, p.54) also defends the further claim that 'the critical reference of Kuhn's philosophy has always been Popper's falsificationism'.

Perhaps we should start by noting that Popper agreed with Kuhn that ‘normal science’ exists. Unsurprisingly, however, he did not describe it in flattering terms:

> “Normal” science, in Kuhn’s sense, exists. It is the activity of the non-revolutionary, or more precisely, not-too-critical professional: of the science student who accepts the ruling dogma of the day; who does not wish to challenge it; and who accepts a new revolutionary theory only if almost everybody else is ready to accept it – if it becomes fashionable by a kind of bandwagon effect. (Popper 1970, p.52)

Popper (ibid.) continued by expressing pity for the predicament of ‘normal scientists’, with reference to educational norms: ‘In my view the “normal” scientist, as Kuhn describes him, is a person one ought to be sorry for... He has been taught in a dogmatic spirit: he is a victim of indoctrination. He has learned a technique which can be applied without asking for the reason why (especially in quantum mechanics)...’

Kuhn disagreed with Popper not because he thought that criticism is unimportant for scientific progress (whatever that may consist in[5]), but rather because he thought that it should only be occasional. (We can admit, of course, that puzzle-solving involves *some* criticism. The point is just that the scope of this is rather narrow.) Kuhn summarized his view as follows:

[5] Kuhn and Popper disagreed on the aim of science. However, this issue can be put to one side for present purposes.

> Sir Karl... and his group argue that the scientist should try at all times to be a critic and a proliferator of alternate theories. I urge the desirability of an alternate strategy which reserves such behaviour for special occasions... Even given a theory which permits normal science... scientists need not engage the puzzles it supplies. They could instead behave as practitioners of the proto-sciences must; they could, that is, seek potential weak spots, of which there are always large numbers, and endeavour to erect alternate theories around them. Most of my present critics believe they should do so. I disagree but exclusively on strategic grounds... (Kuhn 1970b, p. 243 & p. 246)

But what are the strategic grounds upon which Kuhn made his recommendation? His fundamental idea was that only by working positively with our current theories for a considerable period – trying to refine them, improve and increase their applicability, and so forth[6] – can we discover their true strengths and weaknesses. And then if we do decide that change is needed, we will know where to focus our attention:

> Because that exploration will ultimately isolate severe trouble spots, they [i.e. normal scientists] can be confident that the pursuit of normal science will inform them when and where they can most usefully become Popperian critics. (Ibid., p. 247)

One problem with Kuhn's suggestion is that he leaves it so vague. It is not clear, for instance, what counts as a severe trouble spot (and who should get to decide). Furthermore, it is unclear how long we should stick with a theory in the face of

[6] For more on the sorts of activity supposed to occur in 'normal science', see Kuhn 1996, ch. 3 and Rowbottom Forthcoming A.

trouble spots. And finally, crucially, it is unclear why working with a theory for a long time should improve the chance of isolating *genuine* limitations of the theory. This is clearer when we consider Kuhn's proposed strategy in the light of Duhem's (1954, pp. 183-90) thesis – that 'an experiment … can never condemn an isolated hypothesis but only a whole theoretical group… [so] a "crucial experiment" is impossible'.

In short, the salient question is "When should one challenge the theory itself, rather than the auxiliary assumptions used in order to derive predictions from it?" Kuhn seems to have suggested that the auxiliaries do (and should) always give way in 'normal science'.[7] Naturally this is completely at odds with Popper's (1959, p. 83) dictum that: 'As regards *auxiliary hypotheses*… only those are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question.'

Clearly it would be foolish to recommend that we should always consider theories falsified when predictions derived from them, in combination with auxiliary hypotheses, are inconsistent with observations. So Kuhn's recommended strategy is certainly an improvement on naive falsificationism (which was never actually endorsed by Popper).

But let us now reconsider the passage highlighted in the earlier discussion of Popper's views on criticism:

[7] Kuhn does not mention Duhem, or Duhem's problem, explicitly; rather, I propose that this is a helpful way to understand his view that 'anomalous experiences may not be identified with falsifying ones' (Kuhn 1996, p.146).

> [T]he dogmatic scientist has an important role to play. If we give in to criticism too easily, we shall never find out where the real power of our theories lies. (Popper 1970, p. 55)

We can now see that Popper might instead have said that we should be willing to *criticise* auxiliary hypotheses as well as theories, and that we shouldn't be too quick to condemn the latter rather than the former.[8] But there is quite a difference between saying this and saying that 'the dogmatic scientist has an important role to play', because presumably it is possible to attack auxiliary hypotheses rather than theories, in the light of evidence that falsifies the conjunction thereof, *without* being dogmatically committed to the theories. Therefore Popper did not intend 'dogmatism' in the same sense that Kuhn did, as he continued by pointing out:

> [T]his kind of dogmatism is not what Kuhn wants. He believes in the domination of a ruling dogma over considerable periods; and he does not believe that the method of science is, normally, that of bold conjectures and criticism. (Ibid.)[9]

[8] For more on how Duhem's thesis may be handled from a falsificationist perspective, by extending the notion of corroboration to auxiliary hypotheses, see Rowbottom Forthcoming B.

