

## **The notion of psychological objectivity as a key to the heuristic process**

This paper is the work of an ethicist approaching the philosophy of science and wishing to question some of the unnecessary rational dichotomies in which the subject is often discussed. I shall argue a case for something like a non-relativist type of virtue ethics applied to our epistemological understanding of the progress of science, which I describe as “psychological objectivity”. I shall argue that this approach provides an obvious solution to some of the classic debate surrounding the heuristic problem. Because I am dealing with some of the basic assumptions found in the debate in this branch of philosophy, I have chosen to approach it through discussing what have become some of the classic positions in it: those of Feyerabend, Lakatos and Kuhn, rather than of more recent work which elaborates these classic positions.

### **i) The heuristic problem**

The heuristic problem, which is the central concern of the philosophy of science, could be briefly stated as the problem of reconciling our experience that new scientific discoveries are made with the philosophical problem of induction, which suggests that no empirical justification for a scientific theory will ever be categorical. In the broadest terms this problem applies not just to the credibility of scientific discovery at a cultural level (where we speak of a culture, or of the whole human race, making a scientific discovery) but to the discoveries of an individual generalising about her environment. I shall concentrate on two important aspects of this problem: the problem of induction itself and the theory-laden nature of observation.

The problem of induction is that of the relationship between generalisation and particularity. Even if we can take statements about particular events to be true because they have been verified by experience, no finite number of such particular statements can ever provide deductive grounds for a general statement unless there are true particular statements about all

the possible examples to which the general statement refers. Hence no general scientific theory which refers to a possibly infinite number of cases can possibly be proved. If we accept the assumption that statements about particular events can be proved true, however, a general scientific theory can be falsified merely by finding one instance where its predictions about a particular case are observed to be false<sup>1</sup>.

The theory-laden nature of observation, however, prevents us making the assumption that statements about particular events can be proven true. The assumption that they can be true is based on the idea of the mind as a *tabula rasa*, recording impressions with complete neutrality: but of course every observer begins with expectations which may influence what he chooses to observe, as well as physical and mental limitations as to what he is capable of observing given his instruments and senses. Lakatos points out that to give absolute status to particular observations distinct from general theories is to make a demarcation between general and particular which is in fact psychological and give it a status beyond the psychological<sup>2</sup>, since particular statements involve general assumptions and general statements involve particular assumptions.

The language in which both theories and observations must be described provides a further aspect to the problem. Even if we could be sure of actually experiencing absolutely objective observations, we could still not describe them in a way which was free of ambiguities of interpretation. The definitions of the terms describing empirical observation depend only on other definitions, not on any fact, creating a circularity which makes language independent even of experience, let alone any facts which may be the assumed objects of that experience. The very idea of a “fact” is an abstraction from an experience which could be divided up in an infinity of different ways.

These difficulties appear to lead us into a complete relativism whereby we cannot speak of discovery in anything other than conventional terms. Yet this appears to mean that the entire

history of science, from animism to nuclear physics, has not resulted in any objective progress. Applied at an individual level, too, this means that a person makes no objective progress in his understanding as he grows from embryo to emeritus professor. The difficulty is naturally that we must employ a value in order to determine what is “objective progress” and that this value must itself have objectivity in order to justify the claim.

I shall now proceed to examine two types of philosophical response to these problems, arguing that they are unsuccessful in resolving the problem because of their polarised over-reliance on rational dichotomy, before going on to argue that the most sophisticated approaches to the philosophy of science begin to go beyond it.

**ii) The failure of polarised approaches to the problem**

The two polarities of response to the heuristic problem are, firstly, to insist on the absolute nature of scientific progress and try to find a universal formula which will provide a criterion for progress in science; the opposed response is to give up all belief in scientific objectivity, with the apparent corollary that science has no value beyond the subjective. The first position is often described as (scientific) rationalism, the second as relativism.

Types of rationalism include the Kantian attempt to establish Newtonian physics and Euclidian geometry as synthetic *a priori* principles, the empiricist attempt to give certainty to induction through sense data theories, the dogmatic falsificationism I have already mentioned which tried to solve the problem of induction whilst ignoring the theory-laden nature of observation, and the probabilist retrenchment which tried to establish induction as establishing at least a probability of truth. All of these theories have been widely criticised<sup>3</sup>, and I do not intend to give them a detailed treatment here. Kantianism has been overtaken by relativity and quantum theory in physics and by non-Euclidian geometry, whilst sense-data theories tend to merely ignore aspects of the theory-dependence of observation.

