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A SELECTIVE SURVEY OF THEORIES OF SCIENTIFIC METHOD.

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1. WHAT IS THIS THING CALLED SCIENTIFIC METHOD?

For some, the whole idea of a theory of scientific method is yester-year's debate, the continuation of which can be summed up as yet more of the proverbial deceased equine castigation. We beg to differ. There are reasons for the negative view, however, some of which will be canvassed in this selective survey of the territory the debate about theories of method has traversed in the second half of the twentieth century. The territory is very wide-ranging. It is hard to find a perspective from which one can get an overall view. If one focuses on one part of the philosophical debate about method then others go out of focus or do not come into view at all. What will be attempted here is a number of snapshots of the philosophical landscape which hopefully convey, if not the whole picture, something of the debate over method that has taken place.

 One focus concerns the debate surrounding the three figures whose names form the title of this book, Popper, Kuhn and Feyerabend — and to which one can add Lakatos. Both Popper and Lakatos were advocates of what one can call the 'grand' approach to theories of scientific method; grand in the sense that not only did they wish to propose some substantive universal and binding principles of scientific method but also to arrive at the ultimate goal of methodology — a demarcation criterion which draws a sharp line between science and non-science or pseudo-science. Such a goal is evident in Aristotle's attempt to characterise science as that which has apodictic certainty, or in Newtons' attempt in Rule VI of his *Rules of Reasoning in Philosophy* to employ rules of inductive inference as criteria of demarcation. Popper and Lakatos reject such proposed demarcation criteria but still insist that there is some demarcation criterion to be found. On certain interpretations of their work, Kuhn and Feyerabend have been taken to undermine the pretensions of such grand theories of method — along with the principles of method upon which such demarcation criterion relied. That such an understanding of Kuhn's and Feyerabend's position has won wide acceptance is one reason for the common view that discussions of theories of method, coupled to criteria of demarcation, are part of yester-year's debate. However such an interpretation of their work needs qualification, as will be seen §8 and §9. Overall, what will be maintained here is that science can be demarcated by the methodological principles it employs, notwithstanding other attempts at the grand project of demarcation which might be deemed to have failed.[[1]](#endnote-1)

 Whatever differences there are between Popper, Kuhn and Feyerabend, they are united by a common opposition to an inductivist and/or probabilistic approach to methodology. This suggests that one can shift the focus of the debate about method from matters to do with demarcation to matters to do with the nature of scientific inference. And one can refocus again so that the above-mentioned philosophers of science blur into the background while the sharp foreground is occupied by those who have developed inductivist, probabilistic and Bayesian accounts of a scientific method.[[2]](#endnote-2) Refocusing once more, one can bring into view the approaches that are currently being taken by the heirs to both schools of thought. Contemporary methodologists may be openly inductivist and/or probabilistic and/or Bayesian[[3]](#endnote-3); or they may explicitly develop rival theories of method; or they might remain silent on this rivalry and be content to investigate piecemeal particular principles of method. A glimpse of the later two positions can be found in the papers collected in this book.

 Refocussing yet again, all of the above sorts of philosophers of science can disappear into the background while the foreground is occupied by those who reject not only the demarcationist pretensions of methodology but the whole enterprise of scientific method itself. Some take their cue from a particular understanding of Kuhn or Feyerabend. Surprisingly others take their cue from a radical Bayesianism which says that there is nothing to scientific method except the accommodation of one's beliefs to the probability calculus and a rule of conditionalisation.[[4]](#endnote-4) But by far the majority are influenced by sociological and postmodern theories of science which declare the end of methodology as we have known it. There is a conflict between the view of many philosophers that there is something to be said on behalf of the rationality of science, which theories of method try to capture, and the view of many sociologists of science (and those influenced by them) that there is very little to be said for it and much against it. From the sociological standpoint, the very content of our science is (and has been) determined by personal, professional, social and cultural and other such factors or interests. Sociologists also claim that the sciences we adopt are nothing but the result of negotiation between contending parties.

 For many it is the sociological turn in science studies that has done most to turn debates about scientific method into yester-year's issues. Two positions are most commonly adopted. The weaker is that yester-year's debate has shown that there are principles of scientific method, but they are not universally binding and they do not lead to substantive demarcation criteria. The stronger is that yester-year's debate has shown that there are no substantive principles of method at all, and no demarcation to be drawn. Either 'anything goes', or the debate was misguided since it did not look to the social and cultural influences that determine the very content of scientific belief. Both positions will be explored more fully later. But the strong position can readily be shown to be far too strong and cannot strictly be the case, given what the word 'method' might mean.

 The English word derives from the Ancient Greek 'methodos' () 'the pursuit of knowledge' or 'a way of inquiry' (literally 'way of pursuit'). The OED tells us that a method is a way of doing something in accordance with a plan, or a special procedure; or it is a regular systematic way of either treating a person or attaining a goal. It is also a systematic or orderly arrangement of topics, discourses or ideas. Thus one can speak of a method for gutting fish, baking a cake, wiring a fuse box, or surveying a given piece of landscape. There are also methods for teaching languages, e.g., the immersion method; there are methods for teaching the violin to the very young, e.g., the Suzuki method; there are methods for learning, and playing, tennis or golf; and so on. In general there are the methods whereby one can best teach (or learn) a given subject, present an effective case in a law court, write up a report, and so on.

 It would be very surprising if one did not find methods of the above sorts in the sciences. Thus for astronomers who use optical telescopes there are methods for making observations of the positions of heavenly bodies and the times at which they are observed[[5]](#endnote-5), and methods for recording the information. For biologists there are methods for staining tissue for viewing under a microscope, or preparing cellular matter for DNA analysis. For chemists there are methods for preparing solutions with a specific pH value. For sociologists there are methods for preparing questionnaires and there are statistical methods for analysing their results. Mathematicians give us methods for solving various kinds of differential equations, or for finding the curve which best fits some given data. There are also methods for presenting data, methods for presenting the outcome of an experiment and methods for setting out papers for publication. The sciences are full of special methods, techniques and procedures for conducting experiments, analysing data, preparing results, etc. Not to follow these methods, techniques or procedures is, in some sense, to be unscientific — or at the very least to be sloppy and unsystematic in a way which undermines the goal of the activity in which one is engaging.

 Most of the above examples fall either into the category of what one might call 'the material practice of science' (e.g., making a solution of a given pH value, or conducting a particular experiment), or into the category of mathematical methods (techniques for finding solutions to differential equations, or methods such as the method of least squares in curve fitting, etc). But philosophers and scientists are also interested in a range of methods which transcend the material practices of any particular science, or transcend the mathematical methods used to solve particular problems in each of the sciences. More generally they might want to know about the methods for making inferences in science, say from given data to either the truth or falsity, or the probability or improbability, of certain hypotheses. It is here that the topic of scientific inference, in part falling within the domain of statistical inference, comes into contact with what philosophers regard as part of the domain of scientific method. In turn such an idea of a scientific method becomes part of the province of epistemology, viz., there are methods of justifying, accepting and rejecting beliefs not only in science but elsewhere. It is method as a province of, or a close relative to, epistemology, understood as the 'knowledge-getting enterprise', that is usually under discussion by philosophers and not so much the material practices of science or mathematical methods which are also abundant in science.

 Philosophers distinguish at least two broad goals, aims or ends[[6]](#endnote-6) of the scientific enterprise: the non-cognitive and the cognitive. Non-cognitive goals are various. They might concern the personal goals any scientist might have in engaging in their science, from getting a good salary to job satisfaction. Or they might concern the professional goals scientists have, from becoming a member of the Royal Society, getting a Nobel prize or being able to attract large amounts of funding. Or the goals may be humanitarian, social or political, as in using science to improve the health of people, increase productivity, clean up pollution or enhance defence capability. Again political and social goals are involved in the funding of research into one kind of science rather than another (research related to women's health has often received a lower priority in research funding than many other medical research projects). In some cases there may well be methods for making such choices in science in order to arrive at the chosen personal, professional, humanitarian or political goals, or in satisfying if not maximising these goals. Methods for making such choices commonly fall within the province of decision-making under varying degrees of risk. There is a well established body of literature on such decision-making which has grown up in fields such as engineering, economics and philosophy and which can be applied in this area[[7]](#endnote-7).

 From the philosophers' point of view (but perhaps not that of the sociologist of science), such non-cognitive goals appear to be extrinsic to the scientific enterprise. Though they might be external engines driving the enterprise, they do not constitute its intrinsic features. Cognitive goals in science are of a different character and are commonly held to be intrinsic to the scientific enterprise. Such goals have already arisen in talk of the mathematical techniques to be employed in assessing rival hypotheses. Here the cognitive goal is to find the hypothesis best supported by the data, the notion of *best support* being one of the cognitive aims of science. Again they have arisen in talk of methods for *finding* or *discovering* hypotheses in the first place. Here an old distinction comes to the fore between the context of justification and the context of discovery. The crucial question here is: are there are methods for inventing or discovering hypotheses as well as for justifying hypotheses (whether the same or different).

 The arch methodologist Popper surprises his readers when he says: 'As a rule I begin my lectures on Scientific Method by telling my students that scientific method does not exist'. But it transpires that what he means by this is the following three claims:

(1) There is no method of discovering a scientific theory;

(2) There is no method for ascertaining the truth of a scientific hypothesis, i.e., no method of verification;

(3) There is no method for ascertaining whether a hypothesis is 'probable', or probably true (Popper 1983, pp. 5-6)

Popper's first point reiterates his long-held view about the context of discovery, viz., there can be no methods for discovering hypotheses or theories. Popper's position is highly contested. His opponents point to the existence of computer-based procedures for the scientific discovery of hypotheses to fit data, for example the BACON programmes which have even been used on Kepler's data to arrive at his Third Law of planetary motion (Langley et. al. 1987). There are also non-algorithmic procedures for finding curves which best fit given data by using methods such as that of the least mean squares. And there are methods (suggested in the papers by Norton and Forster in this book) of 'deducing hypotheses from phenomena' (first adumbrated by Newton), or making 'deductions' from phenomena along with other heuristic principles.

 Popper's second point takes us away from the context of discovery and back to the context of justification. It has wide acceptance, if by 'verification' is meant 'can be shown to be true by human powers'. It is readily granted that since theories are unrestrictedly general then we cannot show them to be true, even though they may be true. But could we not confirm, or probabilify, them? Popper's third point introduces a further substantive disagreement between his own anti-probabilistic stance in which only deductive reasoning is needed in the sciences[[8]](#endnote-8), and those who adopt either an inductivist or probabilistic or Bayesian view of how hypotheses are to be assessed. Despite his view that there is no method in any of these three senses Popper goes on to say that 'the so-called method of science consists in … criticism' (*op. cit*., p. 7), and then spells out what his critical method is — on which more later.

 The Popperian aim of criticisability of our theories, and the specification of canons of criticism, gives us one conception of what a theory of scientific method would look like. But criticisability is not the only cognitive aim of science, even for Popper. Philosophers look to a number of aims for science of which the most general are descriptive and explanatory aims. The descriptive goal of science is to find out the truths about the world. These truths might be restricted to those about observable phenomena only, in which case we have a refinement of this aim adopted by constructive empiricists such as van Fraassen, viz., to find theories which are empirically adequate in the sense that they fit all observable phenomena (van Fraassen 1980, Chapter 2). Realists have a broader aim; not only do they aim for truths about the observable but they also aim for truths about an unobservable realm of objects, properties and processes and for truths about laws governing these. The dispute between realists and non-realists (such as constructive empiricists) about the realisability of the realist aim has often turned on the viability of a methodological principle, that of inference to the best explanation.[[9]](#endnote-9) Like many others, Popper also says that 'the aim of science is to find *satisfactory explanations*' (Popper 1972, p. 191). Whatever account of explanation is adopted, the goal of increasing our understanding of how the world works, or of increasing our knowledge of how and why the world is the way it is, is a further central cognitive aim of science. The broad cognitive aims of adequate description and explanation give substance to the philosophical conception of scientific method, supplemented by the means whereby these aims can be reliably realised.

2. THE IDEA OF PRINCIPLES OF METHOD

One way of focusing on much of the broad terrain covered by the last fifty years of debate concerning scientific method is by using the following schema. The scientific enterprise can be analysed into three different levels, as illustrated in the accompanying table.

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| *Level 3*Meta-methodologies | *Nihilist*: Sociologists of Science, Postmodernists, etc*A Priori*: Transcendentalism, Logicism, etc*Conventionalist*: early Popper.*Empiricist*: Historical Intuitionism (later Popper and Lakatos); Reflective Equilibrium; Varieties of Naturalism; etc *Pragmatist*: Rescher |
| *Level 2*ScientificMethodologies(SMs) | Aristotle's Organon, . . ., Bacon's Methods, Descartes' *Regulae*, Newton's *Rules*, . . ., Popper's Critical Rationalism, Lakatos' Scientific Research Programmes, Kuhn's Weighted Values, Feyerabend's Methodological Pluralism, Bayesianism, Decision-Theoretic Methods, etc, etc, |
| *Level 1*Historical Sequence of Scientific Theories (of some domain) | *Dreams*: Homer, Bible, . . , Aristotle, . .., Freud, Jung, . . ., Crick, Hobson, . . . *Motion*: Aristotle, . . ., Kepler, Galileo, Descartes, Newton, . . ., Laplace, Lagrange, Hamilton, . . ., Einstein, . . . *Etc:* |

 The bottom level concerns the actual historical sequence of our choices of scientific theories concerning some domain of phenomena. Amongst all the possible theories that might have been given individual or communal assent by scientists over some period of time, the actual historical sequence is a path through the 'tree' of alternative possible histories of science. Thus consider the phenomena of dreaming. Our actual historical sequence of theories about the nature and causes of dreams begins with the works of Homer, the Bible and Plato. Aristotle seems to have been the first to have proposed a theory about the causes of dreams which did not appeal to spirit-like entities and which has affinities with modern scientific theories.[[10]](#endnote-10) Skipping the intervening centuries and focusing on our own, at the beginning of this century there are the psychoanalytic theories of Freud and Jung while in our own time Crick, Hobson and others have proposed theories of dreaming based on the neurophysiological functioning of the brain. If one considers a different domain of phenomena, such as motion, then there has been an equally long sequence of theories amongst the most prominent of which are the theories of Aristotle, Kepler, Galileo, Newton, Laplace, Lagrange and Einstein. Similarly for other domains of phenomena. The idea that there is an historical sequence of scientific theories is fairly uncontroversial; it yields the raw material on which the two higher levels are based.

 Amongst all the possible theory choices scientists might have made at any time, what determines the historical sequence of scientific theories that were actually chosen by the community at large? (This does not preclude two or more theories being chosen by members of a given community at any one time.) Or in other words, what has led to the historical sequence of changes in theory? The word 'change' is used deliberately since talk of growth, development or improvement in our choices involves appeal to methodology, and in particular values, to provide criteria for such growth, etc. So, what values, if any, do the changes exemplify? Let us agree that the historical sequence of theories at least displays, over time, the value of increase in instrumental success, where such success is either our increased ability to predict, and/or our improved ability to manipulate, the natural and social worlds to our own ends. (This allows that not all our attempts at prediction and manipulation have been successful.) Rephrasing the above questions we can ask: what explains our choice of a sequence of theories which yield such instrumental success (given that there are many pathways through all possible theories that would not yield such success)?

 Is the successful sequence of theories due to luck, or to the tacit incommunicable 'feel for the right thing' that some such as Polanyi (1958 and 1966) alleged is indicative of scientific genius? Methodologists suggest that it is neither of these but rather the use we have made of principles of scientific method in choosing theories which are successful. Sociologists of science not only reject appeal to luck or tacit 'feel'; they also reject the appeal to principles of method, offering instead a rival explanation. Our non-cognitive interests, or our social circumstance have largely produced the sequence of historical theories, not principles of method. Which of these last two rivals explains such success better?

 Our non-cognitive personal, professional and political interests are often such that we want theories with high instrumental success. But it is not very probable that merely having such interests would, on the whole, lead us to choose those very theories which, when we examine and apply them, turn out to be successful, thereby realising our interests. Personal, professional and political interests seem, on the face of it, to be quite randomly linked to whether the theories chosen on the basis of such interests are also those which are successful. Methodologists would, in contrast, argue that something else intervenes producing the sequence of theories that satisfy our desire for instrumental success. These are principles of method, some of which do link some of a theory's epistemic and cognitive features to the theory being successful in the above sense. Our use of principles of method provides a more plausible explanation of why we have chosen a highly successful sequence of theories rather than some other possible sequence which might have been less successful. In sum, employing non-cognitive interests in choosing theories makes it improbable that the theories are successful; in contrast employing principles of method in choosing theories makes it more probable that the theories are successful. This is tantamount to saying that the methods we use explain success better than our non-cognitive interests. Using the principle of Inference to the Best Explanation, we can say that there is something true, right or valid about those principles of method we use which explain, much better than our non-cognitive interests do, why we have chosen a successful historical sequence of scientific theories.

 It remains, of course, to show *how* our use of principles of method has produced our historical sequence of successful theories, what the link is between the methods and such success, and what it is about the principles that is right, true or valid (see §12). But the argument above helps undermine the claims of sociologists of science that talk of principles of method can be relegated to yester-year's debates — a claim which will be discussed more fully latter. Thus principles of method have an important role in explaining much of the historical growth of science. But what are these principles like? And why is their use so efficacious in producing successful theories? The first question will be addressed now while the latter is addressed in different ways in subsequent sections.

 Just as there is an historical sequence of scientific theories at the bottom level, so at the next level up, the level of scientific method (SM), there has been proposed a number of theories of SM which also form an historical sequence. (The various SMs are at a higher level only in the sense that principles of method at the second level have as their object of application scientific theories at the first level.) Though the historical sequence of SMs begins with Plato's methodological remarks in his Socratic dialogues, the first fully set out theory of method for science can be found in Aristotle's *Organon.*  Methodological precepts can be found in the heirs to the Aristotelian tradition up until the late Renaissance. But with the advent of the sixteenth and seventeen century 'scientific revolution' methodological matters come to the fore in the work of Bacon, Galileo, Descartes and Newton, just to mention a few.[[11]](#endnote-11)

 In the second half of the twentieth century there has been a proliferation of theories of method. In a 1986 publication, a team of eight researchers did us the service of working through some of the writings on scientific method by five philosophers of science, Popper, Lakatos, Kuhn, Feyerabend and the early Laudan (Laudan *et. al.* 1986). They collected together well over 250 theses about what allegedly does, or what ought to, happen when one theory (paradigm, research programme or whatever) is followed by another. The goal of the team was to compare the 'positivist models' of scientific change with the models of the post-positivists, also dubbed the 'historical school' (founders of which were Hanson, Feyerabend, Toulmin and Kuhn), by testing some of their theses against the historical sequence of scientific changes. Such testing is an important part of what Laudan has called 'normative naturalism', a metatheory about SMs discussed in §11.

 One reason for the need to test was the degree to which the methodological claims uncovered by the research team rivalled one another. Of the 39 sets of theses the team identify, consider the 20th which lists six claims which according to the methodologist named in brackets, tell us how scientists have behaved with respect to particular theories and their ability to solve problems:

Scientists prefer a theory which

(20.1) can solve some of the empirical difficulties confronting its rivals (Laudan, Kuhn);

(20.2) can turn apparent counter-examples into solved problems (Laudan);

(20.3) can solve problems it was not invented to solve (Laudan, Lakatos);

(20.4) can solve problems not solved by its predecessors (Kuhn, Lakatos, Laudan);

(20.5) can solve all the problems solved by its predecessors plus some new problems (Lakatos);

(20.6) can solve the largest number of important empirical problems while generating the fewest important anomalies and conceptual difficulties (Laudan)

(Laudan *et. al.* pp. 171-2; references to work cited by the bracketed methodologists has been omitted))

 While having some features in common some do differ in important ways. So which are correct, if any, or more probable given the choices scientists have actually made? Moreover, the above methodological remarks are expressed as factual claims about how scientists are alleged to behave with respect to the problem-solving abilities of a pair of rival theories; as such these claims are of a historical or sociological character and are open to test against the record of the actual behaviour of scientists in some particular context[[12]](#endnote-12) (a task carried out by normative naturalism in examining many of the 250 theses uncovered by the research team). Though they are not expressed this way, the above can also be interpreted as imperatives about what scientists ought to do when choosing between a pair of theories with respect to their problem-solving abilities. Thus (20.5) can be re-expressed: scientists *ought* to prefer that theory which can solve all the problems solved by its predecessors plus some new problems. This brings us to the issue of what are the methodological principles that comprise SMs, and how they are to be formulated.

3.RULES, VALUES AND METHODOLOGIES

In what follows SMs will have the following characteristics. Each SM has associated with it the pair <R, V> where R (={r1, r2, …, rm}) is a set of methodological rules and V (= {v1, v2, …, vn}) a set of epistemic values (or goals, or aims — these will be treated as equivalent).[[13]](#endnote-13) A principle of a methodology will then be a hypothetical imperative of the form: if one wishes to realise value vi then one ought to follow rule rj; SMs can then be characterised as a set of principles {P1, … Pr}.[[14]](#endnote-14)

 However the theses (20.1) to (20.6) above are all declarative rather than imperative; and they contain no explicit reference to a value. On the second point, there are good grounds to suppose that each thesis has a suppressed reference to some unspecified value. There are various reasons why they might not contain an explicit reference to a value, or values. Perhaps the value is not explicit because it is a general presupposition, or the thesis has been expressed elliptically. However values (ends, aims) must be understood in the context; otherwise the declarative sentence merely contains a rule 'do x' without telling us what the purpose of doing x is. On the first point, we can regard the declarative claim (with reference to a value made explicit) as having the following form: following rj will realise value vi. What is the connection between the methodological principle, which is a hypothetical imperative, 'you ought to follow rj if you want to realise value vi' and the declarative which is an empirical claim 'following rj will realise value vi'? This issue is important for normative naturalism and is discussed in §11.