[9] Writing of a similar earlier passage and anticipating some of this paper's later findings, Musgrave (1974, pp. 580–581) also notes that Popper's comments on dogmatism: '... might seem to conflict with his more frequent emphasis on the desirability of a critical attitude. The apparent conflict is heightened by the psychologicist terminology – to resolve it, we must read 'attitude' in a non-psychological way in both places. But then what is of 'considerable significance' is not a dogmatic attitude *as such*: a dogmatic attitude towards *T* will only be fruitful if it leads a scientist to improve *T*, to articulate and elaborate it so that it can deal with counterarguments; it will be unfruitful if it means merely that a scientist sticks to *T* without improving it.' I will later argue, however, that sometimes a dogmatic attitude may allow a scientist to do things that he otherwise would not.

Popper's comment here is fair, because Kuhn was much more extreme in his claims about the value of shielding theories from criticism. The following two quotations, in particular, illustrate this:

> It is precisely the abandonment of critical discourse that marks the transition to a science. (Kuhn 1970a, p. 5)

> Lifelong resistance, particularly from those whose productive careers have committed them to an older tradition…is not a violation of scientific standards (Kuhn 1996, p. 151)

It is not entirely misleading, therefore, to paint Popper and Kuhn as two extremists on the issue of the role of criticism in science. On the one hand, Popper suggested – at many points in his writing, at least – that:

(P) Each and every scientist should have a critical attitude (and follow the same critical procedures).

On the other, Kuhn – or at least a slight caricature of Kuhn[10] – suggested that:

(K) Each and every scientist should puzzle solve within the boundaries of the disciplinary matrix, on the basis of the exemplars therein, until almost every scientist comes to see particular failures as indicating serious anomalies.[11]

[10] I would defend the view that this is a fair interpretation of Kuhn's view in the first edition of *The Structure of Scientific Revolutions*. Several commentators on this paper have suggested it is a caricature of his later position; I disagree to the extent that I think he maintained that non-puzzle-solving functions are required only in extraordinary science.

I should emphasise that (K) only goes for 'normal science', and that 'puzzle solving' involves many different forms of activity (as shown later in figure 5). It is crucial to the plausibility of Kuhn's view that 'rational' disagreement between scientists (Kuhn 1977, p.332) is permissible during periods of extraordinary science.

It appears to follow that it is necessary (and not merely sufficient) for the best possible science for each and every scientist to either (on P) be maximally critical, or (on K) be an expert puzzle solver (and/or to let critical activity have a narrow and well-defined scope for most of the time). So each and every scientist is expected to perform the same functions, *qua* scientist, on either view.[12] Failure to do so will lead to something less than ideal science.

In what follows, I shall challenge this notion that all scientists should adopt similar stances, and suggest that the best possible science may be realizable in more than one way. I will also suggest that there is a place for dogmatism in something close to Kuhn's sense when we look at matters at the group level, but also that critical procedures are crucial. The key idea, as Kitcher (1990, p. 6) puts it, is that there is 'a mismatch between the demands of individual rationality and those of collective (or community) rationality'.[13]

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[11] Of course the *proper* mechanism by which scientists should come to see particular failures as indicating serious anomalies is never satisfactorily explained in Kuhn's work. And furthermore, it appears that one scientist or another will have to start a chain reaction by questioning the boundaries of the disciplinary matrix. But for present purposes, let us put this to one side.

[12] This may be slightly unfair to Kuhn, because puzzle-solving involves a variety of activities. But Kuhn nowhere explained why some scientists might engage in one type of puzzle-solving, and others engage in another. Furthermore, he did not endeavour to say how (or even if) a balance ought to be struck. For more on this issue, see footnote 20 and the discussion of stances and paradigms in Rowbottom Forthcoming A.

[13] Why did Kuhn and Popper not consider this possibility? It is perhaps easier to say for the latter, given his methodological individualism and the focus on proper scientific method for the individual of

5. A Functional Analysis

The differences between Kuhn and Popper can be neatly understood by thinking in terms of functions, as I will show below. Moreover, thinking in this way suggests a means by which to resolve their debate; namely to consider functions at the group, rather than the individual, level.