Of the rationalist theories probabilism is perhaps the most successful. Its success, though, depends on the conception of probability that is applied: it can be understood in a logical sense as partial entailment<sup>4</sup>, as a reflection of statistical frequency (frequency theory) or as an entirely subjective estimation of likelihood based on experience (subjectivism). Popper successfully attacked the partial entailment theory *as a philosophy of scientific discovery* (as opposed to a purely abstract logic of relationships between putative evidence and putative theory) by pointing out that a finite number of examples used to support a generalisation referring to an infinite number of examples would always lead to a probability of zero<sup>5</sup>. The other two conceptions of probability lead us immediately out of the absolute realm and into the realm of belief formation: subjectivism does this explicitly whilst frequency theory collapses into subjectivism due to the need for observation in the compiling of statistics and interpretation in their application. The probability of a heavy smoker getting lung cancer, for example, thus appears to be a measure of the strength of *belief* of a person or group of persons as to how likely that event is.

The acknowledgement of this on the part of a probabilist should immediately lead us into a psychological understanding of scientific objectivity rather than an absolute one. If, like the other scientific rationalists, however, she clung to the idea of an absolute justification for science, either as an explicit belief or as an implicit belief given her behaviour, despite strong philosophical arguments to the contrary, there would be good grounds for ascribing this belief more strongly to a psychological cause. Scientific rationalism could play the psychological role both of providing a sense of security in the unassailability of science, and of providing a justification for scientific practice in which a theory is over-narrowly adhered to on the grounds that it is proven.

On the other polarity is the relativist view, as expressed for example by Feyerabend<sup>6</sup>.

Feyerabend maintains that there are no rational grounds on which to choose one theory over

another, or to choose one type of method over another, the choice being ultimately a subjective one. The results of different methods of investigation based on different theories are, he claims, incommensurable. Linked to this is an ideology of individual freedom, using arguments inherited from J.S. Mill to argue that individuals should not be constrained by the advocacy of one particular method by those with political power<sup>7</sup>. If the ultimate appeal here is to values based on the rights of individuals, however, it is not clear why we should hold those values, or that an anarchy of method is pragmatically the best way of fulfilling them. In rejecting a scientific dogmatism Feyerabend apparently adopts a moral one instead.

This relativist approach illustrates some further psychological features. On viewing the apparently infinite quantity of possible evidence, the relativist is not able to make the decision to form a belief on the basis of a given weight of experience. The relativist, like the rationalist, wants scientific laws to be unassailable, and, on realising they are not, concludes that there are no rational grounds for accepting scientific theories at all. Any acceptance of such beliefs will undermine the importance of the direct sense-experience with its accompanying conceptual openness. However, the relativist can only defend this suspension of belief by adopting beliefs of a sceptical type. In doing this he often adopts a theoretical dogmatism of the type he was initially trying to avoid, reinforced by the conviction that he cannot be being dogmatic because he is attacking dogmatic positions. In Feyerabend's case this dogmatism has also meant the adoption of an individualistic ethic which lends an edge of moral fanaticism to his scientific relativism. The relativist emphasis on the desire-component and the neglect of the experience-component in his explanation of the formation of beliefs indeed has no rational justification, as the relativist would agree, but he might not agree, as I would suggest, that the compulsion to assert this without offering convincing positive alternatives is best explained psychologically as a reflection of the strength of his own desire-component.

Although relativism has a place as an explicit position in the philosophy of science, as with scientific rationalism it is also an implicit position which could influence the way in which

both science, and also discovery on an individual level, is practised. At an extreme it could mean the refusal to explicitly acknowledge any theory even on a provisional level (though this probably means that there are implicit theories instead). In a milder version it could mean an inappropriate delay to amass evidence before committing oneself to a theory suggested by that evidence. This kind of tendency can be seen, for example, in the political use of the idea of thoroughness in scientific research as a delaying tactic, for example in avoiding confronting environmental issues which are politically uncomfortable.