 Examples of values adopted by SMs include: truth, or increased verisimilitude; empirical adequacy; generality; testability; falsifiability; coherence; explanatory breadth; high probability on available evidence; the capacity to withstand severe tests; openness to revisability; and so on. Could an SM lack any values? As just indicated, though SMs may lack explicitly stated epistemic values, they are generally presupposed. Principles of method which lack any value whatever would become mere rules with no point to their application.

 Rules of method tell us what we ought to do to realise some value. Examples of the methodological rules are: avoid *ad hoc* modifications to theories; prefer theories which make surprising novel predictions over those which predict what we already know; prefer double-blind over single or zero-blind experiments; accept a new theory only if it explains all the successes of its predecessors; reject unfalsifiable theories; for the same kind of effect postulate, as far as possible, the same kind of cause; and so on.

 Principles of method are then hypothetical imperatives concerning the link between rules and values. They are instrumental in character telling us about the means we ought to adopt in order to realise some end. How reliable are they? This is a matter for further investigation. In our actual world they could be 100% reliable, or less than 100%. Thus in their declarative form they could be viewed as akin to statistical generalisations with varying degrees of reliability such as: following r will realise v n% of the time (where 50 < n ≤ 100). The principles might also be statistical comparative in form: following r is more likely to realise value v than following some rival r\*.

 Are methodological principles *a priori* or empirical in character? This is a large question to be addressed in the next and subsequent sections since in the history of methodology they have been understood in both ways. Are they necessary, or contingent? That is, for any enquirer in any possible world are some principles always available since they hold in all possible worlds? Or are the principles contingent in that they hold for only some worlds and that enquirers in sufficiently different possible worlds will have to adopt different principles of method in order to make discoveries about their world? Deductive rules, which aim at the preservation of the truth of the conclusion providing the premises are true, will hold for all enquirers in all possible worlds. But clearly not all principles of method are necessary in this sense. This points to another sense in which principles of method can be said to be reliable. They may be reliable not only because they hold 100% in this world but because they also hold 100% (or nearly so) in a range of possible worlds other than our actual world. Deductive rules with true premises are clearly reliable with respect to their conclusions in both senses.

 The sciences propose laws, one feature of laws being their counterfactual robustness, that is, the laws are not merely generalisations about this world because they tell us what will happen in a sub-class of possible worlds varying in distance from our actual world, viz., the physically (or naturally) possible worlds. If principles of method are akin to scientific laws then they would be contingent and hold only in some sub-class of possible worlds; but they would also exhibit a similar counterfactual robustness. For all enquirers equipped with some principles of method in each of the sub-class of possible worlds, an investigation into their world would be widely reliable; the principles hold for that sub-class of possible worlds. But suppose the principles of method we adopt are not very counterfactually robust in that they apply only in our world, or perhaps only in a small range of worlds close to this one; outside this world, or the small range of possible worlds, they are not reliable for any enquirer. It would then be a matter of epistemic luck that our principles of method do yield theories which do exhibit some success. Our principles would be highly contingent generalisations in that in only slightly different circumstances they would be quite unreliable for use in inquiry.

 The matter of reliability becomes important because we are concerned with reliability across all those worlds which are possible relative to the information enquirers have, including all the information provided by our scientific theories. Up to a point an enquirer can ignore consideration of those worlds which are inconsistent with their best scientific information, or even those which are highly improbable given their best scientific information. But this still leaves a range of worlds which are consistent, or are probable, with respect to that information. Any enquirer would want principles which are reliable in all these worlds. So principles of method can not be just a matter of contingent luck; they must also exhibit some degree of counterfactual robustness and apply reliably in a sufficiently broad range of possible worlds.

 How are rules and values to be distinguished? In 'Objectivity, Value Judgement and Theory Choice' (Kuhn 1977), Kuhn canvasses the idea that there might be rules governing the choice between theories on grounds of accuracy, consistency, scope, simplicity and fruitfulness; but then he abandons the idea that these can be expressed as rules, preferring to understand each of these as values (Kuhn 1977, p. 331). In surveying the literature it is common to find that what one writer regards as a value another treats as a rule, and conversely. Thus while Kuhn ultimately treats simplicity as a value Lycan treats it as a rule saying: 'Other things being equal, prefer T1 to T2 if T1 is simpler than T2' (Lycan 1988, p. 130). Perhaps we can admit a great deal of interchange between what counts as a rule and what counts as a value from one SM to another. Values can get expressed as rules; and rules can be expressed as values. What we must not admit within the same SM are redundant values and rules which are trivially linked with one another, e.g., within the same SM a Kuhnian value of simplicity associated with a Lycan-type rule of simplicity.

 Popper's account of his methodology yields an example of a redundant rule and value. In one place he tells us that falsifiability is one of his aims for science: 'I propose to adopt such rules as will ensure the testability of scientific statements; which is to say, their falsifiability'. (Popper, 1959, p. 49). However later Popper proposes a supreme rule which serves as a norm for other rules of method: '… the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science against falsification' (*ibid*., p. 54) A rule which says 'do not protect against falsification' will, if successfully applied, trivially realise the goal of adopting only falsifiable statements. Popper's method might have other values which are not redundant with respect to this rule (e.g., high explanatory power or increased verisimilitude); or his method might retain the value of falsifiability but realise it with rules different from the supreme rule just cited. However a value of falsifiability linked to a rule which bids us to seek falsifiability is vacuous - or we might say that it involves redundancy. Thus for an SM with its associated rules and values <R, V>, if there is a principle P1 which contains a rule r1 which trivially realises value v1 but another principle P2 which non-trivially realises value v2 then we are to discount P1, but not P2, on the grounds of redundancy.

 In eliminating redundancy appeal is made to a higher third level rule, or meta-methodological rule, which bans such trivial rule-value linkages. The idea of a meta-methodological rule has yet to be introduced; but such a ban on redundancy is an example of a meta-rule, though admittedly not an exciting substantive meta-rule.

 Presumably there is also a meta-methodological rule of consistency which bids us to formulate second level SMs which comprise a consistent sets of principles, i.e., there cannot be pairs of principles one of which bids us to follow r while another bids us not to follow r in order to realise some value v. We will also want, for any SM, sets of values, and sets of rules, each of which taken as a whole are consistent.

 There has been much debate amongst methodologists concerning consistency as a second level rule (or value, or principle) which applies to scientific theories. Some rules would prohibit us from adopting theories which are internally inconsistent while other would prohibit us from adopting theories which are inconsistent with other prevailing theories. Concerning the latter, Feyerabend, in one of his methodological moods, has advocated what he calls 'methodological pluralism' which positively invites the proliferation of theories which are inconsistent with prevailing theories as a condition for the empirical advance of science — this advance being a value allegedly realised by the rule (Feyerabend, 1975, chapter 3). That is, for Feyerabend the value of empirical advance in science is (more likely to) to be advanced by adopting a rule which bids us to proliferate inconsistent theories rather than the more conservative rule which bids us to entertain only theories which are consistent with one another.

 Whether Feyerabend's principle, or its more conservative opposite, is to be adopted is not of immediate concern. What is of interest is whether there should be a *meta-rule* of consistency that is to be imposed on all our values, rules and principles of SMs at the lower level. If we grant that there should be,[[15]](#endnote-15) then it is still an open matter as to whether we adopt Feyerabend's lower level principle of SM that we proliferate inconsistent scientific theories, or we adopt the more conservative principle against inconsistent theories. Thus it would appear to be quite possible that there is a rule of consistency at the meta-level which applies to all values, rules and principles of SMs, but that there be no principle of consistency for any adequate SM concerning what scientific theories we are to adopt or countenance, if science is to advance. But this is only possible if higher level meta-rules are distinguished from the principles of SMs to which they apply. This amplifies the suggestion that there is a third level discipline of meta-methodology to be imposed on the rules, values and principles of SMs at a lower level.

 Thus meta-methodology is not a completely empty discipline; there appear to be at least two meta-methodological rules banning redundancy and inconsistency for principles of SMs. Whether there are more substantive principles remains to be seen. In fact it will be argued that philosophical theories which attempt to justify particular theories of scientific method are best viewed as providing meta-methodological justifications.[[16]](#endnote-16)

4.META-METHODOLOGY

We have now ascended to the quite rarefied atmosphere of meta-methodology which contains at least the meta-rule of non-redundancy and (perhaps) of consistency. As rarefied as it might be, it is not unfamiliar territory for those who have followed the many discussions about how induction is to be justified. In the schema above, rules of inductive inference, such as the simple rule of enumerative induction, find their place as second level principles of an inductivist SM. Hume's philosophical challenge that induction can not be justified is itself a meta-methodological claim about the status of second level inductive principles — and so are most of the arguments philosophers have advanced to rebut Hume's claim. For Humeans, even though we may still make inductive inferences (and perforce must do so), there is no rational basis for making such inferences. This meta-methodological claim is the core of Humean scepticism about induction. However we will call Hume's position 'nihilistic' because of the meta-methodological claim it makes, viz., there is no justification at the meta-level for the second level inductive inferences we in fact use and whose propositional contents are the claims of the sciences at the first level.

 Attempted philosophical rebuttals of Hume's view also have the character of meta-methodological arguments, only two of which will be mentioned here. One of Hume's main lines of attack is that the Principle of the Uniformity of Nature cannot be justified. In his attempt at a justification Kant tried to argue that this Principle, in the form of a principle of universal causation, could be shown to be a synthetic *a priori* truth. Our task here is not to judge the success or failure of Kant's argument (a good account of its failure can be found in Salmon 1967 pp. 27-40). Rather if we generalise Kant's procedure then we can take any principle of method and look for its justification, from a meta-methodological viewpoint, either on analytic or synthetic grounds, or (as is the case in the schema set out in the table) on *a priori* or *a posteriori* grounds.

 Again, attempts have been made to justify induction by investigating a hierarchy of inductive principles. Thus first level inductions about observed white swans or observed green emeralds are justified by appeal to second level inductive principles which are about the success of the arguments at the first level. These second level principles must themselves be inductively strong by their own system of inductive rules at the second level, and have as their conclusion that arguments at the first level will work well the next time they are used. An account of such an inductive justification (at a higher level) of inductive inferences (at a lower level) can be found in Skyrms (1975 pp. 30-41) and Black (1954 pp 191-208). Generalising this procedure we can view the connection between methodology and meta-methodology in the same light. There is a hierarchy of rules in which those at a higher level apply to, and provide a justification for, those at the next level down; in particular the same type of rule (e.g., an enumerative inductive rule) can appear at more than one level in the hierarchy.

 This suggests that third level meta-methodologies have the following features. They are at a different level only in the sense that meta-methodological principles apply to, and are about, principles of SMs at the second level. But they are also intended by some methodologists to bring the principles of SMs into relation with certain historical features of the sciences at the first level; so they apply to more than just principles of SMs. The possibility should also be left open that some methodological rule, or some value, can appear at both Level 2 and Level 3, e.g., the rule, or value, of consistency which applies both to scientific theories and to methodologies. Finally meta-methodologies can embody philosophical theories which in turn yield reasons for adopting some particular set of principles of scientific method. Meta-methodologies thus provide an answer to the 'legitimation problem' for theories of method, viz., the grounds on which SMs can be justified, or legitimated.

 If meta-methods are to provide justifications, then there is an apparent dilemma concerning justifications which they should avoid. If we are to adjudicate between truth and falsity in meta-methodology then we must be able to justify any claims we make. Consider some claim M in meta-methodology. This stands in need of justification. Either we appeal to M itself or to some other Principle M' to provide the justification. If we appeal to M the justification is circular. If we appeal to M' to justify M then we need a reason for accepting M'. Thus an infinite regress of justifications threatens. So the acceptance of any meta-methodology is threatened by either circularity or a regress of justifications. This raises difficulties for attempts to provide justifications in terms of methodological principles elevated to meta-methodology; it remains to be seen how each meta-methodological theory might deal with this difficulty.

 Are there any substantive theories or principles of meta-methodology, other than the two suggested principles of consistency and non-redundancy? The *nihilist* position is that there is no substantive theory or principle to be found at the meta-methodological level to adjudicate between, or to justify, principles of SM. We will need to investigate the extent to which Feyerabend, Kuhn, sociologists of science and postmodernist accounts of science adopt nihilism with respect to the project of meta-methodology. The other approaches to be considered adopt meta-methodologies of varying degrees of strength; either there is some philosophical position which gives us some purchase on principles of SM, or there are quite strong meta-methodological justifications that are available to support principles of SM. The first of these is that meta-methodology is an entirely *a priori* discipline. This might mean that we can give *a priori* justifications for the rules and values of an SM. Or less strongly it might mean that we can show *a priori* that the rules do realise their associated values. Allied to this position is the view that principles of SM have the status of conventions, the most prominent advocate of this view being the early Popper. Rivalling these *a priori* accounts is the view that meta-methodological matters are to be decided in an empirical manner like other matters in first level science. One such view is that of the later Popper and Lakatos. However there are now a number of varieties of naturalism concerning scientific method which are empirical in character, the clearest example being the position adopted by the later Laudan known as 'normative naturalism'. Finally there is an approach that can be broadly characterised as 'pragmatic'; it admits, contrary to nihilism, that there is a meta-methodological story to be told but it is different from either of the *a priori* or empirical approaches just mentioned. Aspects of the positions of Laudan, Rescher and Quine could be characterised under the heading of pragmatism. However not all methodologists we wish to consider fall neatly into one or other of these divisions, as will be seen. We begin by considering meta-methodologies which are *a priori*.

5. SOME *A PRIORI* APPROACHES TO META-METHODOLOGY

There is a long history of *a priori* attempts to provide an answer to the legitimation problem for SMs, three of which will be mentioned here. The first of these we can dub 'transcendentalism'. Transcendentalists attempt to mount some kind of Kantian argument from the bare possibility of science to the presuppositions of the scientific enterprise, amongst which hopefully will be found some principles of scientific method.[[17]](#endnote-17) However transcendental arguments have a low success rate, including arguments in this context to some principles of method. It is hard to see how such an argument could yield any substantive methodological principle.

 A second *a priori* approach might be dubbed 'logicism'. Logicism in the theory of scientific method is the view that just as there are principles of deductive reasoning which (on certain views of the nature of logic) can be given an *a priori* justification, so there are principles of non-deductive scientific inference that can be given an *a priori* justification. One *a priori*-ist approach might be through the Probability Calculus and some of its theorems such as Bayes' Theorem. Since the theorems are simple consequence of the axioms of the Probability Calculus, and if we assume that the axioms themselves can be given an *a priori* justification, then we already have the makings of an *a priori* justification.

 Consider the simplest version of Bayes' Theorem, viz., p(H, E) = p(E, H)xp(H)/p(E). Are the posterior and prior probabilities which make up the Theorem known *a priori* or *a posteriori*? If we could establish *a priori* the numerical values of 'p(E, H)', 'p(H)' and 'p(E)', then there would be a totally *a priori* argument to the value of p(H, E), and thus, it might seem, the beginnings of an *a priori* probabilistic methodology for science. Such a position arises naturally for those, such as Carnap, who think of expressions like 'p(H, E)' on a model of partial entailment, with total entailment being the special case of logical deduction. On this account deductive logic will give an account of when it is appropriate to claim that E logically implies H. Similarly an inductive logic will tell us when E *partially* logically entails H. That is, it ought to be possible to provide a theory of a confirmation function that will give us a quantitative value for 'r' for expressions such as 'p(H, E) = r'.

 It is now well known that the programme of finding *a priori* a numerical value for expressions such as 'p(H, E)', explored by Keynes, Carnap and others for any H and E, faces many difficulties; so this logicist justification of a probabilistic methodology for science has little hope of success. Most present-day advocates of probability in methodology are Bayesians (a position which Carnap also explored); they take a quite different subjectivist approach in which the key issues of justification turn on matters such as avoiding 'Dutch Books' and the like.

 However, even if the programme of finding *a priori* numerical values for the probability expressions were to succeed, it does not follow automatically that all of methodology would be *a priori*. This is particularly the case for Carnap who carefully distinguished between logical and methodological aspects of logic (Carnap 1962, §44 and §45). Thus in the case of deductive systems there are two 'fields' to consider. The first concerns the theorems which pertain to the system; the second concerns the methods which might be used to prove theorems under various conditions and for various purposes. The second field Carnap calls the 'methodology' of deductive logic. (Given the way in which we have already introduced the term 'methodology' in sections 2 and 3, it would be better for us to refer to Carnap's second field for deductive logic as the 'heuristics' of theorem-proving.) Similarly there is a methodology accompanying inductive logic. Thus in testing a given hypothesis h against new and old evidence Carnap says: 'methodology tells us which kinds of experiments will be useful for this purpose by yielding observational data e2, which if added to our previous knowledge e1, will be inductively highly relevant for our hypothesis h, that is, c(h, e1.e2) is either considerably higher or considerably lower than c(h, e1)' (Carnap, 1962, p.203). Thus an inductive logic will yield the values for Carnap's confirmation functions and tell us which is greater; in contrast methodology will tell us both what evidence we should look for and what we should do with the hypotheses with the higher and the lower numerical values for the two c-functions.

 Given the further matters he addresses, it is clear that for Carnap the task of methodology in inductive logic goes beyond the role it plays in deductive logic as a mere heuristics of theorem-proving, thus bringing his conception of methodology into closer contact with the concept we have introduced in previous sections. This becomes evident when one discovers that the methodology of inductive logic contains rules or principles of the following sort. There is a Requirement of Total Evidence which bids us to take into account the total evidence in calculating the degree of confirmation (*ibid*., p. 211). There is also a requirement concerning the variety of evidence to be used in testing a hypothesis (*ibid*., p. 230). Carnap's inductive logic also needs supplementing with methodological rules concerning the decisions that are to be made in the light of one's observations and available hypotheses. To this end Carnap investigates a number of methodological rules from The Rule of Maximum Probability (i.e., from a set of possible events, expect the most probable), to rules of Maximising Estimated Utility (*ibid*., §50 and §51)

 Such methodological rules, which Carnap must add to his account of inductive logic, go beyond methodology understood merely as the heuristics of theorem-proving as in deductive logic. In fact some methodology is required if Carnap's inductive logic is to have any application at all in either scientific or every-day contexts. Thus Carnap's account of the methodology of inductive logic can be bought into relation with the conception of methodology outlined above with its rules which realise values (such as truth). Given this, it is far from obvious that all the methodological principles Carnap discusses have an *a priori* justification; it is clear that some do not. Thus it would be wrong to regard the Carnapian programme as being committed to an entirely *a priori* account of scientific method, though elements of it will have an *a priori* justification (or would have if, for example, there were a satisfactory account of Carnapian confirmation functions).

 Finally, there is a further sense in which methodology might be *a priori*. Most methodologists assume that a rule r of an SM will, when correctly followed, realise some value v. However there is rarely a proof that this is so. One task of meta-methodology would be to show that rules are reliable realisers of certain goals. This suggests another weaker *a priori* approach in which an *a priori* proof might be given for some particular principle of scientific method. Suppose our aim is to get considerably increased support for our theory. How should we go about this? If we adopt a rule which says 'one ought always to take into account new evidence which is unexpected in the light of known evidence' then there is a proof in the Probability Calculus that this rule will realise the aim. The proof is immediately provided by Bayes' Theorem in the form:

p(T, E&K) = p(E, T&K)xp(T, K)/p(E, K)

On certain conditions concerning the numerator on the right hand side, p(T, E&K) is inversely proportional to p(E, K), i.e., the expectedness of new evidence E with respect to old evidence K. If the expectedness is high, i.e., p(E, K) is close to 1, then the goal of increased probability will be realised, but only to a very small extent. However if the expectedness is low, i.e., p(E, K) is close to 0, then the goal of increased probability will be realised in a quite striking way. Using the Probability Calculus in this way, an *a priori proof* can be given of the important principle of many SMs that new expected evidence will realise the goal of increase in probability of our theories; but there is an additional boon in that the proof sets out conditions under which the principle holds.

6. METHODS AS CONVENTIONS: THE EARLY POPPER

Even though Popper remained fairly consistent about what were his preferred principles for a theory of scientific method for critically evaluating scientific theories, commonly called 'Critical Rationalism' (CR), he adopted different meta-methodological accounts of his CR. The early Popper regarded his methodological principles as conventions and justified his SM in much the same way in which one would adjudicate between rival conventions. But the later Popper adjudicated between his own and rival SMs by comparing them with historical judgements about what was, and what was not, great science. In this respect the later Popper and Lakatos have closely similar views about meta-methodology — it is 'quasi-empirical', as they say, and not *a priori* or conventionalist in character. In this section we will examine the views of the early Popper and in the next the later Popper and Lakatos together.