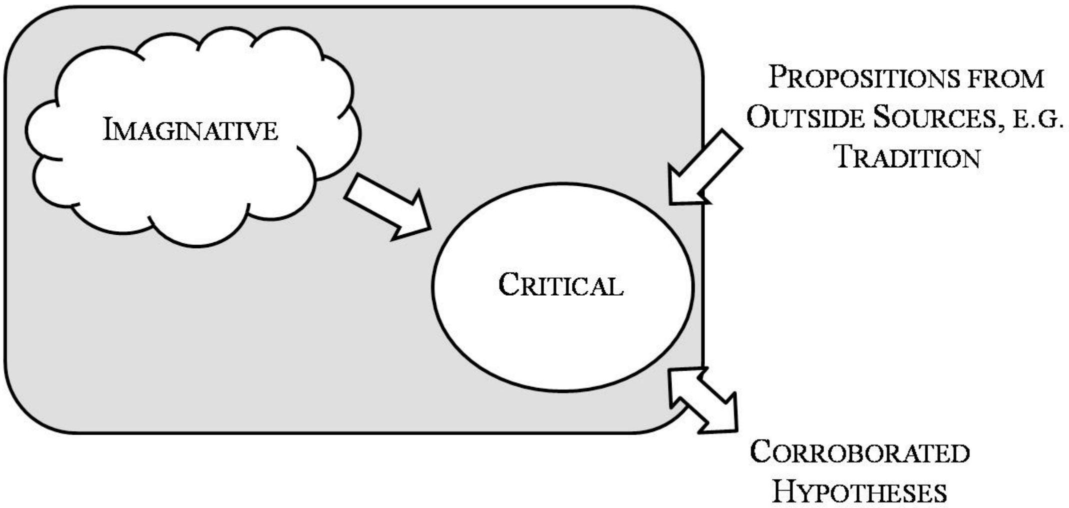

Fig. 1 – The Simple Popperian Scientist

earlier philosophers of science, such as the logical positivists. In short, Popper strongly emphasised the importance of inquirers critically engaging with one another and the significance of tradition, but did not develop a theory of testimony (such as the one offered by Diller 2008) or of non-critical interaction in inquiry more generally. (In his favour, though, I should add that Popper (1975) noted 'that should individual scientists ever become "objective and rational" in the sense of "impartial and detached", then we should indeed find the revolutionary progress of science barred by an impenetrable obstacle'.) The former, on the other hand, was clearly in the correct intellectual territory to think about research groups in terms of inputs, outputs, and functions (or processes). Tentatively, I would suggest that his failure to move to a view of group rationality as distinct from individual rationality was as a result of what he saw in the history of science, from which he derived his normative conclusions, and what Watkins (1970, p. 34) calls his 'Paradigm-Monopoly thesis' that a 'scientist cannot, while under the sway of one paradigm, seriously entertain a rival paradigm'. Ultimately, Kuhn thought that sharp discontinuities (at the group level) are necessary for good science; but allowing for individual scientists to (rationally) do highly different things is to render such discontinuities unnecessary. Thus, Kuhn failed to entertain the notion that group functions and individual functions might come apart.

The simple Popperian scientist fulfils two functions (which fall inside the grey area in Fig. 1): one imaginative, and the other critical.[14] In short, the scientist uses propositions from outside sources (e.g. tradition and experience) to criticise his hypotheses (which are often derived from his imagination). The critical function may involve several procedures, e.g. non-empirical checks for internal consistency as well as empirical tests. Those hypotheses that survive the process count as corroborated (and are outputs). But simply because a hypothesis is corroborated, this does not mean that it is no longer subject to the critical function (and hence the bidirectional arrow between 'critical' and 'corroborated hypotheses').

There is, however, one rather striking feature of the Popperian scientist so depicted; he is purely theoretical in orientation. This appears to be the correct view of Popper's position because he suggests that applied science is the province of 'normal scientists':

> [The normal scientist] has become what may be called an applied scientist, in contradistinction to what I should call a pure scientist. He is, as Kuhn puts it, content to solve 'puzzles'… it is not really a fundamental problem which the 'normal' scientist is prepared to tackle: it is, rather, a routine problem, a problem of applying what one has learned… (Popper 1970, p. 53)

So what should we think of Popper's mention of dogmatism? If we imagine this as a function at the individual level, we will arrive at a somewhat different view from that

[14] Bartley (1984, pp.182–183) emphasised the importance of creativity, i.e. the imaginative function, as follows: '[A]n essential requirement is the *fertility* of the econiche: the econiche must be one in which the creation of positions and contexts, and the development of rationality, are truly inspired. Clumsily applied eradication of error may also eradicate fertility.'

depicted in Figure 1. Instead there will be a 'dogmatic filtering' function, in addition to the critical and imaginative functions, which will serve to ensure that some propositions – and in particular, some theories – are not criticised. As we can see from Figure 2, however, it is unclear that such a filter need be 'dogmatic' in any strong sense of the word. This is because the filter may function such that no (empirical) theory is *in principle* immune to being passed on to the critical procedure. So if a theory is brand new, for instance, perhaps it will be shielded from criticism until it can be further developed (by the imaginative, or creative, function); hence the bidirectional arrow between the imaginative and filtering functions. But if a theory is well-developed, i.e. has had a lot of imaginative effort spent on it, perhaps it will always pass through the filter.[15]