In my survey of rationalist and relativist positions it will be noted that I have only so far dealt with the extremes, putting off consideration of more moderate positions which have been called 'rationalist', like the sophisticated methodological falsificationism of Popper and Lakatos, and other moderate positions which have been called 'relativist', such as Kuhn's. This is because I think there are aspects of these positions which do not reflect the psychological extremes of dualism as I have described them, but move towards a more balanced position which I will want to argue creates the possibility of psychological objectivity.

If it is as yet not completely clear why I make such a strong association between psychological and philosophical positions at the extremes of the philosophy of science, this is because the *modus operandi* of these positions is to make a strong disjunction between philosophy and psychology, the rationalist by denying the psychological influences on belief, the relativist by affirming them but nevertheless claiming that the grounds of scientific belief must be purely philosophical. When we come to consider the views which come closer to acknowledging psychological objectivity, however, this disjunction becomes much weaker and we see a sane acknowledgement both of the philosophical need to affirm beliefs and the psychological influence over them which makes this association more self-evident.

### iii) A psychological explanation without relativism

I shall first consider the position of Lakatos<sup>8</sup>, who has given the most sophisticated expression to the Popperian position. I have already given Lakatos's argument for rejecting the dogmatic form of falsificationism. Instead he advocates a form of falsificationism which admits the conventionality of any judgement that a theory should be dropped on the grounds of evidence against it: it is a pragmatic judgement rather than a question of final "proof" or "disproof". He also believes that any criteria as to the grounds on which theories should be accepted or rejected should take into account the real complexity of the heuristic process as it has occurred historically. Through the examination of historical cases he finds that real scientific discovery does not tend to follow the over-simplified model put forward by dogmatic falsificationists of falsifiable theory which is falsified and then abandoned. In reality most scientific theories consist of 'research programmes' which have a core or 'negative heuristic' which is not itself falsifiable, which together with auxiliary hypotheses allows the derivation of further propositions which Lakatos calls the 'positive heuristic', consisting in falsifiable predictions. The falsification of predictions leads only to modification of the positive heuristic, not to abandonment of the negative heuristic, since the falsification may only indicate faults in the auxiliary hypotheses (to which they are usually attributed).

Lakatos's recognition of the conventional and non-absolute nature of theoretical judgements does not make him a relativist, for he still believes that there are rational criteria which can be applied to such judgements. Apparent falsification of a theory alone is insufficient to provide a rational reason to abandon it, given that one of the auxiliary hypotheses may be responsible: additionally, Lakatos requires that there should be an alternative theory available which is not subject to the same falsification, explains apparent confirmations of the first theory, and predicts new facts which would otherwise appear improbable. Lakatos thus has a place for confirmations as well as for falsifications: but confirmations work mainly only to boost confidence in an established theory whilst falsifications are still pivotal in leading at least to a

reconsideration of a theory, perhaps to its modification. Usually falsifications result only in modifications to the positive heuristic of a theory, and only occasionally is there justification for the kind of scientific revolution created by the abandonment of an entire research programme with its negative heuristic, since while the grounds for modifying the positive heuristic are relatively clear-cut, those for abandoning a negative heuristic are not. Lakatos says that research programmes become “progressive” or “degenerating”, since rather than being clearly right or wrong they are merely more or less fruitful as a basis for discovery.

A scientist operating according to Lakatos’s philosophy thus has to cultivate a delicacy of judgement. He cannot take evidence to speak for itself but must reflect carefully on its relationship to the presuppositions which brought that evidence to his attention. He would be unwise to abandon a theory too rapidly on the grounds of an apparent falsification when that falsification may be due to conditions which he does not yet understand, yet on the other hand his time is not well spent tied to an unproductive research programme. At the same time he does not know for sure whether a currently unproductive research programme will not suddenly produce a breakthrough and lead to many new discoveries.

Lakatos has been criticised for claiming not to be a relativist and yet offering no clear-cut criteria to enable rational choice between research programmes other than with hindsight<sup>9</sup>. For me this is a mark of the acuteness of his understanding of the delicacy of judgement required in treading a middle way between absolutism and relativism in the heuristic process. Lakatos’s fault lies more in his continued insistence that his criteria are entirely rational rather than psychological. Clearly, however, a scientist cannot make a sensitive and balanced judgement about the acceptance or rejection of a theory in isolation from her psychological dispositions: in order to make consistently sensitive and balanced decisions she needs to be sensitive and balanced. But no account of that sensitivity and balance purely in terms of the nature of its results in the shape of scientific reasoning will be able to provide a complete explanation, since the consistent factors of objectivity consist in the psychological nature of the judgement

applied rather than any property of the objects of the reasoning or of the reasoning itself.