 The early Popper was impressed by the way in which science, unlike many other bodies of belief, was open to radical revision (on the whole), such revisions being best exemplified by the overthrow, in the first quarter of the twentieth century, of the highly confirmed Newtonian mechanics by the Special and General Theories of Relativity and by Quantum Mechanics. What makes such radical revision possible? Popper viewed the testing of scientific theories hypothetico-deductively; hypotheses and theories are tested in the context of other auxiliary assumptions by drawing out their test consequences for comparison by observation or experiment. (Observation and experimentation, along with the auxiliaries, might also involve more theory; but this would not be currently under test. In addition Popper allowed for the testing of statistical hypotheses and non-deductive, as well as deductive, drawing out of test implications to be compared against observational reports, or what Popper called 'potential falsifiers'). Because of the unrestrictedly general character of our theories, Popper argues that they cannot be verified; but they could be open to falsification.[[18]](#endnote-18) Thus what makes a scientific hypothesis open to revision is the fact that in principle it has test consequences, each of which has a potential falsifier; these may either remain potential if the test implication is correct or become actual if it is false. Having potential falsifiers which are actual is a necessary, but not a sufficient, condition for falsification; but once the further conditions for falsification are realised[[19]](#endnote-19) then the demand for revision becomes imperative.

 Being open to revision is thus linked to being open to tests — the more open to tests, the more opportunities there are for revision if actual falsification occurs. Popper promotes the logico-epistemic property that scientific theories possess of being falsifiable into a central role in forming his methodologically based demarcation criteria for science.[[20]](#endnote-20) Thus the value of radical revisability is cashed out in terms of falsifiability (or testability, of which a theory needs to be given, including an account of degree of testability). This logico-epistemic property in turn becomes one of Popper's conventionally adopted values: 'My criterion of demarcation will accordingly have to be regarded as a *proposal for an agreement or a convention*' (Popper, 1959, p. 37). What is important here is that the mere logico-epistemological property of falsifiability is not Popper's demarcation criterion in, say, the same fashion as the related Verification Principle was proposed as a criterion of demarcation. Rather demarcation arises from conformity to a set of methodological principles, in which the demarcation proposal plays a central role; but it is the principles that do the work of demarcation for Popper and not the mere logico-epistemological property of falsifiability.

 Though falsifiability is a key Popperian value, other values are also endorsed. One such value is increase in explanatory depth (Popper 1972 'The Aim of Science'), which Popper alleges can be cashed out in terms of increasing falsifiability. In the 1950s Popper also came to adopt increased verisimilitude as a value; and this is also linked to increased falsifiability. What rules of method realise these values? Thought they play an important role in Popper's theory of Critical Rationalism they get scant attention, and are not carefully formulated. Popper eschews any rules of inductive or probabilistic support, claiming that science can get by with only rules of deduction and his theory of non-inductive, non-probabilistic, corroboration (even though probability relations are employed in more formal attempts at the definition of corroboration). It is over the issues of inductive support and confirmation that much of the debate between Popper and his opponents has taken place.

 But there are more positive rules that Popper sets out (especially in Popper 1959, §11 entitled 'Methodological Rules as Conventions') in which he speaks of 'the rules of the game of science'. Such rules are said to differ from the rules of pure deductive logic and are more akin to the rules of chess: '… an inquiry into the rules of chess could perhaps be entitled 'The Logic of Chess', but hardly 'Logic' pure and simple. (Similarly, the result of an inquiry into the rules of the game of science — that is, of scientific discovery — may be entitled 'The Logic of Scientific Discovery')' (Popper 1959 §11). Popper's first example of a rule is not even expressed in rule form: 'the game of science is, in principle, without end' (*ibid*., p. 53). If we were to stop subjecting our scientific claims to test and regard them as 'verified' then we would give up the critical stance. We can take Popper to be proposing an anti-dogmatism rule which bids us: 'subject all claims to test'. However such a rule, to be at all practicable, must be qualified by considerations of diminishing returns. Popper's second example of a rule (*loc. cit*.) spells out one way in which his rules are a 'logic of *discovery*'. The discovery is not so much the *invention* of hypotheses (Popper has ruled this out), but rather the discovery of either which hypotheses we should provisionally accept (they pass tests) or which we should reject (we reject hypotheses because either they have been falsified through hypothetico-deductive testing, or we have rival hypothesises which are more testable).

 When Popper introduces his proposal for a demarcation criterion, he recognises that it is always possible to evade falsification by decreasing the degree of testability of a hypothesis through adopting various 'saving' stratagems (*ibid.,* §6). To combat these stratagems he adds a necessary methodological supplement to his demarcation criterion in the form of a supreme rule about all other rules of method: 'the other rules of scientific procedure must be designed in such a way that they do not protect any statement in science from falsification' (*ibid.,* p. 54). This supreme rule is rather contentless; and, as has been mentioned in §3, is a rule which is redundant with respect to the value it realises. However Popper's more specific anti-*ad hoc* rules are neither contentless nor redundant. The first of these concerns the introduction of saving hypotheses to rescue a theory which has been refuted: 'only those [saving hypotheses] are acceptable whose introduction does not diminish the degree of falsifiability or testability of the system in question, but, on the contrary increases it'[[21]](#endnote-21) (*ibid.*, pp. 82-3). Theories can also be saved by a number of stratagems directed not at the theory under test but the observations or experiments which allegedly refute them (e.g., questioning the competence of the observers or experimenters). To combat this Popper proposes a second anti-*ad hoc* rule: 'intersubjectively testable experiments are either to be accepted, or to be rejected in the light of counter-experiments'. A third anti-*ad hoc* rule is introduced to combat the saving of theories by altering the meanings of their constituent terms. Though no rule is specifically proposed, Popper intends a two-part prescription. The first is that there be a rule requiring semantic stability of terms in hypotheses which are undergoing test. The second is that theories not be presented as sets of implicit definitions which cannot be tested; rather that theories must be regarded as a set of (largely) empirical claims open to test (*ibid.*, §17 and §20).[[22]](#endnote-22) All three anti-*ad hoc* rules have the same value of increasing testability.

 Why the three anti-*ad hoc* rules? Popper recognises that there is a view of scientific method which rivals his own and against which, unlike inductivism, he has no argument. This is a Conventionalist view of method against which Popper can only say: 'underlying it is an idea of science, of its aims and purposes, which is entirely different from mine' (*ibid.*, p. 80), and 'the only way to avoid conventionalism is by taking a *decision*: the decision not to apply its methods' (ibid., p.82). Popper uses the term 'conventionalist' in at least two ways. The first concerns the status of his rules of method; these are conventions in the sense of proposals for an agreement. In Popper's view SMs could not have the status of empirical claims of science; such a view he criticises as 'naturalistic' (*ibid*., §11). And it was evident to him that principles of an SM could not be known to be *a priori* true (or even analytically true). Since the *a priori*/empirical distinction is exhaustive, the only alternative to declaring such principles to be meaningless was to view them as conventions which we could adopt for various purposes. Thus despite disclaimers to the contrary, Popper was still imbued with some of the positivism of his day. The second use of 'Conventionalism' is as the name of an SM. We can glean what Popper takes this to be since a Conventionalist SM adopts rules and values opposed to those of Popper's Critical Rationalism, viz., whatever a Conventionalist SM has as its values, falsifiability is not one of them; and it positively advocates what Popper's anti-*ad hoc* rules prohibit (see *ibid*., p. 81).

 That there are rival theories of SM is a central idea concerning the table drawn up in §2. What is of interest here is Popper's meta-methodological claim that there is no way of adjudicating between rival SMs such as Conventionalism and Critical Rationalism, except to make a decision to adopt one, and not to adopt, or reject, the others. In Popper's view the values embodied in these SMs are so fundamental that no argument can be given for adopting one value over another without a prior commitment to some value. However Popper does not always adopt such a decisionist conception of meta-method, for elsewhere he suggests ways in which rival SMs with their disparate rules and values might be compared. On what grounds should one adopt the rules and values of Popper's SM of Critical Rationalism? Popper says of his own proposal: 'it is only from the consequences of my definition of empirical science and from the methodological decisions which depend upon this definition, that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavours' (*ibid*, p. 55). For example, if scientists intuitively favour the exposure of a theory to test, especially where the test concerns novel test consequences, or they like the challenge opened by a falsification, then Popper says that they will favour his methodology of Critical Rationalism which incorporates these intuitions over the Conventionalist SM which plays them down (*ibid*., p. 80).

 Thus it appears that Popper does adopt a meta-methodological stance that is not merely based on decisions which lie beyond evaluation. He adopts a hypothetico-deductive meta-methodology in which the consequences of any set of rules and values defining some SM are to be drawn out and compared on a number of grounds with some test bases. The first of these are the intuitions of scientists about values and rules embodied in their own scientific endeavours. Other 'test bases' are more philosophical; they concern the ability of any proposed SM, which Popper treats as akin to a theory of knowledge, to uncover inconsistencies and inadequacies in previous theories of knowledge, and to solve problems within epistemology. Thus if a meta-methodology is to be attributed to the Popper of *The Logic of Scientific Discovery* for adjudicating between rival SMs, it is a version of his own theory of SM which he applies to the sciences; but it is elevated to a higher level of meta-methodology and adapted so that it can deal with the assessment of theories which are themselves not empirical in character. On these grounds Popper is able to dismiss Inductivist methodology since his own SM of conventionally adopted rules and values allegedly solves the problems which Inductivism faces (largely by allegedly by-passing them); and he dismisses Conventionalism not because of any problem he can detect in it but because of its alleged inconsistency with the intuitions of scientists about values and the rules to be adopted to preserve those values. It is this last idea which comes to the fore in a slightly different form in the later Popper and Lakatos, and in the context of a more overtly empirical meta-methodology.[[23]](#endnote-23)

 The idea that principles of method are expressions of means for some epistemic or cognitive end is implicit in the account Popper gives of methodology in *The Logic of Scientific Discovery* (see especially sections 9, 10, 11 and 20). It is more explicit in later works when he says in criticism of some doctrines of essentialism in science: 'I am concerned here with a *problem of method* which is always a problem of the fitness of means to ends' (Popper 1963, p. 105 fn. 17). And later he emphasised that the 'ought' of methodology is a '*hypothetical* imperative' with the growth in scientific knowledge as its single goal and its means the critical method of trial and error (Popper 1974, p. 1036). The idea that principles of method are hypothetical imperatives will become important in the discussion of normative naturalism in §11.

7. EMPIRICAL APPROACHES I: INTUITIONISM IN POPPER, LAKATOS, AND REFLECTIVE EQUILIBRIUM

Popper's final extended treatment of issues to do with method occurs in his 'The Problem of Demarcation' (Popper 1974). Popper revisits his earlier definition of science in terms of his criterion of demarcation, as well as the definitions of others, and casts aspersions on them all saying: 'A discussion of the merits of such definitions can be pretty pointless' (Popper 1974, p. 981) The reasons for this have to do with his change in meta-methodological justifications for adopting SMs, since he continues: 'This is why I gave here first a description of great or heroic science and then a proposal for any criterion which allows us to demarcate — roughly — this kind of science'. For Popper the paradigms of great or heroic science are the laws of Kepler and the theories of Newton and Einstein; instances of disreputable science are those of Marx, Freud and Adler.

 Popper's new meta-methodological criterion for assessing SMs is the following procedure: any acceptable SM must capture those sciences on the great/heroic list and miss none out (otherwise some other SM might do better); and it must capture none of the sciences on the disreputable list (on pain of refutation). Popper does not say how the two lists are drawn up; but given the lists they constitute a (fallible) foundation against which SMs can be tested in much the same way that scientific observations are a (fallible) foundation against which scientific theories can be tested. Thus once more Popper's method of Critical Rationalism has been elevated to meta-methodology, but this time it has been provided with a different test basis for theories of SM in the form of the two lists of heroic and disreputable science. Given such a fallible test basis, Popper's approach to the status of SMs is thereby empirical (or 'quasi empirical as Lakatos puts it) rather than *a priori* or conventionalist.

 Popper's intuitions about what is, and what is not, great science seem to be plucked out of the air. In contrast Lakatos suggests a different way of drawing up the fallible foundation against which SMs are to be tested. Appeal is made to the general community of scientists working in a given field, the 'scientific élite', and their judgement as to what is, and what is not, an acceptable move with each change within their prevailing web of scientific belief. Such judgements need not, and better not be, informed by some theory of SM if they are to test SMs. Rather the judgements arise out of the day-to-day workings of scientists independently of methodological or philosophical reflection upon their scientific enterprise. While there is considerable dispute, even amongst scientists, over what is an acceptable SM, there is, alleges Lakatos, no comparable dispute over whether some move in 'the game of science' is scientific or unscientific (Lakatos 1978 p. 124). Granted such an admittedly fallible 'foundation' of value-judgements, Lakatos argues that Popper's own theory of SM, when elevated to a meta-method for testing SMs against the 'foundation', is falsified (see Lakatos 1978 pp. 123-31). Lakatos' case against Popperian Critical Rationalism in part turns on case histories. For example, the alleged Popperian requirement that all theories must have falsifiability conditions laid down before any testing can take place is violated by Freudian psychology; so it is declared to be non-scientific. But equally, argues Lakatos, no Newtonian has ever laid down falsifiability conditions for Newtonian mechanics. So it is equally unscientific — thus removing one of the paradigm cases of heroic and great science. So Critical Rationalism is criticised and found wanting by its own criteria.

 Using the same Popperian meta-method, Lakatos argues that other theories of SM, in particular Inductivism, Conventionalism and his own SM of Scientific Research Programmes (SRP) can also be falsified. However Lakatos can see no reason why he should accept Popperian meta-method while rejecting Popperian SM; so he replaces it with his own methodology of SRP elevated to a meta-method, but retains the test basis in the same 'foundation' of judgements made by a scientific élite. SRP as a meta-method is a research programme within the historiography of science. According to Lakatos all histories of science are written with some implicit theory of SM in mind. Each SM will render episodes in the history of science rational by its own lights, thus providing an 'internalist' explanation of why some episode occurred in the history of science. However it will not be able to explain all episodes, even some of those judged to be acceptable moves by the scientific élite. These will be relegated to an 'externalist' approach to history of science in which the 'irrational' leftovers are available for social or psychological explanation. There will always be a residue of irrational leftovers; however the task of a SRP applied to the historiography of science will be to discover which of the various SMs maximise the number of episodes in the history of science its makes 'rational' by its own lights, i.e., maximise internalist explanations of scientific change.

 Just as an SRP requires that there be some empirical progress, i.e., that it uncovers novel facts, in order for it to be dubbed 'scientific', so a successful historiographical SRP will uncover some novel historiographical facts. The notion of novelty need not be confined to the discovery of previously unknown facts; it also includes those facts which are known but which get their first explanation within some new progressive SRP while rival SRPs of longer standing failed to explain them. The same applies to judgements made by the scientific élite; these can be novel in the sense that they get their first internalist explanation in terms of some SM while all previous SMs treated them as an irrational leftover for psycho-social explanation. In terms of his own criteria for competing SRPs, Lakatos bids us accept that SRP which is progressive with respect to its rivals. In the case of historiographical SRPs, we are to accept that SRP which is progressive in the sense that it renders rational (by its own lights) judgements of the scientific élite that no other historiographical SRP was able to render rational (by their own lights). As Lakatos puts it: '*progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational*' (Lakatos 1978, p. 133).

 What Lakatos has done here is to take his own theory of SM, which he applies to ordinary sciences, and elevate it to a meta-methodology whereby he can assess rival SMs according to whether or not they show empirical progress by rendering rational more of the historical changes in science. Once more an important role is played by the basic value-judgements of the scientific élite about scientific change. The history of science that is 'rationally reconstructed' according to some SM is said to be 'value-impregnated' because it is based in the élite's value-judgements. Lakatos recognises that no SM will capture all such value-judgements, adding that while this would be a problem of Popper's SM elevated to meta-methodology (it gets falsified by its own meta-criterion), it is not a problem for his own SM of SRPs elevated to meta-methodology (since it allows for progress in an ocean of anomalies): '*rational reconstructions remain for ever submerged in an ocean of anomalies. These anomalies will eventually have to be explained either by some better rational reconstruction or by some 'external' empirical theory*' (Lakatos 1978 p.134)

 Finally the role played by the basic value-judgements of the scientific élite explains why Lakatos' meta-method is 'quasi-empirical' and not *a priori* or conventionalist. The history of such judgements provides an empirical basis against which SMs are to be tested, using whatever meta-criterion of test. It has been assumed that such judgements are unproblematic and readily available. But are they? Lakatos cannot relegate all sociological considerations in science to external factors; some sociology is needed to survey the scientific élite to discover what are their judgements about particular moves in the game of science. Nor should it be assumed that there would be unanimity amongst the élite; a sociological survey might show that the views of scientists ranged from strong consensus to equal division for and against. Nor is it clear what the lowest threshold for agreement might be; if there is less than, say, 80% agreement then some of the value-judgments might not be useable to decide important methodological matters. In addition allowance might have to be made if the scientific community were to change its views about some episode over time. Moreover, how is the scientific élite to be determined? We should not admit that all scientists can make value-judgements about all moves in all the sciences, including the many sciences with which they are unacquainted. Nor should we allow the élite to be chosen by the fact that they are good scientists, for what counts as a 'good' could well turn on whether they are appropriate users of some SM — the very matter over which the judgements of a scientific élite have been invoked in order to make adjudications. Nor should any methodology be invoked by the élite as the grounds on which their judgements are made on pain of similar circularity. Presumably philosophers are to be excluded from the ranks of the scientific élite since most of them are untutored in the ways of science; if this is the case then Popper's own list of heroic and disreputable science is hardly to be given the significance he gives it as a test basis.

 The earlier Laudan of *Progress and Its Problems* also assumed that we have '*our preferred pre-analytic intuitions about scientific rationality*' (Laudan 1977, p. 160) based on episodes in the history of science which are much more firm than any intuitions we have about the theories of SM that embody such rationality. But he later abandoned any such role for pre-analytic intuitions for the above reasons, and others such as the following. SMs are deprived of any substantive critical role; this is to be played by the intuitions which no SM should overturn. Nor is it clear that the intuitions will single out some preferred SM above all others; it might well be the case that given all the widely acceptable intuitions, SMs as disparate as those advocated by Bayesians, Popperians, Lakatosians and Kuhnians might fit them equally as well. That is, in Quinean fashion our intuitions, which play a role similar to that of observations in science, might underdetermine SMs in that two or more SMs are tied for best fit with the intuitions (Laudan 1986). In a subsequent section we will look at the view developed by the later Laudan — normative naturalism.

 The idea that we can pit intuitions about particular cases against rules (or principles, of some SM) is the core idea behind another meta-methodological approach known as 'Reflective Equilibrium' (RE), in either a more narrow or a broader version depending on how much is allowed into the equilibrating process. Nelson Goodman originally expressed the idea of RE as a meta-methodological principle for adjudicating between the particular inferences we make and the deductive rules we adopt, saying: '… rules and particular inferences alike are justified by being brought into agreement with each other. *A rule is amended if it yields and inference we are unwilling to accept; an inference is rejected if it yields an inference we are unwilling to amend*' (Goodman 1965, p. 64). Goodman's meta-methodological principle RE can be extended from the case of deductive to inductive logic and to other areas, its best known extension being to the case of ethics in which RE 'brings into agreement' moral principles and intuitions about particular moral examples.

 How might RE adjudicate between rival SMs? It is not clear that it could do a better job than the meta-criteria proposed either by Popper or Lakatos. Suppose we are given a set of intuitions I (about moves in the game of science based, for example, on the judgements of some scientific élite) and a set of principles (rules) P of an SM. First, there is the unclear notion of what might be meant by I and P being 'brought into agreement'. In the case of deductive logic the notion of logical form plays a crucial role; a necessary condition for principles and particular arguments to be 'bought into agreement' is that the same logical form needs to be found in both. In the case of methodology 'bringing into agreement' can only mean that an intuition Ii about a particular move in the game of science is such that a principle Pj of an SM is able to render that move rational in the sense of giving an internalist explanation of that move.

 In addition the notion of 'bringing into agreement' is said to have justificatory force. Thus if Ii and Pj were be bought into agreement with one another then they would both receive justificatory support. This is a significant additional claim since justification does not flow merely from being brought into agreement. In the case of deductive logic, if all our unacceptable intuitions and unacceptable (invalid) rules were to be bought into agreement with one another through systematising all invalid arguments, it could not be said that they would thereby be justified. Further RE might be made more realistic by giving different weightings to intuitions and principles; this would affect whether of not they can be bought into agreement. Thus given the same weightings to Ii and Pj it might not be possible to bring them into agreement and, say, Ii is discarded. But such importance might be attached to preserving Ii in any theory of SM that a weighted Ii is accepted along with Pj which perhaps does not do too good a job of explaining the episode about which such a strong intuition has been expressed.

 If such an approach can be made to work, at best it will suit only one SM at a time; it cannot compare SMs. To compare them, one needs to investigate, first, the extent to which each SM is able to bring its Ps and the Is 'into agreement' and, second, to be able to compare SMs according to the extent they are able to do this. That is, there needs to be some consideration about principles which pass the RE test and intuitions which are to be dropped (or *vice versa*) — and the significance, if any, to be attached to which intuitions are dropped or maintained. Thus the meta-methodological criterion of RE needs supplementing with a further principle about how well each SM produces its 'agreements'; that is, we need a meta-criterion which is not merely RE, but *best overall* RE. And it will be the notion of which RE does *best overall*, and not RE *simpliciter*, that will do most of the work in determining which SM we should adopt.

