

Can the
basic principles
of rationality
be defended
rationally?

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ABSTRACT

It is argued that if theories of rationality are to avoid logical difficulties (i.e., self referential inconsistencies or logical paradoxes), then these theories require *a minimal dogmatism*, that is, some basic presuppositions that are unjustifiable without intuitions, vicious circularity, or infinite regress. Thus, it is argued that if rationality is characterized as the search for justifications then a comprehensive rationality is in the end an impossible and self-contradictory task; while if rationality is characterized by the criticizability of any position, then a rationality that intends to be comprehensive leads to self-referential logical paradox.

It is also argued that there are logical limits to the justifiability of the most basic scientific methodological rules and aims, that is, it is argued that to justify our various scientific methods and aims *we are required to assume without a bona fide justification (i.e., dogmatically)* some basic cognitive methods and goals.

These conclusions are illustrated by exploring the theories of rationality of the Popperian tradition and the theories of scientific method of various naturalised scientific meta-methodologies, in particular, by critically examining the alleged justification of our scientific methods provided by 'evolutionary epistemology' and Laudan's normative naturalism.

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PREFACE

In this thesis, I explore the logical limits of argument and its consequences for rationality in general and for scientific rationality in particular. The rational attitude is at its most basic a high valuation of argument as a justificatory or critical tool, and this high valuation of argument is something that cannot be rationally defended or criticized without assuming it. Hence, a belief in the rational value of argument has a dogmatic character, if by dogma we understand a presupposition without a non-viciously circular justification.

On the other hand, all argument has some premisses, so that an argumentative justification can lead us into an infinite regress, a regress that can only be stopped circularly or dogmatically. And if probative circular arguments are to be avoided, because of their being viciously circular, then we are left with dogmatism as the only option left to stop the regress. This last claim is the main conclusion of this thesis, that is, the contention that we have to accept dogmatically some basic presuppositions of rationality, in general, and of scientific methodology in particular. The theory of rationality proposed by Popper in his 'Open Society' and the alternative proposal of W. W. Bartley are used to illustrate this unavoidable dogmatism in Chapter I. On the other hand, Chapter II is a general introduction to the naturalised tradition in epistemology and to some of the difficulties inherent in this approach. While the specific naturalised meta-methodologies proposed by 'Evolutionary Epistemology' and by Laudan's 'Normative Naturalism' are discussed in Chapters III to V.

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CHAPTER ONE

IS SCIENTIFIC RATIONALITY LOGICALLY LIMITED?

Human reason has this peculiar fate that in one species of its knowledge it is burdened by questions which, as prescribed by the very nature of reason itself, it is not able to ignore, but which, as transcending all its powers, it is also not able to answer.

(Kant)

The Humean predicament is the human predicament.

(Quine)

Two related problems will be introduced and clarified in this chapter through a historical examination of some influential formulations and answers. This chapter's special attention is given to the formulation and the answers of the Popperian tradition to the two problems at hand. The questions to be examined are fundamental sceptical ones: the radical doubts about the logical possibility of rationally defending rationality itself; of reason being its own guarantor, in particular, I will delve into doubts about the logical possibility of rationally defending our most basic procedures of knowledge acquisition. These queries ask for a rational defence (or criticism) of our ultimate methodological presuppositions. What is being challenged is the possibility of rationality as a self-comprehensive activity, an activity without regress, dogmatism, vicious circularities, paradoxes or inconsistencies.

I THE PROBLEMS

P1: Is rationality rational?

Can rationality be self-justifying or 'comprehensive'?

If not, is rationality defensible rationally, i.e. without regress, vicious circularities, logical paradoxes, or fideistic commitments?

In particular, P2: Is scientific rationality rational?

Can scientists be rational about, or defend rationally, their ultimate cognitive aims and rules of method? In other words, can the basic concerns of scientific rationality themselves be given a rational defence?

If not, from what standpoint, other than that of mere prejudice or a viciously circular one, can we assess the ultimate cognitive aims and rules¹ of science? Suppose, for example, that some scientific community espouses some methodological rules; the question then arises of what underwrites these rules. A sceptical answer to this question was given by Sextus Empiricus², who argued that if we would have a basis for saying how things actually are, we would require rules or criteria to select amongst conflicting appearances, criteria that would provide us with knowledge of reality. The sceptic would then probe these rules in their turn, and show (as Sextus does in the five tropes of 'Agrippa') how an infinite regress is started once we ask for warrants of these rules (my P2) so that there is no fixed point,

...from which to begin to establish anything, and suspension of judgment follows.

(Sextus Empiricus.)

The 'dogmatist' then either blocks the infinite regress with something,

¹ The ultimate cognitive presuppositions of science are those cognitive principles and aims underpinning whatever framework of rationality one may propose for scientific rationality, principles such as modus ponens and some inductive rule, or scientific aims such as empirical predictability.

² Quoted and discussed in Hookway, pp. 9-11.

...assume[d] simply and without proof in virtue of a concession.

(Sextus Empiricus)

That is, the dogmatist stops the regress by assuming something on faith rather than on evidence. A third alternative is to stop the regress by arguing circularly, a move by which the justification of the methodological rule rests upon the same rule, or when the justification of the methodological rule depends on propositions whose warrant in turn depends upon the rule at issue.