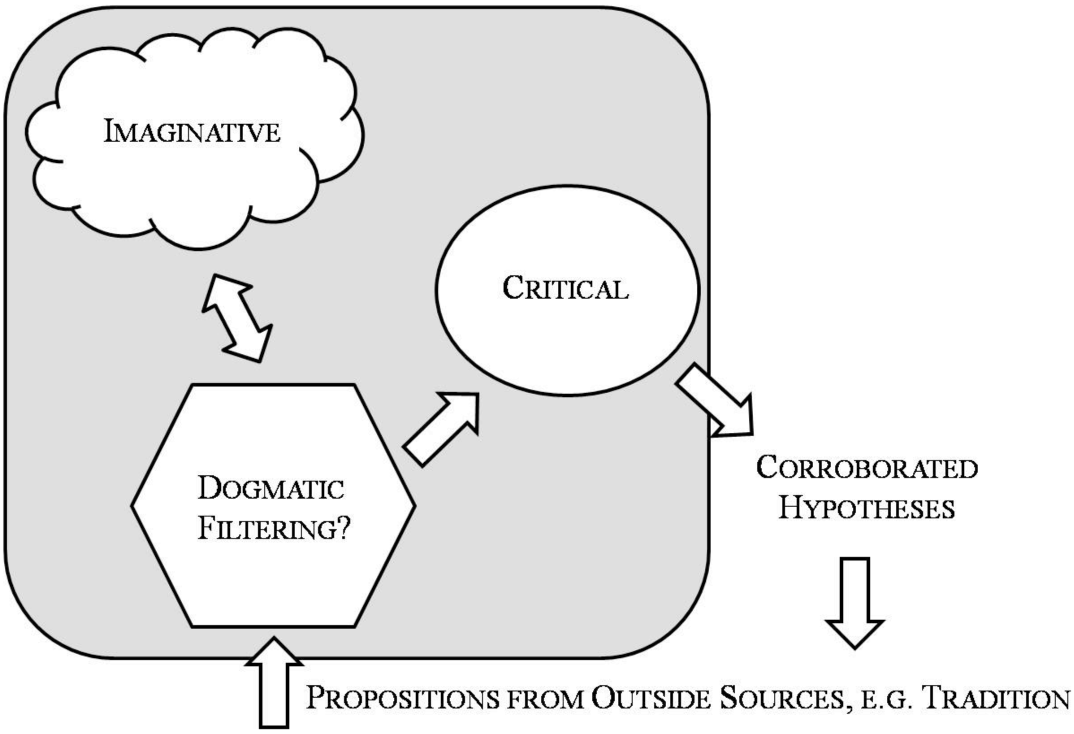

Fig. 2 – The Sophisticated Popperian Scientist

[15] There may also be other ways to set up filters, e.g. to determine which theories or auxiliaries are attacked first. See Rowbottom Forthcoming B.

Let us now compare this with the Kuhnian normal scientist. In contrast to her Popperian counterpart, her primary function is to solve puzzles. And in order to do this, she relies on established scientific theories and data. (We should allow that some of the data used may not itself be a product of science. However, in so far as observations are heavily theory-laden, on Kuhn's view, it is likely that said data will be given an interpretative slant – and/or that what counts as admissible data will be determined – by the disciplinary matrix.) The output of puzzle solving is both theoretical *and* concrete; that is to say, Kuhn does not draw a sharp distinction between 'pure' and 'applied' science in the manner that Popper does.

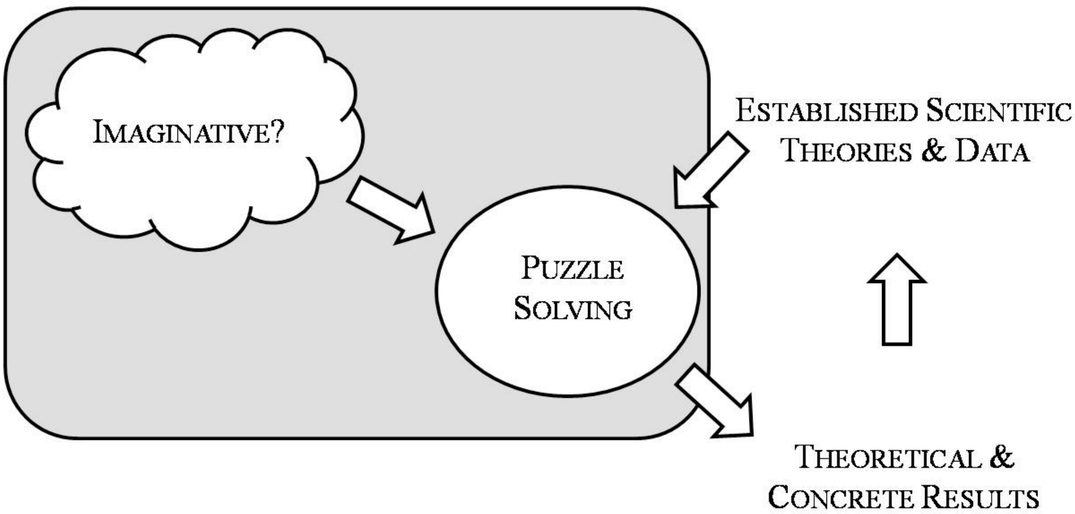

Fig. 3 – The *Prima Facie* Kuhnian Normal Scientist

We might wonder, though, whether good puzzle solving doesn't require a good degree of imagination, and therefore if the imaginative function is not also, as depicted in figure 3, a required component of the Kuhnian scientist. Despite first appearances, a somewhat closer look at Kuhn's position appears to suggest that it is not, because exemplars provide *templates* for puzzle solving. As Bird (2004) puts it:

> In the research tradition it inaugurates, a paradigm-as-exemplar fulfils three functions:
>
> (i) it suggests new puzzles;
> (ii) it suggests approaches to solving those puzzles;
> (iii) it is the standard by which the quality of a proposed puzzle-solution can be measured.[16]

To remove the 'imaginative' function from the picture is not to suggest that puzzle solving does not require considerable ingenuity, on occasion, nor to concede that it is as 'routine' as Popper (1970) suggested. The point is simply that an incredibly difficult puzzle is still little more than a puzzle when the rules of the game and procedures for playing are all fixed. And Kuhn certainly does not suggest that (normal) scientists require anything like '"an irrational element", or a "creative intuition", in Bergson's sense' (Popper 1959, p.32). In the words of Kuhn (1963, p. 362):

> The paradigm he [the normal scientist] has acquired through prior training provides him with the rules of the game, describes the pieces with which it must be played, and indicates the nature of the required outcome. His task is to

[16] See also Rowbottom Forthcoming A, Bird 2000, pp. 68-69 and Hoyningen-Huene 1993.

> manipulate those pieces within the rules in such a way that the required outcome is produced.

We are therefore left with the picture below, depicted in Figure 4, in which exemplars remove the need for an imaginative function. (It is worth adding that an imaginative function may be required in extraordinary science, e.g. in order to bring exemplars in to being, but that we are not presently concerned with this.)

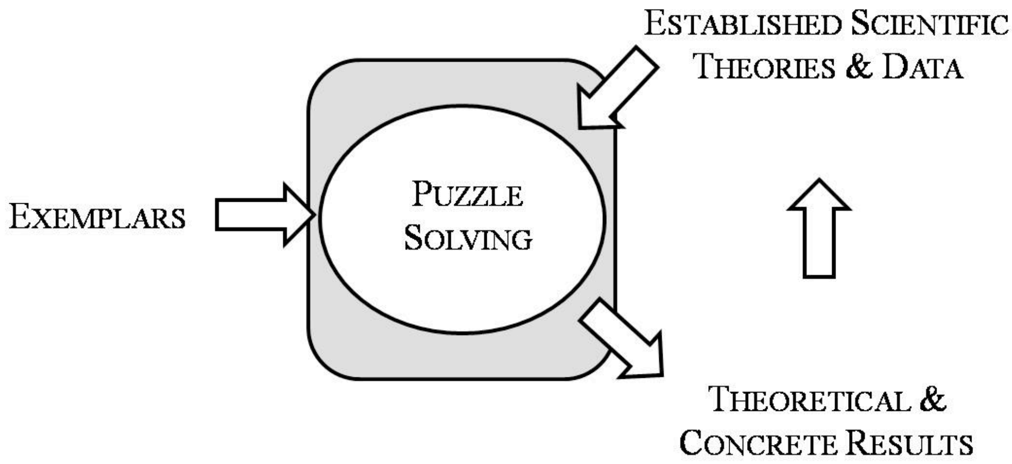

Fig. 4 – The Kuhnian Normal Scientist

Figure 4 does run the risk, however, of making Kuhn's picture look rather simpler than it actually is.[17] This is because many different activities fall under the rubric of 'puzzle solving', as Kuhn explains in chapter three of *The Structure of Scientific Revolutions*. So figure 5 gives a look inside the puzzle solving function, and shows that it is composed of several different processes. For a full discussion of these processes – classification and prediction, theory-experiment alignment, and

[17] The charge could equally be made that the critical function, in Popper's model, is composed of other functions. I accept this, however, as will be made apparent below.

articulation – see Rowbottom Forthcoming A. For present purposes, it suffices to note that these are significant functions within the function of puzzle solving.