 In the critical literature (Stich 1990, chapter 4 and Siegel 1992), other issues are raised about RE pertinent to their application to methodology. The first is whether passing the RE test is *constitutive* of the 'correctness', 'validity' or justification of the principles of method, or merely *good evidence* for their justification or 'validity' or 'correctness'. The second concerns the status of RE (either supplemented or not), viz., whether it is some kind of conceptual truth which can be known *a priori*, or whether it is non-conceptual and knowable only *a posteriori*, or whether it is a non-conceptual truth which is necessary but is knowable only *a posteriori*. Such questions are important given the schema set out in the table in §2 for classifying meta-theories used to adjudicate between SMs. If we follow the critique provided by Stich and Siegel, we are led to the negative conclusion that, whatever the status of RE, it is not a principle that should be adopted to adjudicate between even principles of logic, as originally proposed by Goodman. Thus the prospects of RE as a meta-methodology are not promising, even though some meta-methods do adopt a way of proceeding that is reminiscent of aspects of RE.

8. KUHN'S THEORY OF WEIGHTED VALUES

Those who view Kuhn as holding either an irrationalist or anti-methodology stance, or endorsing a paradigm-relative account of method, can find passages in the Kuhn of 1962 that support these views. Using a political metaphor to describe scientific revolutions Kuhn says of scientists working in different paradigms that 'because they acknowledge no supra-institutional framework for the adjudication of revolutionary difference, the parties to a revolutionary conflict must finally resort to the techniques of mass persuasion, often including force' (Kuhn 1962, p. 93). Continuing the metaphor, there is also a suggestion that the methods of evaluation in normal science do not carry over to the evaluation of rival paradigms:

Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense. (Kuhn 1970, p. 94)

Later he speaks of paradigms 'as the source of the methods … for a scientific community' (*ibid*., p. 103). These and other passages tell us that methodological principles might hold within a paradigm but that there are no paradigm transcendent principles available. Thus it would appear that Lakatos was right to say of paradigm change: '*in Kuhn's view scientific revolution is irrational, a matter for mob psychology*' (Lakatos 1978, p. 91).

 By the time he came to write the 'Postscript' for the 1970 edition of his book, Kuhn effectively abandoned talk of paradigms in favour of talk of exemplars and disciplinary matrices. Values are one of the elements of a disciplinary matrix; and contrary to the impression given above, they are 'widely shared among different communities' (*ibid.*, p. 184). That is, scientists in different communities, and so working in different 'paradigms', value theories because of their following features: they yield predictions (which should be accurate and quantitative rather than qualitative); they permit puzzle-formation and solution; they are simple; they are self-consistent; they are plausible (i.e., are compatible with other theories currently deployed); they are socially useful. These quite traditional notions turn out to be Kuhn's paradigm transcendent criteria of theory choice.

 In a 1977 paper 'Objectivity, Value Judgement, and Theory Choice' (Kuhn 1977, chapter 13) re-endorses these values and adds to them: scope (theories ought to apply to new areas beyond those they were designed to explain), and fruitfulness (theories introduce scientists to hitherto unknown facts). Kuhn initially thinks of these as rules of method in the sense introduced in §3. But owing to the imprecision which can attach to their expression as rules, and the fact that they are open to rival interpretations and can be ambiguous in application or can be fulfilled in different ways, Kuhn prefers to think of them as values to which we could given our general assent.[[24]](#endnote-24) Thus Kuhn adopts a methodology which avoids talk of the rules which we ought follow as means to realise values, and instead focuses on the values we do, or ought to, adopt in our choice of theories.

 Kuhn's list of values does not mention several other important values that have been endorsed by other methodologists. Thus Kuhn does not mention that inductivists and Bayesians put high store on high degree of support of hypotheses by evidence. However in the paper by Worrall in this volume it is argued that it is possible to reconcile this apparent omission with Kuhn's views; the Kuhnian values of scope and fruitfulness are linked to the notion degree of support, even though Kuhn does not spell this link out. Again constructive empiricists put high value on theories which are empirically adequate; in contrast realists wish to go further and value not only this but also truth, or increased verisimilitude, about non-observational claims. Given what Kuhn says elsewhere[[25]](#endnote-25) we may view him as not endorsing the realists' value of truth, though the constructivists value of empirically adequacy is one he could adopt. Finally some methodologists would downplay some of the values Kuhn endorses, such as external consistency or social utility.

 The position of Kuhn on methodology after the first edition of *Structure* yields the following picture of a model of weighted values. (1) There is a set of values which can vary over time, and can vary from methodologist to methodologist. A sub-set of these values could comprise a cluster of central values which hold for most sciences and throughout most of their history. Kuhn's own model is more akin to the latter with his set of values forming a tight cluster without a containing larger set. (2) These values are used to guide and inform theory choice across the sciences and within the history of any one science, including its alleged 'paradigm' changes. That is, the cluster of values are science and paradigm transcendent. (3) The model may be either descriptive or normative. Kuhn does not make it clear whether his model is to be understood as a description of how scientists do in fact make their choices, or whether it is to understood normatively in that it tells us how we ought to make choices. If the latter then there is a need for a justification of the norms it embodies. (4) Kuhn says that the values may be imprecise and be applied by different scientists in different ways. While this is not the case for the value inconsistency (there are fairly precise criteria for internal and external inconsistency for any theory), or for any given degree of accuracy of predictions, some values do exhibit imprecision. Thus accuracy could differ in the required degree. Simplicity might be taken in different ways (simplicity in equations versus simplicity in *ad hoc* assumptions) so that different aspects of a theory might be deemed simple; or the notion of simplicity itself might be taken in different ways or in different degrees. Such imprecision in the interpretation of values can, however, be readily overcome by precisification so that there need not be the wide divergence over the interpretation of values that Kuhn alleges. (5) Different scientists do, as a matter of fact, give different weightings to each of the values.

 Following from (4) and (5) there are two aspects in theory choice — an objective aspect in shared values and a subjective aspect in idiosyncratic weightings of values (and interpretation where this arises). In the light of this, Kuhn claims that there is no general 'algorithm' for theory choice — though there is hardly any methodologist who has required that methodological principles should be algorithmic. This allows that different scientists can reach different conclusions about what theory they should choose. First, they might not share the same values; but where they do share the same values they might interpret them differently or give them different weightings. Shared values (with the same interpretation) and shared weightings of these values will be sufficient for sameness of judgement within a community of scientists. However this might not be necessary; it might be possible for scientists to make the same theory choices yet to have adopted different values and/or have given them different weightings. Thus there is the possibility that consensus might be a serendipitous outcome despite lack of shared values and different weightings. However it is more likely that, where values and weightings are not shared, different theory choices will be made, and there is no consensus.

 Whether scientists do or do not make theory choices according to Kuhn's model is a factual question to answer. But what does the model say about what we ought to do, and what is its normative/rational basis? In particular why, if T1 exemplifies some Kuhnian value(s) while T2 does not, should we adopt T1 rather than T2? Kuhn's answer to the last meta-methodological question is often disappointingly social and/or 'intuitionistic' in character. In his 1977 paper Kuhn refers us to his earlier book saying: 'In the absence of criteria able to dictate the choice of each individual, I argued, we do well to trust the collective judgements of scientists trained in this way. "What better criterion could there be", I asked rhetorically, "than the decision of the scientific group"' (Kuhn 1977, pp. 320-1). As to why we ought to follow the model, Kuhn makes a convenient is-ought leap when he says in reply to a query from Feyerabend: "scientists behave in the following ways; those modes of behaviour have (here theory enters) the following essential functions; in the absence of an alternative mode that would serve similar functions, scientists should behave essentially as they do if their concern is to improve scientific knowledge' (Lakatos and Musgrave (eds) 1970, p. 237). The argument is not entirely clear, but it appears to be inductive: in the past certain modes of behaviour (e.g., adopting Kuhn's model of theory choice) have improved scientific knowledge; so in the future one ought to adopt the same modes of behaviour if one wants to improve scientific knowledge. As will be seen a similar meta-inductive argument is at the heart of the meta-methodology of normative naturalism; so Kuhn's model needs to be assessed in the same way advocated by that meta-method.

 More recent comments from Kuhn (in a paper entitled 'Rationality and Theory Choice') on the status of his methodology arise in a 1983 symposium on 'The Philosophy of Carl G. Hempel' with Salmon and Hempel. In subsequent reflection on that symposium, Salmon argues that it is possible to reconstrue the features of Kuhn's model of weighted values in terms of subjective Bayesianism (Salmon 1990; see also the paper by Worrall in this volume and Earman 1992, chapter 8 for a further attempt to incorporate Kuhn's model into Bayesianism). Bayes' Theorem is able to account for a large number of our central methodological principles, including accuracy, fruitfulness, scope, and so on (but not social utility unless set in a decision-theoretic context). If Salmon's project in which 'Tom Kuhn meets Tom Bayes' is able to account for the theory choices of Kuhn's model, then the independent status of the model is undercut as it is incorporated into a more wide ranging theory of method.

 In his symposium paper Kuhn addresses a point that Hempel had made in an earlier paper about his position, viz., that Kuhnian values are goals at which science aims, and not means to some goal such as puzzle-solving. Here we have followed both Kuhn and Hempel in taking puzzle-solving, accuracy, simplicity, etc to be values (ends) rather than rules (means), though aspects of them as means do inform their function as ends when employed in theory choice. Given that theories are judged by the values they exemplify, Kuhn takes up a further point that Hempel makes, viz., that rationality in science is achieved through adopting those theories that satisfy these values *better*. Hempel thinks that this criterion of rational justification is near-trivial. However Kuhn turns Hempel's near-triviality into a virtue by proposing that the criterion is analytic, thereby adopting as his meta-methodology a theory of analyticity concerning the term 'science'. In developing his views in this paper Kuhn tells us that he is 'venturing into what is for me new territory' (Kuhn 1983, p. 565). So we can take it that the meta-methodological justification developed here is not one that Kuhn had had in mind before.

 Kuhn's account of analyticity is based on what he calls 'local holism'. This is the view that the terms of any science can not be learned singly but must be learned in a cluster; the terms have associated with them generalisations that must be mastered in the learning process, and the cluster of terms form contrasts with one another that can only be grasped as a whole. If 'learning' is understood as 'understanding the meaning', then analyticity becomes an important part of the doctrine of 'local holism' for the central terms of each sufficiently broad scientific theory. In Kuhn's view the doctrine applies not only to specific theories such as Newtonian Mechanics with its terms 'mass' and 'force' which must be learned together holistically. It also applies to quite broad notions signified by the terms 'art', 'medicine', 'law', 'philosophy', 'theology', and so on; the central terms associated with these notions must also be learned holistically. Importantly 'science' is another such broad notion to be learned holistically since 'science' is in part to be understood in contrast to these terms.

 Kuhn recognises that not every science we adopt should possess every value since the values are not necessary and sufficient conditions for theory choice; rather they form a cluster associated with the local holism of the term 'science'. But what he does insist is that claims such as 'the science X is *less* accurate than the non-science Y' is a violation of local holism in that 'statements of that sort place the person who makes them outside of his or her language community' (Kuhn 1983, p. 569). For Kuhn, Y's being more accurate is just one of the things that the local holism of the word 'science' makes Y scientific; Y cannot be non-scientific. For Kuhn, Hempel's near-triviality is not breached because a convention has been violated (this would be the position of the early Popper); nor is a tautology negated. Rather 'what is being set aside is the empirically derived taxonomy of disciplines' (*loc. cit*) that are associated with terms like 'science'. Like many claims based on an appeal to analyticity, meaning or taxonomic principles, one might feel that the later Kuhn has indulged in theft over honest toil. However in linking his model of weighted values to the alleged local holism of the term 'science', Kuhn comes as close as any to adopting the meta-methodological stance that his theory of method has an analytic justification for its rationality. In this respect Kuhn's position has close affinities with that of Strawson (1952, Chapter 9, part II) who tried to justify the rationality of induction in much the same way.

 Finally, Kuhn's later position gives no comfort to those sociologists who wish to appeal exclusively to his book:

The publication of Thomas Kuhn's *The Structure of Scientific Revolutions* in 1962 pointed the way toward the integrated study of history, philosophy and the sociology of science (including technology) known today as science and technology studies (STS). … It alerted STS practitioners to the mystified ways in which philosophers talked about science, which made the production of knowledge seem qualitatively different from other social practices. In the wake of STS research, philosophical words such as *truth*, *rationality*, *objectivity*, and even *method* are increasingly placed in scare quotes when referring to science — not only by STS practitioners, but also by scientists themselves and the public at large.’ (Brante *et. al.* *ibid.*, p. ix.)

Kuhn's attempted meta-methodological justification, along with the values he endorses, place his later work firmly within traditional philosophical concerns about scientific method. As many of his critics have noted, his later work is a retreat from many of the claims of his 1962 book.[[26]](#endnote-26)

9. FEYERABEND'S CRITICISM OF METHODOLOGY

For a person who is famous for alleging that the only universal principle of rationality is 'anything goes', or giving his books titles such as *Against Method*, or *Farewell to Reason*, it might come as a surprise to some to find that Feyerabend, in his autobiography completed just before his death, makes the following claim on behalf of rationality: 'I never "denigrated reason", whatever that is, only some petrified and tyrannical version of it' (Feyerabend 1995, p. 134). Or, 'science is not "irrational"; every single step can be accounted for (and is now being accounted for by historians …). These steps, however, taken together, rarely form an overarching pattern that agrees with universal principles, and the cases that do support such principles are no more fundamental than the rest' (*ibid*., 91). Inspecting his earlier career one will find that Feyerabend even proposed some principles of method, such as the Principle of Proliferation, the aim of which is 'maximum testability of our knowledge' and its associated rule is '*Invent, and elaborate theories which are inconsistent with the accepted point of view, even if the latter should happen to be highly confirmed and generally accepted*' (Feyerabend 1981, p. 105).[[27]](#endnote-27)

 Feyerabend opposed the following view of scientific method (call it 'Rationalism' with capital 'R') espoused by Popper and Lakatos (in fact his criticism of Rationalist methodology is almost entirely narrrowly focused upon the principles proposed by the 'Popperian school' and hardly any others):

(I) There is a universal principle (or unified set of principles) of scientific method/rationality R such that for all moves in the game of science as it has historically been played out in all the sciences, the move is an instance of R and R rationally justifies the move.

His opposed position can be easily expressed by shifting the order of the quantifiers in (I) from 'there exists — all' to 'all — there exists', and then adding a qualification about the nature of the principles (call this 'rationalism' with a little 'r'.)

(IIa) For all moves in the game of science as it has been historically played out in all the sciences, there is some principle (or set of principles) of scientific method/rationality R, such that the move is an instance of R and R rationally justifies the move.[[28]](#endnote-28)

This leaves open two extreme possibilities: that a different rule is needed for each of the moves, or the remote possibility that there is still one universal rule which covers all the moves. Feyerabend's position is close to the first alternative. There might be rules which cover a few moves, but the rules are so contextually limited that they will not apply to a great number of moves and other rules will have to be invoked. Feyerabend also has an account of the nature of these rules:

(IIb) Rules of method are highly sensitive to their context of use and outside these contexts have no application; each rule has a quite restricted domain of application and is defeasible, so that other rules (similarly restricted to their own domain and equally defeasible) will have to apply outside that rule's domain. (Feyerabend often refers to these as rules of thumb.)

 Sometimes Feyerabend adopts quite traditional cognitive values for science, such as the rather Popperian Proliferation Principle 'maximum testability of knowledge'. But Feyerabend is also a social critic of science who asks 'what is so great about science with such aims?', and then argues that for some of the moves in the game of science we would have been better off if they had never occurred. For Feyerabend the whole game of science may not be worth playing because science might make a monster of us (Feyerabend 1975, pp. 174-5). There are better things in life than science, such as acting in plays, singing opera or being a dadaist, and such choices are highly contextual. In sum, for Feyerabend there are a number of broad aspects of science to consider, each of which has its respective values, and these in turn are to be associated with contextual and defeasible rules. By shifting focus from one aspect of science to another, Feyerabend is able to abandon contextual and defeasible methodological rules which are allegedly designed to promote epistemic progress in science, in favour of other goals, for example, dialectical, humanitarian, aesthetic and moral goals, which have little to with scientific method as standardly understood. It is respect of these other goals that Feyerabend like to ask 'What is so great about science?' and then answer by saying 'science is one ideology among many' and can be a threat to the democratic life (Feyerabend, 1978, p. 73 and p. 106).

 In taking the position he does Feyerabend is not beyond the pale of rationality; but he is beyond the pale of Rationality. There is much textual support, only a little of which can be cited here, for the view that Feyerabend is not a Rationalist but a rationalist. His opponents are Rationalists who advocate a unique set of universal rules to be applied to all sciences at all times. On occasions he refers to such Rationalising methodologists as 'idealists', or as 'rationalists' (Feyerabend's little 'r' must be read with big 'R' connotations), thereby creating the impression that he must be an irrationalist. But such talk masks Feyerabend's real position.

 Direct support for claims (IIa) and (IIb) comes from Feyerabend's response to critics of the 1975 *Against Method* in his restatement of his position in the 1978 *Science in a Free Society*. Feyerabend contrasts two methodological positions: *naive anarchism*, with which his own position should not be confused (presumably Feyerabend is a sophisticated anarchist); and *idealism* in which there are absolute rules — but they are conditional with complex antecedents which spell out the conditions of their application within the context of some universal Rationalist methodology. Of naive anarchism he says:

A naive anarchist says (a) that both absolute rules and context dependent rules have their limits and infers (b) that all rules and standards are worthless and should be given up. Most reviewers regarded me as a naive anarchist in this sense overlooking the many passages where I show how certain procedures *aided* scientists in their research. For in my studies of Galileo, of Brownian motion, of the Presocratics I not only try to show the *failures* of familiar standards, I also try to show what not so familiar procedures did actually *succeed*. I agree with (a) but I do not agree with (b). I argue that all rules have their limits and that there is no comprehensive 'rationality', I do not argue that we should proceed without rules and standards. I also argue for a contextual account but again the contextual rules are not to *replace* the absolute rules, they are to *supplement* them. (Feyerabend 1978 p. 32)

Thesis (b) marks the crucial difference between naive anarchism and Feyerabend's own position; moreover the denial of (b) shows that Feyerabend cannot be an arch irrationalist. There *are* rules worth adopting. Oddly enough, universal rules are not to be replaced but are to be supplemented. What this means is unclear; but it might be understood in the following way. Consider as an example of a universal rule Popper's 'do not adopt *ad hoc* hypotheses'. For Feyerabend this is not to be understood as a universal ban which applies to all sciences and in all circumstances come what may. Sometimes adopting *ad hoc* hypotheses will realise our aims better than not adopting them. Feyerabend seems to suggest that alongside this universal rule are other rules (equally open to supplementation one supposes) about its application. The task of these other rules will be to tell us about the occasions when we should, or should not, adopt this Popperian rule. What Feyerabend needs is the notion of a defeasible rule, but defeasibility is not something he ever discusses. If rules are defeasible, then universalising 'idealists' and Rationalists will not be able to apply rules regardless of their situation. Viewed in this light the passage cite above supports the position outlined in the two parts of (II), as do other similar passages in his writings (Feyerabend 1975 p. 32 and chapter 15; Feyerabend 1978 pp. 98-9 and pp. 163-4)

 In the passages surrounding the last quotation Feyerabend attempts to distinguish between a modified idealism, in which universal rules are said to be conditional and have antecedents which specify the conditions of their application, and his own view in which rules are contextual and defeasible. Presumably the difference is that for Rationalist 'idealists' the conditions of application of the rules are spelled out in the fully specific antecedents of conditional rules. But in Feyerabend's view such conditions cannot be fully set out in some antecedent in advance of all possible applications of the hypothetical rule; at best such conditions are open-ended and never fully specifiable. So Feyerabend opts for a notion of a rule which is not conditional in form but categorical, and is best understood as contextual and defeasible. So even if rules appear to be universal, as in 'do not adopt *ad hoc* hypotheses', there will always be vagueness and imprecision concerning their application. There is also no mention of the conditions under which they can be employed or a time limit imposed on their application; presumably this task is to be left to supplementary rules.

 Does Feyerabend adopt as his one and only methodological principle '*Anything goes*' (Feyerabend 1975 p. 28)? No:

As for the slogan 'anything goes', which certain critics have attributed to me and then attacked: the slogan is not mine and it was not meant to summarise the case studies of *Against Method* … (Feyerabend 1987, p. 283)

[it] is not a 'principle' I defend, it is a 'principle' forced on upon a rationalist who loves principles but who also takes science seriously'. (*ibid.*, p. 284)

Once again Feyerabend uses the term 'rationalist' to name his opponents; but from this it does not follow that his own position is 'irrationalist'. Instead his view is that one cannot have both the complexities of the actual history of science and a universal methodology of the sort loved by those he variously dubs as 'Rationalists' or 'Idealists'.

 As he explains in several passages of *Science in a Free Society* (Feyerabend 1978 pp. 32, pp. 39-40 and p. 188): *'anything goes' does not express any conviction of mine, it is a jocular summary of the predicament of the rationalist'* (*ibid*., 188).[[29]](#endnote-29) But the joke has backfired and has been costly in misleading many about Feyerabend's real position. If the Rationalists within the critical tradition want universal rules of method then, given that according to Feyerabend all such rules have counterexamples outside their context of application, the only universal rule left is the 'empty, useless and pretty ridiculous' — 'anything goes'. (*loc. cit*) But this is hardly convincing since the Rationalist need not take up Feyerabend's invitation to adopt 'anything goes'. Any Rationalist will see that from 'anything goes' it follows (by instantiation) that every universal rule of method also 'goes'; but if the Rationalist also accepts Feyerabend's claim that these have counterexamples and are to be rejected as universally applicable then, by *Modus Tollens*, the Rationalists can infer that 'anything goes' cannot be an acceptable rule of Rationalist method — even if jocularly imposed by Feyerabend on serious universalising Rationalists. 'Anything goes' does not mean what it appears to say; it is not even a principle of method that Feyerabend endorses.