Lakatos's philosophy of science does try to provide a description of the sort of scientific behaviour which will lead to objective progress, even though it does not attempt an absolute description of an objective science. The conclusion, then, seems obvious – the objectivity primarily lies not in the science, but in the scientists.

Lakatos effectively suggests that for maximum objectivity we should neither be too attached to theories, failing to abandon them when they cease to be fruitful, nor should we be too hasty in abandoning them at the first apparent falsification. The middle ground between these two psychological tendencies is certainly not easily defined because it is incremental and situationally-relative: yet it is in general psychological terms that we get closest to being able to describe what Lakatos attempts to describe in rational terms.

Another major figure in the modern philosophy of science whose work appears to some extent to support my hypothesis on the nature of dualistic belief processes is Kuhn. Like Lakatos, Kuhn attempts to make his work adequate to the historical reality of how scientific discoveries have been made<sup>10</sup>. Unlike Lakatos, however, he admits that the choice between theories in science cannot be based purely on rational criteria, which has led to him being accused of relativism<sup>11</sup>, though he himself denies this<sup>12</sup>. Whether or not this accusation is fair in the terms in which it was made, I would maintain that his work also at least implicitly suggests a solution to the heuristic problem in terms of psychological objectivity.

Kuhn<sup>13</sup> gives an account of scientific paradigms and their evolution from a period of “pre-science” through a period where they become “normal science” to crisis and revolution. He does not think that there are ultimately any rational criteria with which either to define a paradigm or to choose one paradigm over another, but this does not mean that he lacks appreciation of the value of work within a given paradigm. On the contrary it is during periods of “normal science” that the most solid progress is made. During the process of scientific

revolution, scientists find good reasons to switch paradigms, but those good reasons are not sufficient to make the switch an entirely rational choice: there will also be a subjective and psychological element to the “conversion experience”.

Kuhn’s alleged relativism arises from the point that he does not think one paradigm is better than another at establishing an ontological reality. “There is”, he writes, “no theory-independent way to reconstruct phrases like ‘really there’; the notion of a match between the ontology of a theory and its ‘real’ counterpart in nature now seems to me illusive in principle<sup>14</sup>”. This amounts merely to a rejection of what I have described as an absolutist position, where theory is said to have a one-to-one correlation with noumenal reality. Kuhn claims not to be a relativist because he is “a convinced believer in scientific progress<sup>15</sup>”, but this scientific progress is not to be measured by absolute criteria. Instead he believes that science has steadily improved through history in its “puzzle-solving ability”. His account of this ability could be read as lending support to the concept of psychological objectivity:

Taken as a group or in groups, practitioners of the developed sciences are, I have argued, fundamentally puzzle-solvers. Though the values that they deploy at times of theory-choice derive from other aspects of their work as well, the demonstrated ability to set up and to solve puzzles presented by nature is, in case of value conflict, the dominant criterion for most members of a scientific group. Like any other value, puzzle-solving ability proves equivocal in application. Two men who share it may nevertheless differ in the judgements they draw from its use. But the behaviour of a community which makes it pre-eminent will be very different from that of one which does not.<sup>16</sup>

Kuhn’s emphasis here is on the social context in which this ability most clearly manifests itself, presumably because the variability of its manifestation is more likely to settle into a clearer pattern when many puzzle-solvers are considered together. “Puzzle-solving” though, remains an individual ability involving, above all, skill in relating theories consistently to evidence. Though the term may at first appear to refer to the activity of a merely technical intelligence (due to the connotations of the word “puzzle”, perhaps), Kuhn has already made it clear that this does not involve merely following rules. It therefore suggests skilled judgement in the scientific field of the kind that Lakatos has already cast light on. Such skilled judgement

might be well employed for a whole lifetime within the scope of the “normal science” of a given paradigm, or may need to be employed, as Kuhn mentions, to decide between paradigms. The fact that other psychological factors also come into play in theory choice indicates only that this “puzzle-solving ability” is imperfect.