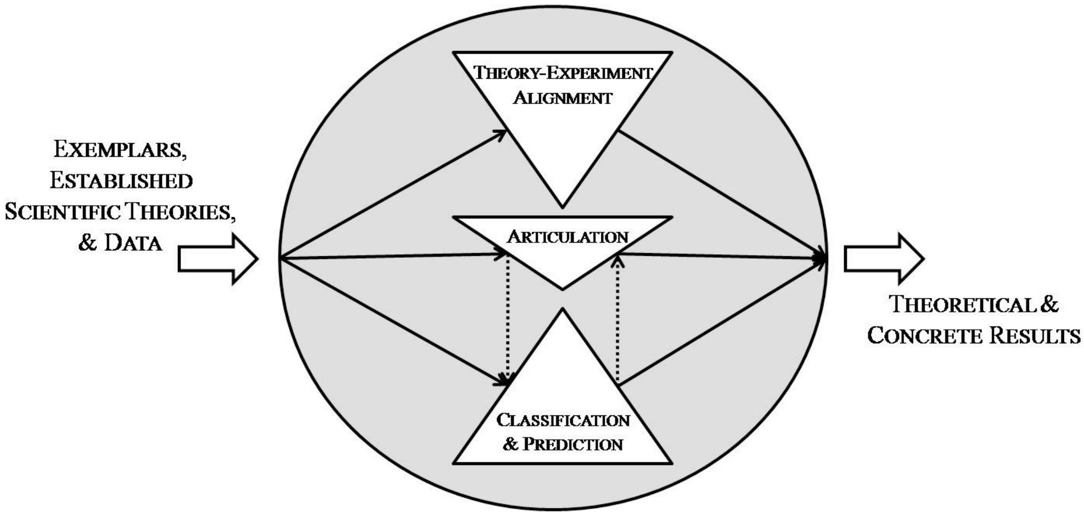

Fig. 5 – Inside Puzzle Solving

We have now seen that despite their strikingly different views of the ideal scientist, both Popper and Kuhn had understandings that can be modelled with ease via a functional perspective. For both, to be a good scientist is simply to perform specific functions. And good science is to be understood as an activity performed by large numbers of good scientists in precisely the aforementioned sense.

However, this functional analysis makes the following questions, which we will come to in the next section, salient. Why not have different functions performed by different scientists? And why not entertain the possibility that it is (sometimes) *necessary* for the functions be performed by different individuals in order for science to be (or to be as close as possible to) ideal?

6. Functions at the Group Level: A Hybrid Model

Moving to a consideration of functions at the group level allows us to consider, first and foremost, the possibility that both dogmatism and criticism are vital components of the scientific enterprise. And while it may be suggested that Kuhn would have agreed in so far as criticism might play a crucial part in extraordinary science, his picture is one where science *should* go through different phases. In short, his view appears to have been that either all scientists should be doing normal science, or all scientists should be doing extraordinary science. The possibility that it might be preferable for the two kinds of activity to co-exist, in so far as some might be dogmatic and others might be critical at the same time, was never dismissed on adequate grounds. Neither Kuhn nor Popper examined this possibility in any serious depth; the former because of his belief in the importance of revolutions (derived from his historical studies), and the latter because of his long-standing belief in the importance of criticism.[18]

[18] **Thus it took Feyerabend (1970, p.212) to suggest that 'the correct relation is one of *simultaneity* and *interaction.*'**

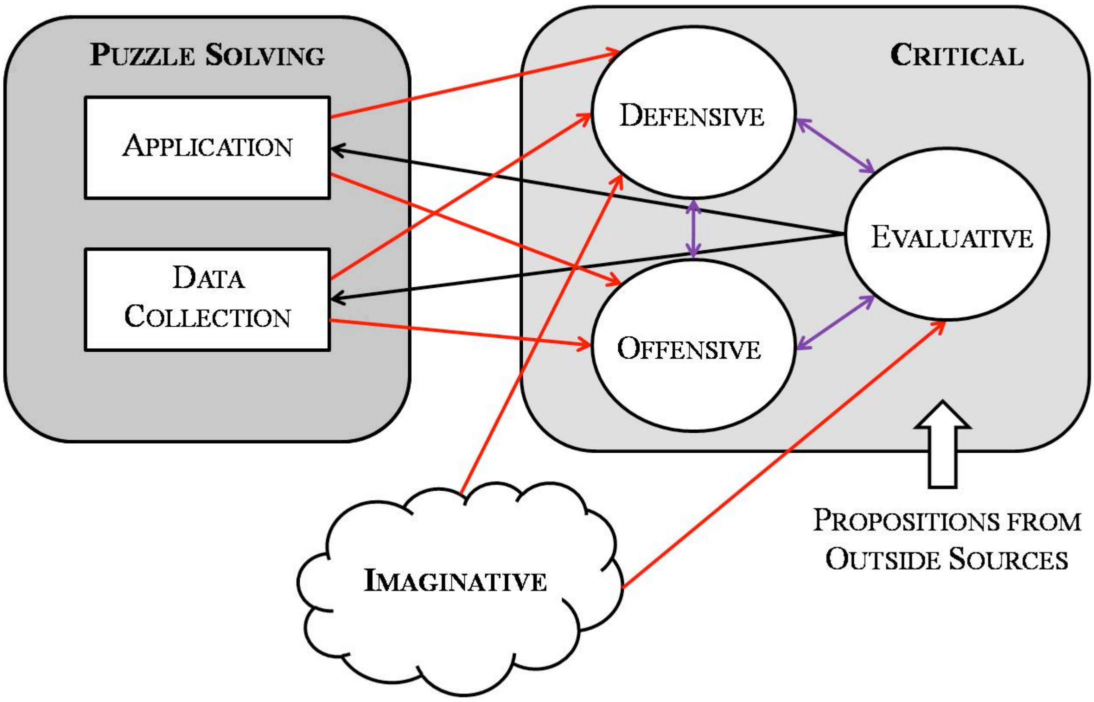

Fig. 6 – A Hybrid View of Science at the Group Level

Allow me to start by giving an overview of Figure 6, which may initially appear impenetrable. According to this model, (ideal) science involves each of the three primary functions discussed previously: imaginative, puzzle solving, and critical. The imaginative function provides *some* objects of criticism, which may be evaluated and rejected, or defended, attacked, and then subsequently evaluated. (Note also that said evaluation may rely on propositions from outside sources, e.g. tradition, too.) The critical function has three parts: offensive, defensive, and evaluative. These should be reasonably self explanatory, but will be illustrated in the course of the subsequent discussion.