 Given that one can adopt defeasible principles and remain a rationalist about method, Feyerabend does not appear to be the opponent of theories of scientific method that he is often made out to be, or says that he is. Granted such Feyerabendian principles, what does he say about their justification? There are two approaches. The first would be to take some principle that Feyerabend advocates (such as the Principle of Proliferation or rules of counter-induction) or some principle he criticises (such as the alleged principle of consistency or Popper's anti-*ad hoc* rule) and attempt to evaluate them either on logico-epistemological grounds or on the historical record of the decision context of various scientists. But the latter would take us into a long excursion through episodes in the history of science, and the former has to some extent been carried out.[[30]](#endnote-30) Instead we will look at Feyerabend's meta-methodological considerations in justification for his views on SMs.

 If we are to attribute to Feyerabend a meta-methodology concerning his defeasible rules, then it veers between that of a Protagorean relativist and that of a dialectical interactionist in which principles and practice inform one another. In setting out his position he adopts Popper's term 'tradition'[[31]](#endnote-31) to refer not only to mythical and scientific systems of belief, but also to traditions including those of religion, the theatre, music, poetry, and so on. He also speaks of the rational tradition; but unlike Popper he does not privilege it by claiming some special 'second-order' status for it. For Feyerabend all traditions, including the critical or rational tradition, are on an equal par. In resisting the idea that there is a special status to be conferred upon the rules that comprise the tradition, Feyerabend adopts a Protagorean relativism about traditions — at least in the 1978 *Science in a Free Society*. About traditions he makes three claims:

*(i)Traditions are neither good nor bad, they simply are*. … rationality is not an arbiter of traditions, it is itself a tradition or an aspect of a tradition. …

*(ii) A tradition assumes desirable or undesirable properties only when compared with some tradition. …*

*(iii) (i) and (ii) imply a relativism of precisely the kind that seems to have been defended by Protagoras.*  (Feyerabend 1978, p. 27).

 For at least the Feyerabend of *Science in a Free Society*, there is a rational tradition; and it has contextual defeasible rules which can be used to evaluate claims in other traditions (which in this context we can take to include not only the principles of any SM but also any meta-methodology which attempts to justify any SM). But given his Protagorean relativism about traditions, all such evaluations are from *within* a tradition. There is no absolute tradition, encapsulated in some meta-methodology, which stands *outside* all other traditions and from which we can evaluate them. In this sense no tradition is an absolute 'arbiter' of any other. What does this mean for the contextual defeasible rules of method that Feyerabend endorses? We take this to mean that such rules of method have no further justification other than that they are what we have simply adopted as part of our critical tradition. Their truth, validity or correctness is at best relative to a tradition; there is no further meta-methodological account of their status to be given by appealing to some absolute or privileged tradition of Rationality. It is this relativism that has led some to claim that Feyerabend, even if he admits there are defeasible rules of method, is at heart an irrationalist.

 If Feyerabend really adopts a Protagorean relativism about rules of the sort Plato describes in the *Theaetetus*, then at best we can say that there are rules R-relative-to-tradition-T, and rules R\*-relative-to-tradition-T\*, and not merely rules R and R\* which might come into logical relation with one another. Such a version of relativism undercuts the very possibility of rules ever being assessed with respect to one another. But this is often something Feyerabend requires we do. This suggests that Feyerabend is not really a relativist but a pluralist about rules and the traditions they embody (and pluralism need not entail any relativism). The running together of these two notions is evident in the following passage: 'Protagorean relativism is *reasonable* because it pays attention to the pluralism of traditions and values' (*ibid*., p. 28) What is still excluded by this stance is any attempt to invoke meta-methodology to give an *a priori* or even an empirical justification of his defeasible rules of method. But the pluralism does make possible the critical 'rubbing together' of different traditions, something that Feyerabend would endorse given his principle of proliferation. And it does make possible the following more dialectical view of the interaction between traditions, rules and practices.

 There are remnants of the positions of the later Popper and Lakatos with their appeal to the intuitions of a scientific élite in Feyerabend's talk of the interaction between reason and practice, of which he distinguishes three aspects. First, he acknowledges that reason can be an independent authority which guides our practices in science — a position he dubs 'idealistic'. But also 'reason receives both its content and its authority from practice' (*ibid*., p 24) — a position he dubs 'naturalism'. Though he does not say how reason gets its authority, his position is one in which a strong role is given to intuitions about good practice; this is reminiscent of the later Popper, Lakatos and the meta-method of reflective equilibrium of §7. But both naturalism and idealism have their difficulties, says Feyerabend. Idealists have a problem in that too ideal a view of rationality might cease to have any application in our world. And naturalists have a problem in that their practices can decline because they fail to be responsive to new situations and need to critically re-evaluate their practice. He then canvases the suggestion 'that reason and practice are not two different kinds of entity but *parts of a single dialectical process*' (*ibid*., p. 25).

 But even the talk of reason and practice being separate 'parts' of a single process draws a misleading distinction, and so he concludes: '*What is called 'reason' and 'practice' are therefore two different types of practice*' (*ibid*., p. 26). The difference between the two *types* of 'practice' is that one is formal, abstract and simple, while the other is non-formal, particular and submerged in complexity. Feyerabend recognises that the conflict between these two types of practices (or 'agencies' as he goes on to call them) recapitulates 'all the "problems of rationality" that have provided philosophers with intellectual … nourishment ever since the "Rise of Rationalism in the West"' (*ibid*., pp. 26-7). As true as this may be at some level of abstraction, Feyerabend's shift to a position of dialectical interactionism with its additional plea for a Principle of Proliferation with respect to interacting traditions (includes those of theories of SM), does have the characteristics of an appeal to some meta-methodological theory. But it tells us nothing more than what we had learned from the intuitionistic approaches of §7, except that the task of bringing our practices (in science and elsewhere) into line with our theories of those practices (i.e., our theories of SMs) might be harder than we thought. Looked at this way, we can resist the temptation to go relativist by view the activity of bringing the rules of 'reason' and the particularity of 'practice' into accord with one another as yet just one more activity on a par with any other activity.

10. NIHILISM ABOUT METHOD: THE SOCIOLOGICAL TURN

Nihilism is the view that there is no legitimation possible for any SM, and in particular there is nothing to be found at the meta-methodological level. Reasons for this are various. For those who take Feyerabend to be opposed to all methodology and to claim that 'anything goes', then he is a nihilist about method. But as we have seen, that is not his position. However he comes close to it when, in one of his moods, he advocates a Protagorean relativism of methods to traditions and epochs of science, rather than a pluralism of methods. Similarly we have seen that at one stage Kuhn claimed that methods are paradigm-relative. But the status of their relativisms remains obscure, unhelpful and in the long run wrong as both recognise in their different ways. Nihilism come in other forms. Thus Lyotard on postmodernism in science:

But to the extent that science does not restrict itself to stating useful regularities and seeks truth, it is obliged to legitimate the rules of its own game. It then produces a discourse of legitimation with respect to its own status, a discourse called philosophy. I will use the term *modern* to designate any science that legitimates itself with reference to a metadiscourse of this kind making explicit appeal to some metanarrative such as the dialectics of Spirit, the hermeneutics of meaning, the emancipation of the rational or working subject, or the creation of wealth. … I define *postmodern* as incredulity towards metanarratives. … To the obsolescence of the metanarrative apparatus of legitimation corresponds, most notably, the crisis of metaphysical philosophy and of the university institution which in the past relied on it' (Lyotard 1984, pp. xxiii-xxiv)

Here talk of 'metanarratives' is akin to what we have called 'meta-methodology' (though Lyotard uses the term 'narrative' in a much wider sense than to refer to only second level SMs or to the first level sciences themselves). But it should be noted that no meta-methodologist we have considered has adopted three of the four metanarratives Lyotard mentions in order to legitimate methods in science, and in turn the sciences themselves. What a 'hermeneutics of meaning' might do for any attempt to justify any theory of SM is obscure; in any case it is a topic outside the scope of this 'Selective Survey'.

 What of Lyotard's opening suggestion that issues of legitimation arise when we go beyond stating useful generalities and aim for truth? Suppose we were to eschew truth about science's regularities and stay with useful regularities. One aspect of their usefulness must be that generalities remain correct for the next case to which they allegedly apply — that is, their usefulness turns on methodological principles associated with inductive inference in science. But ever since Hume the meta-methodological matter of 'legitimating' inductive inferences has been with us without the advocacy of postmodernism. Moreover, even for the incredulous postmodernist, we need to be given reason to believe, or a 'proof', that there is no legitimation for principles of method (as in the case of the just mentioned inductive methods, or any other methods). But to show that there is no such 'proof' of legitimation can be just as difficult to establish as that there is a 'proof' of its legitimation (whatever 'proof' is taken to mean here). The upshot is that the postmodernist sceptical nihilist about legitimation must indulge in some meta-methodology, just as sceptics who deny that there is any epistemology must also indulge in some epistemology to establish their case.

 Such is the alleged urgency of the 'crisis' of legitimation that even the university as an institution is threatened. Whatever crises universities face, or the various sciences either in themselves or in relation to society, it is doubtful that solutions to the methodological problem of legitimation will either relieve such crises, or deepen them if no solution is found. However the fruits of a belief in the failure of legitimation are all too evident in much of the intellectual life of universities, and elsewhere. In Lyotard's book there is little discussion of the sciences and principles of method, even of the sort given by the methodologists mentioned elsewhere in this 'Survey'. And its general orientation towards a Wittgensteinian conception of language and rules does little to establish that science and its philosophy has moved into a postmodern phase. So we will set Lyotardian postmodernism aside, but note that there might be other reasons for its claims about science.[[32]](#endnote-32) Some postmodernists appeal to Kuhn and his sociological turn, and to Feyerabend's anarchism, to provide arguments for their case. But as we have seen neither eschew methodology completely, and both attempt to find some way of legitimating its claims. However a case can be made for the postmodernist position by appeal to sociological studies of science — to which we now turn.

 Though sociologists of science often appeal to Kuhn as a precursor, Kuhn resisted any such alliance. Surprisingly he says of a projected book (unfinished before his death): 'It is needed, that is, to defend notions like truth and knowledge from, for example, the excess of post-modernist movements like the strong program' (Kuhn 1991, pp. 3-4). And elsewhere he adds 'I am amongst those who have found the claims of the strong program absurd; an example of deconstruction gone mad' (Kuhn 1992, p. 9). Kuhn goes on to speak of the manner in which, according to sociological studies, a community of scientists is said to reach a consensus about scientific belief. Kuhn reports that negotiation plays a central role, but little else: '"the strong program" has been widely understood as claiming that power and interest are all there are. Nature itself, whatever that may be, has seemed to have no part in the development of beliefs about it' (*ibid*., p. 8). Kuhn's last point is important since for the sociologists what scientists believe is constrained only by the negotiations that take place between themselves and not by any role that nature might play in saying 'yes' or 'no' to such beliefs. So what are the claims of the Strong Programme (SP) in the sociology of scientific knowledge?

 The sociology of science before the mid-1970s was conservative in that it did not view the very content of scientific belief as a social causal product, and thus open to sociological investigation, as were the funding of science, or its gender biases, or its hierarchies, social organisation and reward system, and so on. A radical shift was made by a number of people such as David Bloor, who gave SP[[33]](#endnote-33) its name. Though he states its four central tenets succinctly it is far from clear what they mean. The first, the Causality Tenet (CT), says of SP: 'It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief' (Bloor 1991, p. 7)

 Sociologists do not take the care that philosophers do over the distinction between knowledge and belief. So we will go along with Bloor and understand CT as pertaining to *beliefs* (held on the part of some individual x). Since beliefs can be either true or false, then we can immediately incorporate Bloor's second 'Impartiality Tenet' into CT, viz., '[SP] would be impartial with respect to truth or falsity, rationality and irrationality, success or failure' (*loc. cit*).[[34]](#endnote-34) Thus CT is quite broad with respect to the beliefs within its scope. We will take it to include all scientific beliefs whether they be laws and theories or particular observation statements. Under this heading we can also include the axioms of a systematic presentation of a theory and/or each of the theorems which flow from them (the axioms and theorems being taken either singly or conjointly). CT could also be taken to encompass each of the SMs and meta-methods (including their norms) that philosophers and scientists have proposed, and have been under examination in this 'Survey'; these, too, are grist for the sociological mill of SP understood in its full generality with respect to belief.

 What causes a belief p in the mind of some scientist x? It is x's social condition (call this 'Sx') in cooperation with other conditions of x which are not social (for convenience call these 'Nx'). Thus CT, in its full generality with respect to all beliefs and all persons (including scientists), says:

CT: for all (scientific) beliefs p, and all scientists (and perhaps others) x, there are social conditions Sx, and there are other non-social conditions Nx, which cooperate together such that (Sx & Nx) cause x's belief that p.

What falls within the scope of the non-social causes Nx? It includes items such as our biology, our cognitive structures which we have inherited through the processes of evolution, our similarly inherited sensory apparatus, and our history of sensory inputs (but not our reports of them). Much of the non-social is constant for most of humanity. In contrast the history of our individual sensory input, as well as our individual (or our group's) social condition, does vary from person to person, or from group to group. Thus though the factor Nx must enter into the causal nexus producing our beliefs, it will not explain, apart from appeal to variable sensory input, the variation in our beliefs. What will do this will be the varying social factor Sx. The list of variable social factors is open-ended, but it includes at least: the language we learn and the way we thereby express our beliefs and report our experiences (SP endorses the view that all observation is theory-laden); the beliefs we acquire through acculturation and education; our social circumstance including the class into which we fall. It is the variation in social factors such as these which is alleged to be the main cause of variation in belief. We are to appeal to these factors in offering causally-explanatory accounts of why, say, x believes that p.

 Expressed this way, CT can be viewed as a thesis about the causes of belief within a strongly naturalistic programme encompassing a range of mainly social, but also non-social, factors. It is important to note what SP omits from the list of possible causes of belief; in particular, the norms of reason found in logic or methodology (or more properly when these are believed by x) are not admissible causes of x's other scientific beliefs. Their omission is explicitly required by the third Symmetry Tenet which says: '[SP] would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.' (*loc. cit.*) Much turns on the *types* of causes that produce belief. Less strong sociological theories than SP would allow both of the following: (1) there are false and irrational beliefs in science that could be explained by appeal to some type of factor, e.g., the scientist's political circumstance; (2) a quite different type of factor, e.g., methodological norms, might be invoked to explain true and rationally held beliefs. The symmetry tenet of SP appears to rule out the second quite plausible type of explanation and require that in both cases the causes be of the *same type*, viz., social causes only. This is another sense in which the SP is strong. Only *social* causes are to be admitted in the explanations of belief and its variation. Non-social factors (apart from each person's history of experience) will, of course, also play a causal role, but this can often be relegated to the background of common causal conditions.

 Bloor acknowledges that this is one of the grounds on which his critics have resisted SP, saying: 'The problem running throughout most exchanges over the status of the symmetry requirement lies in the clash between a naturalistic and a non-naturalistic perspective. The symmetry requirement is meant to stop the intrusion of a non-naturalistic notion of reason into the causal story' (*ibid*., p. 177). It is as if the norms of reason and methodology are a *deus ex machina* which intervene in the causal order to produce beliefs; they are not to be admitted as part of the SP's conception of a naturalistic causal order. However Bloor does allow that 'naturalised norms' may enter into the causal order because we are 'natural' reasoners. What this might mean is that, as part of our evolutionary and cultural inheritance, we have acquired beliefs about norms and these 'natural acquisitions' enter in as non-social, rather than social, factors of the causes of belief. (If our natural reasoning capacity is something that arises socially, as does our acquisition of a natural language unlike our Chomskian deep structures, then it enters as a social cause of belief.) But as is well known from research in cognitive psychology, such naturally acquired beliefs about norms offers no grounds for supposing that we use rules of reasoning correctly; given the extent to which we fail to reason according to *Modus Tollens* or commit the gambler's fallacy, the grounds on which we are 'good' 'natural' reasoners are slim indeed.[[35]](#endnote-35) In the long run SP claims about ourselves as 'natural reasoners' comes to the following about the correctness of norms: norm x is correct if and only if the community assents to x. But we are right to be suspicious of sociological formulations which turn merely on communal assent.[[36]](#endnote-36)

 If the social factors are quite external to us (e.g., our class status), how do they manage to causally produce beliefs in our minds in a way which is not overtly behaviouristic? It is unclear how such external factors, such as class status, manage to penetrate our minds to cause beliefs in something as remote as science without at least our *awareness* of our class interests entering into the casual chain. This suggests that cognitive factors enter into the causal story and that it is cognitive attitudes to social factors, such as beliefs or interests, which are causally active and not the external social factors themselves. If so, CT has been improperly expressed since what has been left out are cognitive attitudes to social factors. This omission is remedied in what might be called the 'interests' version of CT (ICT).

ICT: for all (scientific) beliefs p, and all scientists x, there are interests of x, Ix, such that Ix cause x's belief that p.

Here we can drop reference to the non-social factors since the causal chain is alleged to run independently of them at the cognitive level from interests to beliefs. Now ICT is *not* to be understood as a thesis about, say, the interests that fund, or do not fund, research into some science. This is a legitimate issues for a sociology of science to investigate; but it has no bearing on any sociology of *scientific 'knowledge'.* Rather ICT is a thesis within the sociology of scientific 'knowledge' and as such concerns *beliefs* and their *causes*. In this respect ICT is not a special case of CT since what does the causing is a cognitive attitude to a social factor, and not the social factor itself.

 In §1 and §2 matters concerning cognitive and non-cognitive interests in science were raised, and it was asked whether such interests could explain why x believes that T (where 'T' is a scientific theory) better than belief in principles of method could. Suppose T does satisfy those interests, including even cognitive interests such as 'predicts a novel fact'. There it was argued that it was quite serendipitous that having some *interest* in a theory, such as the cognitive interest of predicting novel facts, is causally efficacious in choosing T, which does predict a novel fact, rather than some T\* which predicts no novel fact. As we all know, having an interest in some feature does not guarantee that we can always hit upon a thing with that feature. What is wanted, instead, are some methodological principles which, when we use them, are reliable in choosing those theories which exemplify certain values (such as producing novel facts) in which we have an interest. Methods will serve our interests in when we use them to choose theories; but merely having an interest without a methodology is no guide to choice.

 There is much critical literature on SP.[[37]](#endnote-37) Only one critical point will be raised here. This is the flawed causal methodology adopted by those advocates of SP who undertake historical studies which allegedly exemplify SP. We will consider Forman's study of the emergence of the belief in acausality[[38]](#endnote-38) in physics in Weimar Germany in the decade after the end of World War I. He says of this widespread belief that he 'must insist on a causal analysis' (Forman, 1971, p. 3) thus bringing his analysis into conformity with SP. Now CT maintains that for x's belief in acausality there is some social cause Sx of this belief. The position of the quantifier 'there is' in this formulation of CT shows that the social causes of the same belief can (in some cases must) vary for physicists around the world; the alleged social causes of the belief on the part of Weimar physics will clearly be different from the social causes of the belief of, say, some budding Rutherford in the New Zealand of the time, or elsewhere. Those opposed to SP will argue that even if Weimar physicists acquired their belief in acausality socially, those elsewhere might not. Others might initially get their belief by reading the literature in physics (this is in part a social process of information transmission); but they are quite capable of carrying out their own investigations in the light of some theory of SM to get independent evidence for the belief.

 So let us restrict the scope of x in CT to the case of the dozen physicists of Weimar Germany that Forman investigates. We will accept unquestioningly his analysis of the cultural milieu of the time in which society was hostile to science and to its notions of causality and embraced the neo-romantic, even 'existentialist', doctrines in Spengler's book *Decline of the West* in which a causal view of the world was also condemned. However it is not sufficient merely to claim that the dozen physicists lived in a milieu of this sort; this could be the case whether they were aware of it or not. For the milieu to have an affect on the physicists' beliefs we need to add that they were *aware* of doctrines that were embraced in their cultural milieu. So CT needs to be reformulated as a thesis not about some Sx causing x's belief in acausality (how could it?), but the significantly different thesis: x's *awareness* of Sx causes x's belief in acausality.

 Being socially aware people, most of the dozen physicists Forman investigates were aware of their cultural milieu and its hostility to notions of causality. So was their awareness merely a mental accompaniment to their belief in acausality — this belief being caused in other ways due to the internal development of physics? Or was their awareness of their milieu's hostility the very cause of their belief in acausality? Advocates of SP have to show the latter. And if they can, the result would be rather shocking. Major beliefs in science are just 'social imagery', to quote the title of Bloor's book. They are not acquired on grounds to do with the sciences themselves and their accompanying principles of scientific method; nor are they acquired on grounds to do with how the world is and what our theory says of it, this being one of Kuhn's complaint about SP.

 However what is shocking is the failure of sociologists to employ correctly the methodology of causal analysis to show the following claims. Let 'A' be the physicists' *awareness* of their hostile cultural milieu, 'B' their *belief* in acausality, and P an internalist story based in *physics* about how, on the basis of issues in science alone, acausality came to be believed. Then the following needs to be shown about what went on in the mind of each physicist: (1) A accompanies B; (2) P accompanies B; (3) P does not cause B; (4) A causes B. Now (1) and (2) can hold, as Forman shows us; but this does not show that one of (3) or (4) hold.