Kuhn’s concept of “puzzle-solving ability” does not map perfectly onto my concept of psychological objectivity, but the latter could be seen as a more flexible version of the former which can be applied more easily in a broader philosophical context beyond that of science. What Kuhn intended by “puzzle-solving ability” may also involve non-transferable abilities quite specific to a scientific context. What is clear, however, is that it involves a capacity for commitment to a theory when it is productive during periods of “normal science”, together with a critical capacity to be exercised at times of revolution. It is this flexible aspect of the ability that I would wish to isolate as descriptive of psychological objectivity.

Both of these great philosophers of science, then, drawing on historical evidence (none of which I have had space to consider here) have reached broadly similar conclusions about the nature of scientific progress. These conclusions, suitably interpreted, appear to support the view that the objectivity of the heuristic process is psychological in nature. They do so by supporting both the negative hypothesis that it is a psychological tendency of over-attachment either to theories or to their refutation which *impedes* objective progress, and (perhaps less strongly) the associated positive hypothesis that it is balanced psychological states on the part of scientists which enable objective progress.

#### **iv. Conclusion**

The conclusion that heuristic progress is based on psychological objectivity goes against the grain of most modern discourse on the subject, which tends to adopt the Humean assumption of the absolute separation of facts and values, together with the assumption that psychological

qualities are necessarily subjective. The very idea of psychological objectivity requires a reconsideration of our uses of the terms “subjective” and “objective” so as to separate them from necessary association with a simple subject of consciousness or a “real world” wholly distinct from that consciousness.

The full exploration of the implications of this approach, not just in philosophy of science but also in ethics and other areas of philosophy, is a much bigger task which I have undertaken elsewhere<sup>17</sup>. However, here I have attempted only the more limited task of showing that the more subtle and effective a response to the heuristic problem in rational terms, the closer it actually comes to offering an account in terms of psychological objectivity.

This is not an individualistic conclusion at odds with a sociological approach to science, as the psychological states of scientists can be considered collectively or even culturally as well as individually. In order to consider the degree of psychological objectivity of a scientist one must obviously take that individual in context. The most psychologically balanced individual will not necessarily succeed in making heuristic progress if he is in conflict with a surrounding culture where the psychological norms are unbalanced (either by accepting a dogmatic understanding of the world, or by excessive scepticism).

This can, however, offer a common framework of explanation for the question of objectivity in science and in ethics, offering the potential to resolve the conflicts frequently found between these two standpoints in the modern world. If objective progress in science is understood to depend, not solely on a question of verification or falsification or its absence, but rather on the degree of objectivity of the scientist, her claims can be weighed fairly against the counter-claims that may be made by those

appealing to the standards of, say, ethics, religion or aesthetics, by considering the objectivity of those offering such claims in a similar way. Much work remains to be done on the details of how such comparisons could be made.

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<sup>1</sup> This is the position presented by Lakatos as “dogmatic falsificationism”, the dogmatism arising from the assumption that there are sufficient empirical grounds to categorically prove an observation statement. See Lakatos (1974).

<sup>2</sup> Ibid. p. 97-8

<sup>3</sup> e.g. in Lakatos (1974) and Polanyi (1962)

<sup>4</sup> See Carnap (1962)

<sup>5</sup> Popper (1959)

<sup>6</sup> Feyerabend (1975)

<sup>7</sup> “The reasons were explained by Mill in his immortal essay *On Liberty*. It is not possible to improve upon his arguments” Feyerabend (1978) p.86. Despite his sophistication in scientific questions, Feyerabend adopts this naïve and dogmatic approach to ethics throughout his (1974) and (1978).

<sup>8</sup> All the views subsequently attributed to Lakatos are derived from Lakatos (1974)

<sup>9</sup> e.g. by Feyerabend (1974)

<sup>10</sup> Kuhn’s work in this area actually predates Lakatos’s and probably influenced it, as Chalmers (1978 p.89) suggests.

<sup>11</sup> E.g. by Chalmers (1982) p.107-9.

<sup>12</sup> Kuhn (1996) p. 205-7

<sup>13</sup> Kuhn (1996 –3<sup>rd</sup> Edn)

<sup>14</sup> Ibid. p.206

<sup>15</sup> Ditto

<sup>16</sup> Ibid. p.205

<sup>17</sup> See my unpublished Ph.D. thesis “A Buddhist theory of Moral Objectivity”, Lancaster University 2001. This paper is an adapted extract from chapter 2 of this thesis.

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