Now it is crucial to distinguish between critical procedures (or methods) and the critical attitude. That is to say, *it is possible for science to perform a critical function* *with wide scope even when none of its participants have (completely) critical attitudes*. One simple way to see this is to imagine a scenario in which each scientist holds different assumptions dogmatically, but in which no peculiar assumption (*qua* proposition or theory) is held dogmatically by all scientists. *So at the group level no statement is beyond criticism*.

We can develop this idea by considering another simple scenario in somewhat greater depth. Imagine two dogmatists, D_1 and D_2, who are dogmatic only in so far as they will do anything to defend their individual pet theories, T_1 and T_2, which are mutually exclusive. So D_1 will *defend* T_1 at all costs, e.g. by challenging auxiliary statements used to generate predictions from T_1 in the event of the possibility of empirical refutation, just as D_2 will defend T_2. (This is fulfilling a *defensive* function.) To the death, neither D_1 nor D_2 will abandon their respective pets. But each will try to persuade other scientists – whether or not they try to persuade one another – that their own pet is superior. And as part of this, said dogmatists need not only defend their own pets against attack, but may also attack the rival pets of others. So part of D_1's strategy to promote T_1 may be to attack T_2, just as part of D_2's strategy to promote T_2 may be to attack T_2 (i.e. to fulfil an *offensive* function). Thus both dogmatists may fulfil (narrowly focused) critical functions of attack and defence.

But if everyone were such a dogmatist, stalemate (and perhaps even disintegration of science) would ensue. This is why a third critical function, that of evaluation, is crucial in order to judge whether T_1 or T_2 emerges victorious. Needless to say, such an evaluative function may be performed by interested third parties who are not themselves committed to either T_1 or T_2. So note that a third dogmatic scientist, D_3, who is set upon defending a theory T_3 which may stand irrespective of whether T_1 or T_2 is correct, may serve as an evaluator of the debate between D_1 and D_2. In short, to attack or defend as a result of dogmatism in one context does not preclude evaluating in another.

But how might dogmatic individuals benefit science in a way that their completely critical (and/or highly evaluative) counterparts might not? The simple answer is that they may be far more persistent in defending their pet theories (and therefore attacking competitor theories) than a more critical individual could be. So they might, for example, consider rejecting auxiliary hypotheses when their critical counterparts would not (and would instead simply reject a theory). But it is well worth re-emphasising that being dogmatic in this sort of sense does not preclude being critical. *Rather, the critical activity of such an individual will have narrow scope; it will be aimed only at defending pet theories, and attacking competitor theories*. So in short, to be critical in some small area is still to be critical, even though it is not necessarily to have the critical attitude that '*I may be wrong and you may be right, and by an effort, we may get nearer to the truth*' (Popper 1945, vol. II, p.249), or to be a pancritical rationalist in the sense of Bartley (1984). Just as there are occasions where 'a commitment to the paradigm was needed simply to provide adequate motivation' (Kuhn 1963, p. 362), there may be occasions where dogmatic commitment is crucial in order to push the scientist to consider avenues that would be ruled out by more evaluative individuals. My point here is that territory may be explored which would otherwise not be, and that this might result in a variety of fruits; I do not join Kuhn (1996, p.247) in thinking that such exploration will, as a general rule, be successful in isolating 'severe trouble spots'.

Furthermore, it may be a good thing for individual scientists to devote themselves to performing a small number of functions.[19] Perhaps, for instance, it is extremely difficult (due to human limitations) to be an expert puzzle solver and an expert attacker. Perhaps, indeed, the kind of person who is an expert attacker is often a lousy puzzle solver (because he or she finds it hard, *qua* boring, to work with externally imposed frameworks of thought or to perform repetitive tasks). So here we might say that something like van Fraassen's (2002, 2004a, 2004b) notion of a stance is relevant, especially if we think of this as involving *mode of engagement* and a *style of reasoning* (Rowbottom and Bueno Forthcoming). One scientist's style may be to think outside the box, and he might engage by performing wild new experiments or working on highly abstract theories. Another scientist's style may be to think inside the box, and she may engage by repeating well-known experiments (with minor refinements). And trying to force either scientist to change style or mode may be unwise. Indeed it may not even always be possible for such changes to occur.[20]

In closing this section, we should also consider how the puzzle solving function may relate to the critical one. (Consider, again, figure 6.) First, only theories which are positively evaluated (by those performing the evaluative function) will be used for puzzle solving purposes. It is these theories that will be applied, and which will determine what sort of data is normally considered to be worthy of collection. Second,

[19] An interesting objection to the picture shown in Figure 6, indeed, is "Why should each and every scientist not perform all of *those* functions?" This question was put to me when I presented this paper at the Future of Humanity Institute, Oxford.