 Of the physicists Forman discusses, perhaps Richard von Mises comes nearest to being his best example as he was a convert to the *Weltschmerz* of Spengler. Did von Mises' Spenglerism cause him to believe in acausality rather than matters internal to the physics with which he was acquainted? Despite all the citations of his work, Forman does not show that it was von Mises' awareness A of his cultural milieu, rather than physics P, which caused this belief B. The closest he gets in his research into von Mises' writings on Quantum Mechanics at the time is a revised address about which Forman makes the following unhelpful remark: 'Admittedly, von Mises has invoked the quantum theory as the occasion for the repudiation of causality' (*ibid*., p. 81). But even though von Mises might have used the revised address to tell us of his recent change of mind to belief in acausality, this does not show that it was A, his awareness of his cultural milieu, rather than his physical beliefs P, that caused of his belief in causality, B. But Forman insinuates that this might be so. But to merely insinuate this, and to invoke other reasons or rationalisations based on Spenglerian considerations, has two failings. First, it does not establish causal claim (4); second, either it attributes a massive amount of self-delusion to von Mises or it tells us that he was lying, given that he mentioned his new belief in acausality in the context of his address on physics. Critics are aware of the methodological shortcomings in the studies alleged to support SP, even those who take the view that scientific belief needs both a externalist (sociological) and internalist (using some SM) explanation:

when we come down to the content of physics, we must of necessity take into account internal as well as external considerations. … Forman has succeeded in demonstrating that physicists and mathematicians were generally aware of the values of the milieu …. But when we come to the crucial claims, that there was widespread rejection of causality in physics, and that there were no internal reasons for the rejection of causality, then the weakness in his argument also becomes crucial. For there were strong internal reasons for the rejection of causality … (Hendry 1980, p. 160)

 There are several other ways in which the SP can be interpreted to give and account of theory choice (such as the notion of negotiation) that cannot be explored here. But if SP were true of how we have acquired our scientific beliefs then its nihilistic stance towards meta-methodology would be supported, and its rejection of the role SMs have been alleged to have played in the history of our choice of theories would have been vindicated. If SP were true, it would show that our norms of method can not, and have not, played any critical role in the evaluation of our theories. At best we are to see our science as a mere reflection of our (believed) social circumstance. Further, postmodernists would have some grounds for their incredulity towards metanarratives. But it is doubtful, given their flawed causal methodology, that SP has any case studies which support it. Despite the popularity of SP, and the many confusions which surround how we are to understand and test its theses, the critical role that methodology can play tells us that we are not rationally obliged to believe SP, whatever our social circumstance.

11. EMPIRICAL APPROACHES II: METHODOLOGY NATURALISED

As far as methodology goes, Quine adopts quite traditional principles such as the standard views on confirmation and disconfirmation, simplicity, conservatism (or familiarity), and so on, without attempting a further elaboration of what such principles might be like.[[39]](#endnote-39) Given that Quine is also an advocate of some version of holism as expressed in the metaphor of Neurath's boat, we can assume that not only the sciences but also its principles of method along with logic are open to revision if there is lack of fit between them and our experiences (or reports of experience). In this respect methodology and the sciences are on a par. However the requirement of 'fit' or overall coherence takes us into the province of meta-methodology in which there is a least some meta-principles of overall consistency and/or degree of coherence across our total web of belief. What is of interest, then, is not so much Quine's views on methodology, but his more radical view that there is no first philosophy on the basis of which we can make a meta-methodological stand in, say analyticity, or *a priori* knowledge, or whatever (though the requirements of consistency and coherence, along with a hypothetico-deductive model of testing, are necessary if the holistic picture is to be preserved).

 The deflation of epistemological, and any meta-methodological pretensions is well expressed by Quine in a number of places. First, his 'Five Milestones of Empiricism', the fifth being

naturalism: the abandonment of the goal of first philosophy. It sees natural science as an inquiry into reality, fallible and corrigible but not answerable to any supra-scientific tribunal, and not in need of any justification beyond observation and the hypothetico-deductive method. (Quine 1981, p. 72)

This is a weaker characterisation of naturalism than the one to follow. Pride of place has been given to the H-D method, supplemented with some principles of confirmation and disconfirmation. But one is left wondering whether the H-D method does fulfil the role of a 'supra-scientific tribunal' in needing no justification. Though some methodologists favour the H-D method, many, especially Bayesians, reject it as seriously deficient and paradox ridden.[[40]](#endnote-40) So, what is the justification of any principle of method, H-D or not, within a naturalistic framework?

 A stronger version of naturalism occurs in an often-cited passage from Quine's ‘Epistemology Naturalized’:

Epistemology … simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input … and in the fullness of time the subject delivers as output a description of the three-dimensional world and its history. The relation between meagre input and the torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted epistemology; namely, in order to see how evidence related to theory, and in what ways one’s theory of nature transcends any available evidence. (Quine 1969 pp. 82-3)

Quine appeals only to the science of psychology. But if we extend an invitation to all the sciences to join in an investigation into epistemology as a 'natural phenomenon', then we must allow the sociologists of the previous section their say as well. Broadly construed to include all the sciences, there is nothing in Quine's account of naturalism that a supporter of the Strong Programme could not endorse since, as we have seen, they also admit a role for both social and non-social (including psychological) factors in the causation of belief.

 But the passage is not without the same difficulties that faced the Strong Programme, viz., its account of the normative within a framework of naturalism. Let us focus on methodology, and in particular the evidence relation. This, we might have initially assumed, is partly a normative or evaluative notion in that we speak of good, less good or bad evidential connections between observation and theory. But this appears to have been replaced by a descriptive account, perhaps containing causal or law-like claims within several sciences from theory of perception to linguistics, about the chain of links from sensory input to linguistic outputs. Is Quine's position eliminativist in that the norms of evidential reasoning are deemed to be non-existent in much the same way as we now deem phlogiston or Zeus’ thunderbolts to be non-existent? Or are the norms still there but lead a life in heavy disguise through definition in terms of the predicates of the biological, psychological and linguistic sciences? Or are they supervenient in some way on the properties denoted by such predicates? Whatever the case, we lack an account (if there is one) of the special features of those causal links between sensory input and linguistic output that characterise justified or warranted or rational belief in theory, and the special features which produce unjustified, unwarranted or irrational belief in theory. Moreover which 'human subjects' should be chosen for study: the good or the bad reasoners?

 In his recent Pursuit of Truth Quine changes tack and emphasises the distinction between the normative and the descriptive saying:

Within this baffling tangle of relations between our sensory stimulation and our scientific theory of the world, there is a segment that we can gratefully separate out and clarify without pursuing neurology, psychology, psycho-linguistics, genetics and history. It is the part where theory is tested by prediction. It is the relationship of evidential support. (Quine 1992, p. 1)

This appears to suggest that the evidential relation is outside the scope of the naturalists' programme. In his replies in the Quine Schilpp volume there is also a more modulated position:

A word now about the status, for me, of epistemic values. Naturalism in epistemology does not jettison the normative and settle for indiscriminate description of the on-going process. For me normative epistemology is a branch of engineering. It is the technology of truth seeking, or, in more cautiously epistemic terms, prediction. Like any technology it makes free use of whatever scientific findings may suit its purpose. It draws upon mathematics in computing standards of deviation and probable error in scouting the gambler's fallacy. It draws upon experimental psychology in scouting wishful thinking. It draws upon neurology and physics in a general way in discounting testimony from occult or parapsychological sources. There is no question here of ultimate values, as in morals; it is a matter of efficacy for an ulterior end, truth or prediction. The normative here, as elsewhere in engineering, becomes descriptive when the terminal parameter has been expressed. (Hahn and Schilpp 1986, pp. 664-5)

 Just as Quine once wrote a paper called ‘Three Grades of Modal Involvement’ so there might be a paper called ‘Grades of Naturalistic Involvement’, or, if you like in reverse ‘Grades of Normative Involvement’, in which one investigates a range of possible involvements of the normative with the naturalistic — but neither is the normative totally abandoned, nor is an a priori independent ‘first philosophy’ advocated. In such an investigation Quine's metaphorical talk of the norms of method as engineering, or as 'efficacy for an ulterior end', or as the technology of truth-seeking or of prediction, needs clarification.[[41]](#endnote-41) Concerning methodology, much clarification has already been done by Larry Laudan in his theory of Normative Naturalism (NN) — the next meta-methodology to be investigated.

 The account of rules and values given in §3 accords with that of Laudan who says that methodological rules of the sort 'one ought to do what rule r says' are elliptical and omit reference to epistemic values which are an important, though often unmentioned, axiological aspect of the epistemological context in which rules are to be applied. When values are mentioned explicitly then we have principles of instrumental rationality, or r-v hypothetical imperatives:

(P) if one's cognitive goal is v then one ought to follow r.

Laudan claims that such hypothetical imperatives are warranted if the following universal declarative sentence, or hypothesis, H, is true:

(H) following rule r realises goal v.

A comparative statistical formulation would be:

(H') if one follows rule r then one more frequently realises goal v than by following any alternative r' to r.

These two kinds of r-v hypotheses will be collectively referred to as 'Hs'.

 Claims like (P) are imperatives within some SM. In contrast the Hs are empirical hypotheses about the strategies scientists employ to achieve (or not as the case may be) their scientific values. They are means-ends hypotheses of an instrumental methodology. They can be viewed as sociological hypotheses about a range of actions of scientists and the frequency with which their following particular rules realises particular values. As such the collection of Hs constitute an empirical science about the best strategies we have adopted in constructing the historical sequence of other theories about the world. Thus in parallel to the sciences, there is a history of the development of Hs over time, i.e., a history of the development of our SMs. Moreover the Hs can, like the hypotheses of any science, be proposed, confirmed, rejected and modified in the course of the progressive development of the means-ends science. It is this feature of SMs as a set of Hs that is the focus of NN.

 The Hs, we might say, are naturalised versions of principles of SMs. But if we naturalise all of them we have eliminated appeal to any principles of method to decide between the Hs of our ends-means science. One role meta-methodology can have is to provide principles to adjudicate between the various (sets of rival) Hs. To this end Laudan proposes a meta-inductive rule MIR:

If actions of a particular sort, m, have consistently promoted certain cognitive ends, e, in the past, and rival actions, n, have failed to do so, then assume that future actions following the rule 'if your aim is e, you ought to do m' are more likely to promote those ends than actions based on the rule 'if your aim is e, you ought to do n'. (Laudan 1987, p. 25; Laudan 1996, p. 135)

 Sometimes MIR is expressed as an instance of the Straight Rule of enumerative induction, as above, and on other occasions more broadly as a rule about frequencies. Thus Laudan says: 'empirical information about the relative frequencies with which various epistemic means are likely to promote sundry epistemic ends is a crucial desideratum for deciding on the correctness of epistemic rules'. (Laudan 1990a, p. 46) In the case of the latter, we should be alert to the fact that the meta-methodology of NN is more than MIR: it also carries with it the baggage of a statistical methods for the estimation of frequencies. But as will be discussed shortly, many statistical methods have *a priori* aspects; so the meta-methodology of NN cannot be purely empirical in character.

 MIR is the core meta-methodological principle of NN. We may use MIR to adjudicate between rival hypotheses about the efficacy of rule r compared with any rival r' in achieving goal v. To illustrate, Laudan points out that while MIR seems quite minimal, it is strong enough to test some commonly accepted rules of SM against the historical record. On this basis he claims that MIR alone is able to eliminate a number of proposed rules such as: 'if you want theories with high predictive reliability, reject *ad hoc* hypotheses' (Laudan, 1987b, p. 27). Such rules, when supplemented with the value they are supposed to achieve, can be shown to be unreliable with respect to that value. Once a hypothesis H has passed its test using MIR, we can formulate it as a hypothetical imperative of method. We can also add it to MIR thereby bootstrapping our way to further meta-principles for adjudicating between rival hypotheses of the means-ends science of naturalised methodology. The warrant for the methodological principle P (which is a hypothetical imperative) is just the empirical means-end hypothesis H that has passed its test.

 So understood, NN has an important task to perform: given the vast number of methodological principles that scientists and philosophers have proposed, NN advocates an empirically based method, no different from that found in the sciences, for testing just how well the principles perform their task. NN can yield a growing means-ends science of Hs tested by MIR, or MIR supplemented with already confirmed methodological principles; and as this science grows it will in turn yield a body of more refined principles of SM. Importantly NN goes about its task in a manner free from the difficulties that faced the later Popper, Lakatos and the reflective equilibrium principle, discussed in §7, and also Feyerabend (§9). No appeal need be made to intuitions; instead episodes from the historical record of the sciences are investigated to uncover the strategies that best realise values. There are a number of case studies of how particular principles of SM have faired under the regime of NN (see the programme outlined in (Laudan *et. al.* 1986) and some of the results of testing in (Donovan *et. al.* 1992)). However we will focus on questions which arise concerning the meta-methodology of NN.

 The programme of NN depends crucially on a meta-rule of induction thereby tying the justification of each methodological principle to the problem of the justification of induction. This raises a number of issues for NN understood in this way. The first has to do with the actual test procedures that advocates of NN have employed in the literature. It turns out that their procedure is not one of collecting a vast number of historical episodes and then performing an induction over, or performing a statistical analysis of, the episodes. Rather some proposed naturalised methodological hypothesis is tested against the historical record using not MIR but principles of instance confirmation or disconfirmation or, as is openly admitted, the H-D method.[[42]](#endnote-42) Though such a deviation from the strict programme of NN might be allowed in the absence of the much harder work of employing MIR alone, none of the three principles are problem-free and at best can only provisionally be used as meta-principles until the more austere programme with MIR alone is launched. For the H-D method is itself highly contested by probability theorists. And principles of instance conformation face problems such as that of grue or the Raven's Paradox, while instance disconfirmation faces the Quine-Duhem problem. These are significant problems in the theory of scientific rationality for which NN assumes an answer rather than provides one.

 The second issue is that while Laudan maintains that the principles of SMs are all hypothetical (or a categorical with suppressed antecedents), the meta-rule MIR is different in that it is categorical. But it can also be viewed as a hypothetical if its suppressed antecedent contains reference to the value of truth. In making ordinary enumerative inductive inferences about scientific or everyday matters what we want is a reliable inference, i.e., one which, given true premises based on what we have observed in the past, enables us to draw a further truth as conclusion, which is either about the next unobserved case or is a generalisation. Thus the goal of ordinary inductive inference , and of MIR, is truth. We also aim for truth, or high probability of truth, when we test the various Hs, the naturalised form of principles of method. That is, we want to know that the hypothesis 'following r always realises value v' is true, or at least has been highly confirmed. Clearly the value of truth plays a crucial role in meta-methodology and in determining the hypotheses of the means-ends science.

 The next set of issues focus on MIR. First, by itself MIR might be quite unable to perform its task of yielding principles of method when confronted with the massive amount of information provided by episodes in the history of science without the assistance of statistical theory. Some sampling method needs to be employed to determine how many historical cases need to be investigated in order to determine, to a specified degree of accuracy, any proposed means-ends hypothesis.[[43]](#endnote-43) Second, principles of enumerative inference, such as MIR, also obscure two important aspects of inductive inference that Hempel (1981) invites us to separate out. The first is matters to do with rules for determining the probabilities of hypotheses given evidence; the second, formulating rules of acceptance which would tell us what hypothesis we should accept. Pursuing the second aspect takes us in the direction of the theory of epistemic utility which Hempel had earlier developed, or the decision-theoretical approach of others. In a quite standard version of this theory, suppose we are given the probability of some hypothesis H on evidence E. Then there are two 'actions' a scientist can take: either accept H or reject H. Moreover there are two states of the world: either H is true or H is false. This yields a matrix with four outcomes, each of which is assigned some 'utility' by the scientist. Which action should the scientists perform? One standard answer is to accept that hypothesis with the greatest expected utility. Thus problems to do with inductive inference land us squarely in issues central to decision theory and the principles on the basis of which rational decisions are to be made.

 What bearing does this have on NN with its meta-principle of MIR? If we were to take as the 'action' following rule r, and if we were to take as the 'state' the realisation of some value v, then providing the scientist can attach utilities to the four outcomes (one of which would be, say, following r but not realising v), then we have a decision-theoretic framework for evaluating principles of method. Viewing matters this way, in the meta-methodology of NN Laudan's MIR has been replaced by decision-theoretic principles. Thus the debate about the status of the meta-methodological principles of NN has been shifted to the debate about the status of the principles of rational decision making which are to be used in determining the more particular principles of the SMs of science. This leaves untouched issues about which principles of SM do, or do not, survive testing. Moreover, even though the materials used in the test procedures are garnered from the historical record of science thereby underlining one way in which NN is empirical, the meta-methodological principles of decision theory by which the test procedures are carried out have a distinctly non-empirical character. Thus NN cannot be, through and through, an empirical science; it is also conceptual, as will be seen at the beginning of the next section.[[44]](#endnote-44), [[45]](#endnote-45)

12. PRAGMATISM AND METHODOLOGY

Some approaches to meta-methodology may be characterised as 'pragmatist', though what is intended by that term is not always easy to pin down. If pragmatism is at least characterised as denying that there is a first philosophy which has *a priori* or analytic modes of justification, then Quine's naturalistic account of methodology can be said to be a variety of pragmatism. Laudan endorses such a meta-epistemological claim adding that epistemology, with its methodological principles, is neither a synthetic *a priori* subject, nor is it conventionalist as the early Popper suggested (Laudan 1996, p. 155). For NN, epistemology's hypotheses about inquiry are to be judged like any other hypotheses of science. However Laudan points out that science has both conceptual and empirical elements; and since methodology is to be construed along the lines of a means-ends science, then it too will have both conceptual and empirical elements. In comparing the science of methodology with physics Laudan says: 'Both make extensive use of conceptual analysis as well as empirical methods' (*ibid*., p. 160). Thus the naturalists' science of methodology will not be wholly empirical but will be in part non-empirical. If so, room will have to be made for a role for conceptual analysis while rejecting the idea of a synthetic *a priori* first philosophy.

 In admitting that there are conceptual elements to meta-methodology, Laudan is not so hard-line a pragmatist as Quine. Nor is he so parsimonious as Quine who mentions few goals for science beyond truth and prediction. Though Laudan is a critic of theoretical truth as an unrealisable goal of science (Laudan 1984, chapter 5), he is broadly pluralistic in admitting a wide range of goals for science, both currently and in the past. In so far as pragmatism is characterised as a pluralism with respect to our epistemic and cognitive goals Laudan is a pragmatist. On a third characterisation of pragmatism in terms of the actual practical success of science, Laudan is also a pragmatist since he requires that methods be judged by their production of successful science. On this broad criterion, the role that Feyerabend assigns to successful practice in assessing methods might make him a pragmatist of sorts; but not obviously the later Popper or Lakatos who base their empirical view of science not on actual practice but the judgements of a scientific élite about actual practice. One philosopher who puts emphasis on the role of practice in meta-methodological considerations is Rescher - to whom we now turn.

 Rescher has developed a systematic account of human knowledge, which eclectically combines elements of scepticism, realism, naturalism, pragmatism and ideas from evolutionary theory. As against the classical pragmatists who construed the truth of individual claims in terms of practical utility, he develops a methodological variant of pragmatism according to which methodological rules are justified by their success in practical application. Rescher accepts a correspondence theory of truth and rejects the classical pragmatist conception of truth applied at the level of individual theses, i.e., propositions; this he calls '*thesis* pragmatism'.[[46]](#endnote-46) This is contrasted with *methodological* pragmatism, which places the locus of pragmatic justification on methodological rules. For Rescher propositional theses about the world are correspondence true or false; and they are justified, not pragmatically, but on the basis of methodological rules. However it is these rules, and not the propositional theses about the world that they help generate, that are justified by successful practical application.

 Rescher's methodological pragmatism has much in common with naturalism because it treats methodological principles as subject to empirical evaluation, and hence provides a naturalised account of epistemic warrant. However Rescher recognises a fundamental problem concerning the justification of principles of method which he expresses in terms of the Pyrrhonian sceptic's problem of the *criterion* or *'diallelus*', i.e., circle or wheel (see Rescher 1977, chapter II §2). This is a problem we have met in §4, viz., that there is no direct or method-independent way of determining that use of a methodological rule yields its goal. Either we use principle M, in which case M presupposes its own correctness, or we use another principle M', in which case there is an infinite regress of principles. We need to see in what way the pragmatist's appeal to practice might overcome this problem.

 Rescher conceives of methodological rules as instruments, as tools for the realisation of some end. So understood they are in conformity with the account of rules, and the values that the rules are supposed to realise, given in §3 and §11. Rescher also requires rules be evaluated in the same way that instruments are evaluated viz., pragmatically. That is, we need to show that instruments "work" in that they produce some desired end (the end presumably being a theory which exemplifies some desired epistemic value). However it is not sufficient for a rule to produce its end once, or a few times. Rescher requires that rules regularly realise their ends: 'the instrumental justification of method is inevitably general and, as it were, statistical in its bearing' (*ibid*., p. 5). Such a requirement is nothing other than Laudan's more explicit account of NN with its appeal to meta-induction or statistical inference. Also akin to NN is Rescher's use of such an 'inductivist pragmatic' method for comparing how well two or more rules realise the same end, or how rules can be refined so as to more regularly realise some end, or how principles once established can be used to supplement the inductivist pragmatic meta-methodology. Rescher, like Laudan, draws out an aspect of the naturalist view of method in which methodological principles of the means-end science, like the hypotheses of any other science, can be revised and improved in the process of examining their degree of pragmatic success.

 Induction plays an important role in both the meta-theory of Laudan's NN and Rescher's methodological pragmatism. Rescher extends his pragmatism in an attempt to resolve the problem of the justification of induction (see for example Rescher 1973, chapter 2 and Rescher 1992, chapter 9). We will not pursue the pragmatic justification of induction here. However we will focus on another aspect of Rescher's position that distances his pragmatism from Laudan's NN in that it employs an argument that Laudan finds inadequate on the grounds of NN's own test procedures, viz., a modified version of Inference to the Best Explanation (IBE) which links the success of science produced by the application of methodological principles to the truth-indicativeness of the principles themselves. A similar argument using a simple but direct version of IBE was set out in §2 to show the superiority of an appeal to methodological principles over non-cognitive interests as an explanation of the instrumental success of the historical sequence of scientific theories. Here some of the promissory notes on which that argument was based will be cashed.

 To connect practical success with truth, Rescher deploys a number of broad metaphysical presuppositions which we might refine or abandon in the course of inquiry but which, at the moment, we hold of ourselves and the world in which we act as agents. They are as follows. (a) Activism: we must act in order to survive, hence beliefs have practical relevance since they lead to actions which have consequences. (b) Reasonableness: we act on the basis of our beliefs so that there is a systematic coordination of beliefs, needs and action. (c) Interactionism: humans actively intervene in the natural world and are responsive to feedback from this intervention in both cases of satisfaction and frustration. (d) Uniformity of nature: continued use of a method presupposes a certain constancy in the world. (e) Non-conspiratorial nature of reality: the world is indifferent in the sense that it is neither set to conform to nor set to conflict with our actions. On the broad metaphysical picture that emerges, the practical success of our belief-based actions, and the theoretical success of our beliefs, both turn on the correctness of assumptions like (a) to (e). They set out general but contingent presuppositions about the sort of agents we are and the sort of world in which we act and believe (both individually and communally) and the fact that we have achieved a wide degree of success in action and belief.

 Consider now principles of method that govern inquiry. They are not merely locally applicable but also apply across a very broad front of inquiry into a wide diversity of matters. They also have a large measure of success in that rules regularly deliver beliefs that instantiate our desired practical goals and theoretical values. Roughly, the idea is that our methods might mislead us some of the time, but it is utterly implausible to hold that most of the time they systematically lead us astray, given our success in the world and the way the world is as presupposed in the general metaphysical picture above. On the basis of this success, what particular epistemic property might we attribute to our methodological principles? Rescher sometimes speaks of this as the 'legitimation problem' for our methods (Rescher 1977, p. 14). A number of terms could be used to name the property possessed by our methods that leads to their success, such as 'validation', 'truthfulness' and 'adequacy'; Rescher uses these terms in various contexts along with 'truth-correlative' and 'truth-indicative' (see Rescher 1977, chapter 6, especially p. 81 and p. 83). We will settle on the last of these and summarise Rescher's non-deductive plausibility argument as one which shows that the success of our methods is evidence that our methods are truth-indicative.

 The argument for this position has much in common with the use of IBE to argue for scientific realism. However Rescher refers to it as a '"deduction" (in the Kant's sense)' (*ibid*., p.92). But the argument is not strictly deductive (as the shudder quotes allow), but rather an inductive plausibility argument to be distinguished from certain forms of IBE that he wishes to reject. IBE as used by realists is an argument from the instrumental success of our theories to the truth of the theories. Given both realist and various rival non-realist interpretations of some theory, it is alleged that realism offers a much better explanation of, or makes more probable, the theory's instrumental success than any of its rival interpretations (some of which offer no explanation at all). Rescher does not endorse this form of IBE with its strong conclusion that theories are true, or even that they have high truth-likeness. Rather he prefers to say:

The most we can claim is that the inadequacies of our theories — whatever they may ultimately prove to be — are not such as to manifest themselves within the (inevitably limited) range of phenomena that lie within our observational horizons. …

In general, the most plausible inference from the successful implementation of our factual beliefs is not that they are right, but merely that they (fortunately) do not go wrong in ways that preclude the realization of success within the applicative range of the particular contexts in which we are able to operate at the time at which we adopt them. (Rescher 1987, p. 69)

 Let us transfer these considerations to the role that methodological principles play in inquiry. First expand the notion of the 'success' of our theories from merely their instrumental success in pure inquiry to include their success in enabling the domain of human action to be vastly increased (for example, science has investigated the properties of new substances, and this has resulted in new technology such as our having non-stick fry pans with which to cook). And let us shift focus from whether scientific theories are true to what epistemic properties our principles of method must possess if they are to yield, as the result of their application in inquiry, pragmatically successful theories (both practically and cognitively). What we want are principles which not merely yield successful theory on a few occasions, but quite generally; and even though these principles might sometimes fail us, they are not thereby totally invalidated. That is, we want principles which have a high degree of reliability across many sciences and yield success in practical and theoretical contexts while being defeasible. Presenting Rescher's argument this way shows it to be an inductive argument, akin to IBE, from the wide-ranging practical and cognitive success of our theories, to the best explanation of that success, viz., the methodological principles themselves having not gone massively wrong in their application, or having not manifested whatever inadequacy they might possess. This feature we might call the 'truth-indicativeness' of methodological principles. But they are only indicative because they are open to revision and modification in the course of growing inquiry conducted by agents such as ourselves in a world such as the one we inhabit.

 Some such modified version of IBE lies at the core of Rescher's meta-methodology of 'methodological pragmatism'. What justificatory basis is there for this form of argument? Rescher's response to the question is to see the re-emergence of the diallelus, or great circle or wheel of justifications (Rescher 1997, chapter 7). Characteristically for a pragmatist, there is no independent standpoint from which a justification can be given for IBE, or any other rock-bottom meta-methodological principle. Rather the same form of argument just given above is used at all points to: (i) validate scientific theories in terms of their success in practical and theoretical application; (ii) validate principles of inquiry (i.e., the level 2 SMs) in terms of their success in yielding the successful science of (i); (iii) validate the metaphysical picture of the world which sets out the presuppositions we must make about the sort of world in which we live and the sort of agents we are that makes such success in (i) and (ii) possible. Some such pragmatist picture, Rescher alleges, closes the circle without making it necessarily vicious. But the pragmatic picture depends on some plausibility argument, or some version of IBE, which is used at different levels and which closes the circle. If the above account of Rescher's argument is correct, it is one which is highly contested by fellow naturalists such as Laudan who restricts his meta-methodology to induction and who explicitly rejects the use of any form of IBE or plausibility argument (Laudan 1984, chapter 5). For those who find that some forms of IBE can be satisfactorily used, this helps draw another distinction between the kinds of pragmatism that Laudan and Rescher espouse.[[47]](#endnote-47)

13. BAYESIANISM AND SCIENTIFIC METHOD

The view that scientific reasoning, and scientific method more generally, are to be understood in probabilistic terms has recently been given a great deal of attention by its many adherents and is well served by a burgeoning literature.[[48]](#endnote-48) The scope of this book is to explore non-Bayesian approaches to methodology. However in a survey such as this it will not be amiss to review Bayesianism, however briefly, in terms the framework developed here. Bayesianism is not a unified position, all aspects of which are agreed to by all parties. A brief outline will be given of a standard form of subjective Bayesianism called here 'orthodox Bayesianism', or 'OB' for short. Then we will sketch briefly the position of two philosophers, Shimony and van Fraassen, who, in quite different ways, deviate from OB. Moreover Bayesianism is not without its critics, one of the more important works in this area being Mayo 1996. Papers in this book which deal with methodology in relation to Bayesianism include those by Fox and Worrall.

 The central core of OB involves appeal to the axioms of the probability calculus and the several forms of Bayes' Theorem which follow from these axioms. The various forms of this Theorem provide an account of how probable hypotheses are on given evidence, how rival hypotheses can be compared with respect to (growing) evidence, and how the same hypothesis fairs with respect to different bits of evidence over time. It also provides illumination concerning such matters as the variety of evidence, the Quine-Duhem problem and the various paradoxes that beset theories of confirmation (such as 'grue', the 'raven's paradox, irrelevant tacking paradoxes, etc). OB also comes equipped with a number of powerful theorems, such as those which inject a degree of objectivism (or intersubjectivism) through 'convergence of opinion' proofs; these show the conditions under which different persons with widely different initial degrees of belief in some hypothesis can ultimately converge in their relative degree of belief in that hypothesis given evidence, as the evidence comes in over time.

 How are probabilities to be understood? By far the most commonly accepted view is that of subjectivism, or personalism, in which probabilities are understood to represent rational degrees of belief on the part of persons. Also central to OB is a principle of updating probabilities in the light of new evidence, the simplest of which is a rule of strict conditionalisation. Understanding the probabilities subjectively, this rule says: if a person acquires evidence E with certainty, then for some hypothesis H the old and new degrees of belief are related as follows: pnew(H) = pold(H, E).[[49]](#endnote-49) The above provides a sufficient basis for a wide-ranging theory of confirmation, of the sort set out in Earman (1992). Bayesianism also provides a basis for decision-theoretic approaches to the foundations of scientific inference; this is explored in Savage (1954), Jeffrey (1983) and Maher (1993).

 How good an account does OB give of scientific inference? Howson and Urbach explore the connections OB has with the general theory of statistical inference, thereby displaying one advantage it possesses over many other theories of scientific method (outlined in previous sections) which do not always link readily to theories in statistics. Earman also claims that, for all the problems he finds with Bayesianism, it 'provide[s] the best hope for a comprehensive and unified treatment of induction, confirmation and scientific inference' (Earman, 1992, p. xi). Further, Salmon argues that, as far as Kuhn's theory of weighted values is concerned, it is possible to give an account of its methodological prescriptions entirely in terms of Bayes' Theorem (Salmon 1990; however see the paper by Worrall in this volume). Earlier Salmon had given his account of how Bayesianism fairs with respect to some of its other rivals, such as Popper's theory (Salmon 1967), a matter which Howson and Urbach explore even more fully.

 Such comparisons of OB with rival theories of method concerns the virtues of level 2 theories of SM. In judging between such rival SMs, appeal is made to meta-methodological criteria such as greater comprehensiveness, ability to deal with long-standing problems in confirmation theory, and coherence with other mathematical theories (e.g., the advantageous links Bayesianism has to statistical theory). A positive comparative judgement in favour of Bayesianism is made by its adherents despite some of its acknowledged inadequacies (see Earman 1992 chapters 5, 9 and 10). Bayesians also go meta-methodological when they give reasons for why a person's degrees of belief should conform to the probability calculus. Several kinds of justification have been proposed, the most common kind being based on 'Dutch Book' considerations. Ramsey and de Finnetti proposed that there is a connection between degrees of belief and betting behaviour, and used this to justify the conformity of degrees of belief to the axioms of the probability calculus. Commonly in a 'Dutch Book' one makes a set of bets such that whatever the circumstances one cannot win. Thus 'Dutch Book' arguments attempt to show that if one's degrees of belief do not conform to the axioms of the probability calculus then a Dutch book can be made against one in that one's bets on the truth or falsity of hypotheses is such that one can never win. The converse claim is also important, viz., if one's beliefs do conform to the axioms then a Dutch Book cannot be made. These important meta-considerations, which have to do with coherence or consistency conditions for assignments of degrees of belief, have attracted considerable comment but not complete agreement on all details.[[50]](#endnote-50)

 The further elaboration and defence of the various Bayesian approaches cannot be made here. Instead mention will be made of two widely differing ways in which some have thought that OB is in need of supplementation, the first being due to Shimony, and the second, only briefly sketched, due to van Fraassen. In a paper[[51]](#endnote-51) which widely ranges over theories of probabilistic method, Shimony set out his version of what he called 'tempered personalism' within the context of a naturalistic view in which we, as beings in the natural world, are capable of reasoning about, and investigating, that world. Shimony argues for at least the elements of the OB position, as already set out above, against other probabilistic approaches to scientific method. But he makes some significant additions; the one discussed here is his 'tempering' condition.

 Despite the 'convergence of opinion' theorems, Shimony fears that OB allows people to set their prior degree of belief in some hypothesis at, or close to, the extremes of 0 and 1 so that convergence might not take place in their lifetime of evidence gathering, or converge not at all. The issues of the constraints to be placed on prior probabilities, and the 'swamping of priors', is an important one for Bayesians (see Earman 1992, chapter 6). In order to avoid excessive dogmatism or scepticism on the one hand and excessive credulity on the other, and to encourage a genuinely open-minded approach to all hypotheses that have been seriously proposed, including the catch-all hypothesis,[[52]](#endnote-52) Shimony proposes that the radically subjectivist personalist approach to Bayesianism be tempered by adding the following condition (also suggested by others such as Harold Jeffreys).

Tempering condition (TC): 'the prior probability … of each seriously proposed hypothesis must be sufficiently high to allow the possibility that it will be preferred to all rival, seriously proposed hypotheses as a result of the envisaged observations' (Shimony 1993, p. 205).

Shimony also adds that tempered personalism is a contribution towards a 'social theory of inductive logic' of the sort envisaged by Peirce, in contrast to the individualism of the untempered subjectivist interpretation, in that TC applies to all inquirers and all the hypotheses they entertain.

 Shimony's TC is a substantive methodological supplement to subjective Bayesian methodological principles (but one that has received little endorsement from strict observers of the tenets of OB). Shimony recognises that there is vagueness in what is meant by 'seriously proposed hypothesis' and that, since there is no way of establishing TC using the axioms of probability, there is an issue about its status. Initially Shimony though that there might be an *a priori* justification for TC. However in a searching analysis of the what TC might mean and what status it might have, Teller shows that there is no *a priori* justification for TC available; nor do other pragmatic justifications work (see Teller (1975) for these arguments). However for Teller, and now for Shimony, the negative conclusion is not problematic since they now advocate empirical rather than *a priori* justifications of principles of scientific method. They also suggest that 'the process of subjecting a method of scientific inference to scientific investigation' (Teller 1975, p.201) needs to be set in the context of meta-methodological investigations of the sort suggested in §7 (Goodman's reflective equilibrium) or §11 (Quinean or other naturalisms).

 Shimony in his 'Reconsiderations' of his earlier paper which advocated TC, proposes four further principles of methodology 'chosen to expedite the machinery of Bayesian probability' (Shimony 1993, p. 286; for the principles see sections V and VI); only one principle will be mentioned here. The four principles are proposed as contingent claims which can be shown to fail in some possible worlds; importantly they are alleged to be true of our world and to have some empirical support. In setting out some conditions for the possibility of inquiry for Bayesian agents in a world like ours, they fulfil a similar role to the general metaphysical principles presupposed by Rescher's pragmatic meta-methodology, even though the paint a somewhat different metaphysical picture from that of Rescher. But they also suggest substantive principles of method. Thus Shimony's Principle 2 says: 'A hypothesis that leads to strikingly successful empirical predictions is usually assimilable within a moderate time interval to the class of hypotheses that offer "understanding", possibly by an extension of the concept of understanding beyond earlier prevalent standards' (*ibid*., 287). Unfortunately this principle is expressed as a descriptive claim rather than a norm, though its transformation to a norm is fairly obvious. As an example Shimony has in mind the prediction of the Balmer series from Bohr's early 1913 theory of the atom. Given this surprising prediction, on Shimony's Principle 2 Bohr's theory ought to be assimilated in whatever way into the prized circle of hypotheses that offer "understanding". Any further discussion of Shimony's principles would have to look into the way in which they allegedly expedite Bayesian machinery, or whether Bayesian methods can readily accommodate such a recommendation and that it is otiose. It also needs to look at whether the four principles are all the contingent empirical principles that a fully fledged methodology requires to do justice to our scientific practices.

 Another quite different set of modifications to strictly minimal orthodox subjective Bayesianism has been proposed by van Fraassen. His position is finely nuanced with respect to OB, and can be best grasped by considering his own characterisation of traditional sceptical epistemology in terms of the following four theses:[[53]](#endnote-53)

(I) there can be no independent justification to believe what tradition and ourselves of yesterday tell us (i.e., what we find we already believe);

(II) it is irrational to hold unjustified opinion;

(III) there is no independent justification for any *ampliative* extrapolation of the evidence plus previous opinion to the future;

(IV) it is irrational to make such ampliative extrapolations without justification.

Theses (I) and (II) concern belief, with the justification and rationality of belief set out as separate theses. Theses (III) and (IV) concern a special subclass of our beliefs, viz., our ampliative inferences, with the justification and rationality of ampliative inferences and their deliverances set out as separate theses.

 It is possible, with a little gentle massaging, to present three other epistemological positions in terms of whether they accept or reject theses (I) to (IV) of the sceptics' epistemology. Let us call 'Traditional Epistemology' (Trad.E) the view that there are justifiable ampliative rules of inductive inference, including even inference to the best explanation; thus Trad.E rejects the sceptic's (III) which holds that, even though there might be ampliative rules, they lack any independent justification. In rejecting (III) traditionalists can then take on board (IV) concerning the rationality of ampliative inference and its deliverances. They also reject (I) in that they hold that all beliefs are open to justification given their rules. That is, traditionalists think that all beliefs are justifiable and that we have at hand all the justifiable ampliative inferences needed to carry out the job of justification. They can now accept (II) since they are armed with the sufficient justifiable beliefs (due to the rejection of (I)) and sufficient justifiable ampliative inferences (due to the rejection of (III)). In sum, Trad.E accepts (II) and (IV) while rejecting (I) and (III).

 The position of Orthodox Bayesianism (OB) can be characterised as follows. It agrees with (I) but rejects (II). In accepting (I) OB takes on board what beliefs we currently have and works from there, regardless of what independent justification they may or may not have. But OB rejects (II). Within the context of OB this just means that we are free to assign any prior probability to hypotheses we like and that we are not irrational in so doing; all that is required is that we meet the coherence requirement for the distribution of degrees of belief in so freely distributing. Further, since conditionalisation is not strictly a kind of ampliative inference and is one of the central characteristics of being a Bayesian, then OB, on van Fraassen's characterisation, accepts (III) and (IV) thereby underlining the emphasis on learning from experience and past opinion.

 What of the 'New Epistemology' (NE) advocated by van Fraassen? He characterises NE as adopting (I) and (III) but rejecting (II) and (IV). That is, we cannot given an independent justification for either our beliefs or our principles of ampliative inference; but, given the standards of rationality he adopts, it is not irrational to either hold unjustified beliefs or employ ampliative inferences. To characterise his position van Fraassen draws a useful comparison between Prussian law, in which everything is forbidden which is not permitted, and English law in which everything is permitted that is not explicitly forbidden. The Prussian position of Trad.E is well expressed by Bertrand Russell who said: 'Granted that we are certain of our own sense-data, have we any reason for regarding them as signs for anything else …?' (van Fraassen 1989, pp. 170-1). This is to be compared with the English position in which '*rationality is only bridled irrationality*' and 'what it is rational to believe includes anything that one is not rationally compelled to disbelieve' (*ibid*., pp. 171-2).

 The position of van Fraassen's NE with respect to its Trad.E rival is clear. But what of the contrast between NE and OB? Both accept (I) and (III) and reject (II). That is, both NE and OB join hands with the sceptic in holding that nothing, neither beliefs nor ampliative inferences, can be justified in the way Trad.E requires; and they both part company with the sceptic in rejecting the claim that it is irrational to maintain an unjustified belief. However the difference between NE and OB is that NE rejects (IV) while OB accepts (IV). That is, for NE we are permitted to amplify belief even when ampliative inferences are unjustifiable; but for OB this is not permitted and is irrational. In rejecting (IV), the permissive 'English' position of van Fraassen allows one to perform ampliative inferences that lack justification without incurring any epistemic censure for so doing. That is, it is rationally acceptable to believe anything without justification unless one is rationally compelled to disbelieve it. In contrast OB is more restrictive in that it supports Trad.E in rejecting as irrational belief in any claim that transcends the deliverances of ampliative inference.

 Van Fraassen's NE is liberal permissive 1960's epistemology while OB wants to claw back some "law 'n order" in epistemology, but not as much as the regimented adherent to Trad.E. Note that both NE and OB are permissive (on the whole) with respect to what we already believe (both accept (I)). And neither want to reject any of these beliefs on the grounds that, because they lack independent justification of the sort required by Trad.E, they are irrational (both reject (II)). For OB rejecting (II) entails freedom with respect to the assignment of any priors to our beliefs (modulo the coherence requirement). But NE and OB do part company over (IV), one illustrative reason being as follows.