[20] In fact, as I argue in Rowbottom Forthcoming A, appeal to something like stances is necessary for Kuhn irrespective of whether any concessions are made on the issue of criticism. In particular, appeal to stances can explain how different activities occur within a disciplinary matrix, i.e. account for the differences in puzzle-solving activity. For example, they can explain why one scientist endeavours to articulate a theory, whereas another seeks only to apply it in unproblematic circumstances, or why two different scientists look to different exemplars (and therefore puzzle solving strategies) to tackle one and the same puzzle.

however, the puzzle solvers' data and results may be useful to those who are performing attacking and defensive critical functions. (Attempts to puzzle solve may isolate unanticipated trouble spots, for example, just as Kuhn suggested.) Third, the results of the attacking and defending processes will be evaluated, and this will determine what sort of puzzle solving takes place next. So in short, there may a fruitful interchange between puzzle solvers and criticisers; and perhaps this is the genuine lifeblood of science.

7. Further Questions

The model proposed above raises a quite different set of questions from those explicitly tackled by Kuhn and Popper, and shifts the focus of the debate. How should the balance between functions be struck? That is to say, for any given group of scientists, how many should be fulfilling puzzle solving functions, rather than critical functions? And of those performing critical functions, how many should, say, be performing evaluative functions? These questions, and others like them, may not have 'hard and fast' answers that are contextually invariant. Instead, the proper distribution of activity may depend on the skill base available – e.g. perhaps despite their best efforts, some people cannot feign being dogmatic when they are not, in so far as they cannot really push themselves to defend a theory come what may – and also the state of science at the time. If T_4 were evaluated as suffering from severe defects but there was nothing else available to put in its place, for instance, then perhaps more imaginative effort would be required. Similarly, if T_5 remained untested and unchallenged, then perhaps more offensive and defensive interplay concerning T_5 would be merited. (So note also that the wisdom of occasional episodes resembling revolutions, but not quite so extreme and wide-ranging as Kuhn's model demands, may be accounted for.)

I should emphasise that I have not denied that there is a fact of the matter about what an individual scientist might best do (or best be directed to do) in a particular context of inquiry. Rather, I have suggested that determining what this is requires reference not only to the state of science *understood as a body of propositions (or as knowledge)* but also to *what other scientists are doing* and *the capacities of the individual scientist*. Consider a new professional scientist, going into his first postdoctoral research project; and let his capacity for good work be fixed by his interests, desires, and experience. Assume he could work just as well in group B as in group A. It might be preferable for him to join A because its line of inquiry is more promising than that of B, on current indicators, although it has fewer members.[21] So in short, I take there to be measures – even if they are rough measures, such as Popper's corroboration function[22] – of how theories (and/or research programmes, modelling procedures, etc.) are faring. And these, given the resources at our disposal, determine how we should respond.

A simple analogy may help. Imagine you, the chess player, are managing science. The pieces are the scientists under your command, and their capacities vary in accordance with their type (e.g. pawn or rook). The position on the board – nature is playing the opposing side – reflects the *status quo*. And now imagine you are told that, against the rules of normal chess, you are allowed to introduce a new pawn (which you can place

[21] Note that this doesn't depend on assuming that what everyone else (other than the individual) is doing is fixed. The best thing to do might be to have the newcomer replace a particular scientist in A, so that she could be moved to B, and so on. Think of the newcomer as an added resource.
[22] For an extensive discussion of this, see Rowbottom Forthcoming C.

on any unoccupied square).[23] (This is akin to the introduction of a new scientist; pieces working in combination on your side can be thought of as research groups, and so on.) Some moves will be better than others, given your aim of winning the game, and in some circumstances it will be clear that one available move is best.

So my own view is that considering social structure neither precludes employing insights from what might be called the 'logical' tradition in the philosophy of science – formal apparatus, such as confirmation or corroboration functions, for instance – nor requires acceptance of the view that studies in scientific method *always* require reference to the history of science. Social structure is relevant to questions of scientific method; but it is hardly as if when we discuss groups, rather than individuals, we suddenly find ourselves in territory where the 'logical' tradition has nothing to offer.[24]

The picture presented in this paper is complex, and the questions enumerated in this concluding section are daunting. It may prove to be the case that they are beyond our power to answer satisfactorily except in highly idealized contexts. Nevertheless, it appears that complexity is necessary if we are to truly get to grips with the question of how science should work. At the very least, the model here considered, e.g. as presented in figure 6, provides a basic framework with which to tackle practical questions when considering the research activity of a group (or groups). And even if that model is rejected, to consider functions *at the level of the group* is arguably to make an important conceptual breakthrough in understanding (and therefore shaping)

[23] Incidentally, there are variants of chess, such as Crazyhouse chess, where this sort of thing is possible.

[24] I say this in part because one commentator on the ideas in this paper, who works in the 'logical' tradition, reacted by declaring that "Kuhn was [just] a sociologist". Not only is this wrong – as Jones (1986) shows – but also remarkably myopic.

science. If there is one message to take away, it is that *ideal science may be realizable in more than one way*.

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