 Central to OB are the 'convergence-of-opinion' theorems which say that, even on quite divergent assignment of priors (excluding 1 and 0) to hypotheses, the probabilities given evidence will converge upon continued conditionalisation as that evidence comes in over time. This suggests that Bayesian inquirers should not end up with divergent and irreconcilable beliefs on pain of irrationality; hence a rationale for OB's adherence to (IV). But in rejecting (IV) NE is at odds with OB over the issue of the rationality of divergent irreconcilable beliefs. For OB, if inquirers arrive at divergent irreconcilable beliefs, then someone has been epistemically misbehaving and is to be censured. For NE, that divergent and irreconcilable beliefs have been arrived at by the gentle sway of the rules of NE's permissive epistemology is not necessarily a sign of irrationality, nor a sign of epistemic misbehaviour which is to be censured. Inquirers can have divergent irreconcilable beliefs without epistemic fault. This is one of the lessons to be drawn from the notion of the underdetermination of theory by evidence and the rules whereby theory and evidence are to be assessed.[[54]](#endnote-54)

 There are many further considerations surrounding the issues just broached concerning NE which involve van Fraassen's principle of Reflection, the rule of conditionalisation and the role of dynamic Dutch Book arguments. However the above will suffice as an account of one aspect of the nicely differentiated position van Fraassen adopts with respect to OB in opting for NE in its place. His position can be summed as follows, with a contrast drawn between his position not only with respect to an adherent of OB who looks to unique outcomes from their ampliative inferences but also relativists:

Like the orthodox Bayesian, though not to the same extent, I regard it as rational and normal to rely on our previous opinion. But I do not have the belief that any rational epistemic progress, in response to the same experience, would have led to our actual opinion as its unique outcome. Relativists light happily upon this, in full agreement. But then they present it as a reason to discount our opinion so far, and to grant it no weight. For surely (they argue) it is an effective critique of present conclusions to show that by equally rational means we could have arrived at their contraries?

I do not think it is. …

So I reject this reasoning that so often supports relativism. But because I have rejected it without retreat to a pretence of secure foundations, the relativist may think that I still end up on his or her side. That is a mistake. Just because rationality is a concept of permission rather than compulsion, and it does not place us under sway of substantive rules, it may be tempting to think that 'anything goes'. But this is not so. (van Fraassen, 1989, pp. 179-80)

14. CONCLUSION

The word 'Selective' in the title is deliberate. There is no way, short of writing a whole book, that could even cursorily mention all those who have made a contribution in support of, or against, the idea of scientific method during the last half-century. Our focus has been on only some authors while others have only been mentioned in passing or not at all. One important omission is any mention of the theory of reliable inquiry which arises out of formal learning theory (See Kelly 1996). However the volume as a whole compensates for this in that one of the workers in the field, Kevin Kelly, has a paper which investigates this approach in relation to normative naturalism.

 Other omissions at least include the following: the application of belief revision theories to problems of scientific method; a fuller discussion of the ways in which Bayesianism might be expounded and/or challenged; the work of statisticians such as Fisher, Neyman, Pearson, Jeffreys amongst others that relates to issues in methodology; recent work in AI on modes of inference relevant to methodological matters; and so on. In addition, since the 'Selective Survey' is focused largely on meta-methodological issues, there is not much discussion of particular principles of scientific method such as inference to the best explanation and the like. Nor is there a discussion of particular methodological issues that arise in connection with testing causal claims, the postulation of intervening variables, and so on.

 However in focusing on issues in methodology that arise out of Popper, Kuhn and Feyerabend and some of the new approaches to methodology that have developed recently, there is more than enough for one book. What the editors hope to show is that, given some of the anti-methodology trends in philosophy of science which either follow the sociologists of science or postmodernism with its 'incredulity towards metanarratives', there is still much life in the discipline of methodology. Importantly the 'Selective Survey' attempts to put some of the pro- and anti- methodology camps into an overall framework of the sort set out in §2 in order to show that each occupies some part of the logical space of possible positions, and then to reveal possible lines critical evaluation of each.

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1. Laudan is one methodologist who argues that there are principles of method, which we can suppose can be used to demarcate science, but who argues against several other ways of drawing any demarcation criterion; see Laudan (1983) 'The Demise of the Demarcation Problem'; this is reprinted in Laudan 1996, chapter 11. [↑](#endnote-ref-1)
2. Earlier advocates of the three positions would be the inductivist Reichenbach (1949), the probabilist Carnap (1962) and the subjectivist Bayesian Savage (1952). [↑](#endnote-ref-2)
3. By far the greatest number of contemporary heirs to the 'inductivist'/'probabilistic' approach are Bayesian. There are a few objectivist Bayesians such as Rosenkranz (1977), but by far the majority are subjectivist Bayesians such as Jeffrey (1983), Earman (1992) and Howson and Urbach (1993). [↑](#endnote-ref-3)
4. Such a position is adopted in Earman (1992) chapter 8 section 7. He cautions against the idea of 'The Methodology of Science' of the sort advocated by Popper and Lakatos; he also endorses the Feyerabendian position that there is no such 'Methodology' (but not necessarily for Feyerabend's reasons) and leaves methodological advice of the sort Methodologists might offer to scientists to actions chosen on the basis of maximum expected utility. [↑](#endnote-ref-4)
5. Pertinent here are issues to do with the 'personal equation' of astronomers often discussed in texts concerning the measurements with optical telescopes to determine the position of a heavenly body and a given time. Under most conditions all observers make systematic misobservations of the time; they are systematically too late. This is an astronomer's 'personal equation' which has to be determined so that recorded temporal observations can be corrected to give true temporal observations. [↑](#endnote-ref-5)
6. Sometimes these three terms are given different senses for various purposes; here they will be treated interchangeably. [↑](#endnote-ref-6)
7. For a useful introduction and survey of some of the literature on science decision-making see Shrader-Frechette (1991). [↑](#endnote-ref-7)
8. The view that there is no formal probability logic of the sort developed for deductive logic is widely held, this being in part one of the lessons to be drawn from Goodman's 'grue' paradox. However the Popperian view that in science we can get by only with deductive logic and no probability logic of any sort is an additional claim which is highly contested. A defence of the Popperian position is given in Musgrave (forthcoming). [↑](#endnote-ref-8)
9. Some form of inference to the best explanation, viz., that greater explanatoriness is a guide to truth (or increased truth), is one of the methodological principles used by realists to establish realist claims. Though there are many supporters of the viability of such an inference form, doubts that need to be answered have been raised by Laudan (1984, chapter 5), who argues for its inadequacy on historical grounds, and van Fraassen (1989, chapter 6) who argues that it leads to incoherence. [↑](#endnote-ref-9)
10. Aristotle's theory has aspects which accord with quite recent accounts of the function that dreams might play in our physiology: see Gallop 1990. [↑](#endnote-ref-10)
11. Accounts of the history of theories of method outside the survey period adopted here, can be found, for example, in Losee (1993), Laudan (1981), Oldroyd (1986) Blake, Ducasse and Madden (1960) and Gower (1997). [↑](#endnote-ref-11)
12. In Donovan *et. al.* (1992) some of these theses are tested against actual pairs of scientific theories such as: the emergence of the theory of plate-tectonics against the background of its rivals, the theories of Ampère and Biot in electrodynamics and rival theories about nuclear magnetic resonance. See the papers by Frankel, Hoffman and Zandvoort. The upshot was that theories are expected to solve some, but not all, of the problems not solved by their rivals or predecessors. Theories are not required to solve all the problems solved by their predecessors; some loss of problem-solving power is allowed to occur in the transition from one theory to another. The first claim is not surprising but the second claim might well be. Methodologists have often claimed that scientists do not, as a matter of fact, accept theories with less content than their predecessors and have also proposed rules prohibiting the acceptance of such theories. Such a rule would seem appropriate if the goal of science is to maximise the content of our theories. But would it be unwise to adopt the strategy prescribed by such a rule if science is to yield new exciting theories? What the above research suggests is that for some episodes in science this goal and its associated rule have not been adopted. Is such behaviour by scientists acceptable, or are they methodologically misbehaving? [↑](#endnote-ref-12)
13. In what follows capitals ‘R’ and ‘V’ stand, respectively, for sets of rules and values while lower case ‘r’ and ‘v’, with or without subscripts or superscripts, are particular rules and values. [↑](#endnote-ref-13)
14. There is no agreed terminology in the literature. What we have called 'methodological principles' (which contain both a rule and a value) are sometimes called 'methods', or methodological 'rules', or 'standards', etc. On our usage, if reference to a value is suppressed then principles will be truncated to rules. [↑](#endnote-ref-14)
15. Perhaps Feyerabend would not grant this and would argue that his proliferation principle ought to also hold at the meta-level because proliferation of inconsistent meta-theories is necessary for meta-theoretical advance. Also advocates of para-consistent logics might find adopting a consistency principle at the meta-level is too conservative since they can provide resources for coping with inconsistency. [↑](#endnote-ref-15)
16. The conception of rules, values and principles developed in this section has been influenced by that found in Laudan 1996 chapter 7. The idea of meta-methodology in this and the next section has also been influenced by Laudan *op. cit*., as well as Hempel 1983 where a distinction is drawn between meta-methodologies which are rationalist in that they have *a priori* elements, and those which are pragmatic and naturalistic in that they emphasise empirical elements. The idea of a meta-methodology can be found in the early Popper; it come into its own in the work of Lakatos. [↑](#endnote-ref-16)
17. Aspects of a transcendental approach are explored in Buchdahl (1980) and in Albert (1984). [↑](#endnote-ref-17)
18. It is often complained that Popper failed to recognise that the Quine-Duhem problem stood in the way of falsification and that there is not the asymmetry between verification and falsification that he alleges. However Popper is at least aware of the Quine-Duhem problem and in Popper (1957) §29 footnote 2 attempts to address it by proposing ways in which the same hypothesis can be tested in different theoretical contexts. Whether his proposal is entirely successful is another matter. [↑](#endnote-ref-18)
19. See Popper (1959) §22 for the difference between falsifiability and falsification and the extra conditions, such as the existence of corroborated falsifying hypotheses, required for falsification. For Popper falsification is not the simple inconsistency of a single observation report and a test consequence of a hypotheses under test, as illustrated in many introductory texts. However there is a further methodological issue, which Popper acknowledges, of how corroborated falsifying hypotheses pass their test. It is also important to note an important feature of Popper's rule about falsification. Once falsified a theory is no longer a candidate for the truth; but this does not mean that one should not still work on a falsified theory. This point is not always made clear in Popper; but it is explicit in Popper 1974, p. 1009. [↑](#endnote-ref-19)
20. Logically, Popper's hypothetico-deductivism (H-D) has the same character as Ayer's proposal for a Verification Principle (VP). It is well known that all proposed formulations of the VP have been shown to have counterexamples in that any proposition whatever could be shown to pass the VP test. The same applies to the H-D model as a criterion of demarcation for science; any proposition whatever can be shown to be scientific in that it has testable deductive consequences in the presence of necessary auxiliary claims required by the H-D model. For a recent report on the current state of play concerning VP, and thus the H-D model, see Wright (1989). However the H-D model is not all there is to Popper's demarcation criterion. [↑](#endnote-ref-20)
21. Not only are there problems with how such an anti-*ad hoc* rule is to be formulated (Lakatos distinguishes three notions of *ad hoc* in Lakatos 1978, chapter 1), but it is also problematic whether any saving hypothesis can stand in the required increasing content relation given Popper's more formal account of empirical content and degree of falsifiability. This last issue is discussed in Grünbaum (1976). [↑](#endnote-ref-21)
22. These are not the only methodological rules Popper proposes. Elsewhere further rules can be found, such as the rules concerning observation statements (*ibid*., §22) and rules for the testing of probabilistic statements which are otherwise unfalsifiable (*ibid*., §65). [↑](#endnote-ref-22)
23. For a fuller account of Popper's early encounters with conventionalism and his, and Lakatos', more empirical approach to meta-methodological matters (discussed in the next section) see Nola (1987). Hempel (1983, section 5) also discusses the status of Popper's meta-methoology saying that despite appearances it is not merely decisionist but has justificatory aspects which are empirical in character. [↑](#endnote-ref-23)
24. Some rules fully determine what one is to do and leave little or no choice in action such as 'take your hat off on entering a church'. But other rules leave some choice to the actor; thus 'make a contribution to the collection' does leave open how much one gives in obeying the rule. The methodological rule 'choose theories on the basis of simplicity' is more like the latter than the former sort of rule in that it leaves some options open for the scientist. Because of the openness of some rules, and because Kuhn perhaps thinks of rules as being determinate, he chose to talk of values instead. In order to accommodate Kuhn's position, in the talk of rules R and values V above we need to recognise that they may be fully determinate (or nearly so), or they leave open a range of options as to how one is to fulfil them. [↑](#endnote-ref-24)
25. See Kuhn (1970, p. 206) for his rejection of the idea that our theories do approximate to the truth about what is "really there", and that "there is, I think, 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". [↑](#endnote-ref-25)
26. The papers in this collection which deal with Kuhnian themes include those of Pyle, Worrall and Forster. [↑](#endnote-ref-26)
27. A list of methodological principles which Feyerabend at one time endorsed is given in Preston 1997 section 7.5. The Principle of Proliferation is criticised in Laudan 1996, pp. 105-10. [↑](#endnote-ref-27)
28. Feyerabend often talks of 'rules' or 'standards' rather than 'principles'. Given the terminology adopted here principles become rules if all reference to values is omitted. The varying terminology between ourselves and other writers should cause no problems. [↑](#endnote-ref-28)
29. In the 'Preface' to the Revised 1988 edition of *Against Method* Feyerabend again points out: "'anything goes' is not a 'principle' I hold" (p. vii), but which he thinks that Rationalist must hold. He explains that he cannot endorse this claim because: "I do not think that 'principles' can be used and fruitfully discussed outside the concrete research situation they are supposed to affect". This is hardly clear; however it might be taken to be a reference to the contextual character of principles of method. [↑](#endnote-ref-29)
30. Earlier criticisms of Feyerabend's position appeared in reviews of *Against Method* to which Feyerabend replied; many of the replies are collected in Feyerabend 1978, Part Three. For two recent evaluations of Feyerabend's views on particular principles see Laudan 1996 chapter 5 and Preston 1997 chapter 7. [↑](#endnote-ref-30)
31. Popper uses this term, and elaborates his theory of a rational tradition which emerged with the Ancient Greeks and which includes his own critical rationalism, in a paper entitled 'Towards a Rational Theory of Tradition' in Popper 1963. [↑](#endnote-ref-31)
32. An alternative line on the ramifications of a postmodernist approach to science that goes beyond the methodological issues discussed here is Rouse 1996, chapters 2, 3, and 4. Rouse is aware of 'all the analytical deficiencies of Lyotard's essay on the postmodern condition' though he thinks that Lyotard's 'diagnosis … as one of growing "incredulity towards metanarratives" seems quite accurate' (Rouse 1996, p. 56). [↑](#endnote-ref-32)
33. The roots of SP reach back into theories of the sociology of knowledge which have their foundation in the work of Marx. Durkheim's and Mannheim's sociology of knowledge are a more immediate influence. However Mannheim excluded most of science from the scope of the sociology of knowledge while SP extends Mannheim's theory to all of science, thus indicating one sense in which SP is 'strong' and Mannheim's is weak (Bloor calls it the 'teleological model'). Bloor says of Mannheim that his 'nerve failed him when it came to such apparently autonomous subjects such as mathematics and natural science' (Bloor 1991, p. 11). [↑](#endnote-ref-33)
34. We can also incorporate Bloor's fourth tenet about the reflexive application of SP to itself into the scope of CT; however reflexivity will not be discussed here. [↑](#endnote-ref-34)
35. Though there is much in the literature of cognitive psychology on the issue of the biological basis of competence reasoning, we will cite one reference to a philosopher who would question our 'natural 'rationality: Stich (1985). [↑](#endnote-ref-35)
36. Though the matter cannot be discussed here, such community-based notions of correctness raise the following questions: is x correct because the community assents to x?, or does the community assent to x because x is correct? Similar questions were asked in Plato's *Euthyphro*. [↑](#endnote-ref-36)
37. See for example 'The Pseudo-Science of Science? in Laudan 1996 chapter 10. This originally appeared as a symposium with Bloor in *Philosophy of the Social Sciences* **12**, 1982. See also Roth 1987, chapters 7 and 8. The second edition of Bloor 1991 'Afterword' contains Bloor's replies to some of his critics. [↑](#endnote-ref-37)
38. Forman is not specific as to what this belief may be; it varies over beliefs such as not every event has a cause, or that there are objective chances, or that there are statistical laws that are not reducible to non-statistical laws, or that physical systems do not evolve deterministically as classically supposed but rather indeterministically, or that not all laws are exceptionless, and so on. Nothing turns on the specific content of the belief during the period of the rise of Quantum mechanics under investigation. [↑](#endnote-ref-38)
39. Quine's most extensive discussion of such principles can be found in Quine and Ullian 1978. [↑](#endnote-ref-39)
40. For criticisms of the H-D method see Reichenbach 1949 pp. 431-2. Earman 1992 pp. 63-6 discusses a number of difficulties for the H-D method concerning confirmation and the irrelevant tacking paradox. [↑](#endnote-ref-40)
41. For an excellent account of Quine's zig zag path through the issues surrounding norms, naturalism and epistemology, see Haack 1993 chapter 6 and Foley 1994. For a discussion of a different view about the norm/naturalisation issue see the exchange between Hooker 1998 and Siegel 1998. [↑](#endnote-ref-41)
42. In an introduction to a collection of papers which test particular principles of method, Laudan and his co-researchers recognise problems with their test strategy saying:

Some have asked why we choose to test theoretical claims in the literature rather than to inspect the past of science in an attempt to generate some inductive inferences. After all, the hypothetico-deductive method is not the only scientific method and it is not without serious problems. But our reading of the history of methodology suggests that hypothetical methods have been more successful than inductive ones. Add to that … that we see no immediate prospect of inductive generalisations emerging from the historical scholarship of the last couple of decades and it will be clear why we have decided on this as the best course of action. Many of the theorists claims can be couched as universals and hence even single case studies … can bear decisively on them. Further, given enough cases, evidence bearing on even statistical claims can be compounded. (Donovan *et. al.* 1992, pp. 12-3)

 What this passage shows is that those carrying our research in the light of NN can, and do, abandon Laudan's original meta-inductive rule MIR, or its statistical version, and replace it by H-D methods of test. However this must be provisional since these methods employed at the meta-level must themselves be open to test using meta-principles which are generally acceptable, such as MIR. The H-D method has not achieved comparable acceptance amongst methodologists. Moreover the authors recognise that whatever successes the H-D method has, it cannot be used without some peril. See Donovan *et. al.* 1992, p. xiv ff. [↑](#endnote-ref-42)
43. One important sampling method that could be employed goes by the name of 'Monte Carlo methods', an account of which can be found in many texts on statistics. [↑](#endnote-ref-43)
44. In this brief account of NN we have passed over the problem of the status and role of norms within naturalism. For a discussion of some of these issues and a reply to critics see Laudan 1996 chapters 7 and 9. See also the exchange between Hooker 1998 and Siegel 1998. [↑](#endnote-ref-44)
45. Papers in this book which deal with aspects of NN are those of Kelly and Sankey. [↑](#endnote-ref-45)
46. Rescher holds a correspondence view of truth, as far as the definition of truth is concerned. In contrast, as far as the criterion for the recognition of truth is concerned, Rescher is a coherentist. Pragmatic elements enter into his overall position elsewhere, but not in connection with the definition of truth. For a criticism of the extension of pragmatism to the definition of truth, rather than the rules for recognising truth, see Rescher 1977 chapter IV. [↑](#endnote-ref-46)
47. For an extended and sympathetic account of Rescher's pragmatism with respect to science, see Hooker 1995, chapter 4. Hooker also considers Popper, Piaget and naturalism in the same context of his 'systems theory' of reason and knowledge in science. [↑](#endnote-ref-47)
48. There is a considerable literature starting with the work of Ramsey and De Finnetti in the first half of the twentieth century. More recent accounts include Jeffrey 1983, Earman 1992, Howson and Urbach 1993 and Mayer 1993. [↑](#endnote-ref-48)
49. There are more sophisticated forms of updating beliefs, for example where the belief in E is less than certainty; see Jeffrey 1983, chapter 11. [↑](#endnote-ref-49)
50. For considerations largely for and some against synchronic and diachronic forms of the Dutch Book arguments see Earman chapter 2, Howson and Urbach chapters 5 and 6 and Maher chapters 4 and 5. A critical account can be found in Christensen 1991. See also van Fraassen 1999, chapter 7 and the position of van Fraassen 1995, especially p. 9 where he says 'I will explain why I do not want to rely on Dutch Book arguments any more, nor discuss rationality in the terms they set, though they have a very specific heuristic value'. [↑](#endnote-ref-50)
51. The paper 'Scientific Inference' first appeared in a collection in 1970. Reference to the paper will be made to its reprinted version in Shimony 1993; the reprint is followed by a re-assessment of his views called 'Reconsiderations of Inductive Inference'. [↑](#endnote-ref-51)
52. Bayes' Theorem standardly applies to a set of exclusive and exhaustive hypotheses. Thus if one considers hypotheses H1, H2, . . . Hn-1, then one also has to take into account the catch-all hypothesis Hn which is ¬[H1 v H2 v . . . v Hn-1]. The hypotheses one actively considers might not contain the correct hypothesis while the catch-all does at least this, even if it is just Hn which simply says 'neither H1 nor H2 nor . . . nor Hn-1'. [↑](#endnote-ref-52)
53. The four theses are set out in van Fraassen 1989, p. 178. The brief account of van Fraassen given here is indebted to Kukla 1998 chapter 12, especially sections 12.3 to 12.6. Kukla provides a useful evaluation of the position set out in van Fraassen 1989 Part II, chapter 7 'The New Epistemology'. [↑](#endnote-ref-53)
54. It is also one of the grounds on which the dispute between realists and van Fraassen constructive empiricists is held to be both irreconcilable but epistemically blameless. See Kukla 1998, chapter 12.4, pp. 156-7 for considerations as to why this might not be a compelling way of drawing the difference between OB and NE with respect to the realism/anti-realism issue, since advocates of OB need not be as impermissive as van Fraassen suggests. [↑](#endnote-ref-54)