The problem of justifying our methodological criteria is especially important in periods of intellectual crisis, in periods of epistemic change, when there is no unchallenged criterion of knowledge. The fundamental question then arises of how to justify the basis of one's knowledge; this problem of deciding on a criterion became important, for example, during the Reformation and its sequel (the period 1500-1675), because the need arose of how to recognize religious truth (if any). Protestants proposed an alternative criterion of religious truth ("what individual conscience is compelled to believe on reading Scripture is true"³) to the orthodox Catholic one (the Church's traditional authority as the judge of correctness of Bible interpretation and any other religious propositions) and then one had to choose between these two competing criteria. One had to identify the correct criterion, that is, one had to decide how to justify the basis of one's knowledge.

The search for a solution to conflicts between criteria leads to the trilemma already discussed by Sextus:

... in order to decide the dispute which has arisen about the criterion, we must possess an accepted criterion by which we shall be able to judge the dispute; and in order to possess an accepted criterion, the dispute about the criterion must first be decided. And when the argument thus reduces itself to a form of circular reasoning the discovery of the criterion becomes impracticable, since we do not allow them [the Dogmatic philosophers] to adopt a criterion by assumption, while if they offer to judge the criterion by a criterion we force them to a regress *ad infinitum*.

(Sextus Empiricus, *Outlines of Pyrrhonism*, II, Chap. IV.)

OF RATIONAL METHODS AND SCIENTIFIC AIMS

Most philosophers before the 1960's (such as Hempel, Carnap, E. Nagel, Popper) considered science as the archetype of a rational enterprise. Hence, for these thinkers a theory of science would also have been a theory of rationality, and since scientists do not seem to act capriciously when evaluating scientific theories, science has been thought of as having a method (or methods), and the rationality of science has been thought to reside in its method.

In what follows, I will address the question of how to justify as correct our scientific methods and aims (problem P2) and I will end concluding that our ultimate scientific methods have to be accepted as correct dogmatically. That is, it will be concluded that our most basic scientific methods have to be accepted as correct without justification or only with a viciously circular justification.

Now, whatever the method of science is, one would expect that it will include some collection of inferential processes, such as induction and deduction, plus some 'source' or non-inferential processes, such as memory, perception and intuition. Let's focus on the justification of our inferential processes.

Induction and deduction have been the prototypes of rational inferential strategies, and the argumentative dead ends reviewed by Sextus: infinite regress, dogmatism, and circularity, have been examined repeatedly when searching for warrants for induction and deduction. Thus, Hume argued against induction in a form reminiscent of Sextus' Agrippan modes, he claimed that no rational justification can be provided for induction:

³ Cf., H. Popkin, p. 3

... all our experimental conclusions proceed upon the supposition that the future will be conformable to the past. To endeavour, therefore, the proof of this last supposition by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question.

(Hume, 1748, pp. 35-6.)

Thus, Hume answered P2 (for induction) in the negative, since to try to prove induction empirically is to end up with a circle, and to escape the circle one would have to support one portion of Reason, induction, by another portion of Reason, say deduction. But this cannot be done, at least *prima facie*, since deduction and induction are different forms of inference, thus in the case of valid deductive arguments, if the premisses are true then the conclusion will of necessity be true, while in the case of inductive inferences from true premisses a conclusion with any true value can follow. Thus, Russell's inductive turkey (which after being fed every day for many months and inductively inferring that this phenomenon was a natural regularity finally found, to his chagrin, that he was going to be cooked) dramatically exemplifies this last situation. Therefore, the deductive support desired for induction is unavailable, since deduction cannot warrant a form of inference that is not deductive⁴. Hume offered instead a non-rational explanation for our inductive or causal reasoning:

Our judgements concerning cause and effect are derived from habit and experience; and when we have been accustomed to see one object united with another, our imagination passes from the first to the second, by a

⁴ Inductive inferences are not deductions except if we put as a premiss of the inductive argument a principle of induction. This deductive-inductive argument could go as follows:

Many A's are found to have property B, in a wide variety of situations and without exception.

Principle of induction: If many A's are found to have property B, without exception and in a wide variety of situations, then probably *all* A's have property B.

Then probably all A's have property B.

The problem of finding a justification now re-arises, however, because the sceptic will demand the warrant of the principle of induction, and this principle again cannot be warranted by deduction, because the conditional characterizing the principle of induction may well have a true antecedent and a false consequent.

natural transition, which precedes reflection, and which cannot be prevented by it.

(Hume, 1739, p.147.)

Hume claims that inductive or causal reasoning is the result of our mental functioning, and he claims that its source is our disposition to reason inductively or causally, and that this disposition of ours is guided by habit and imagination, that it is a 'natural transition' that is involuntary. Now, this putative urge of ours does not legitimise induction, it does not justify it. In other words, a propensity for inductive inferences may be a fixed brute fact of the human condition, but this human fact does not legitimise our inductive inferences. It does not provide us with reasons for thinking that inductive inferences lead to truths (at least, in the sense of the correspondence theory of truth.) In short, even if it could be proved that we have an inductive disposition, this fact by itself would fail to show that our hypothetical inferential disposition is correct. A Humean would say, that while reasoning inductively,

... we accept what we find ourselves accepting, and rational evaluation does not influence acceptance.

(Hookway, p. 94)

If so, we would find ourselves being lead by our imagination and habit into sustaining beliefs that cannot be warranted. We would find ourselves constrained by processes that seem to undermine our intellectual freedom; we would be psychologically inclined not to doubt our inductive beliefs even though we cannot logically warrant them. If so, we cannot disqualify as irrational the man who does not make inductions (who for example makes counter-inductions) since this strange man could always retort that he just has a different habit, and then there wouldn't be any difference between insanity and sanity as Russell would say⁵.

D. Papineau has argued in favour of the reliability of induction⁶ as follows:

⁵ B. Russell, 1945, p. 673.

⁶ Cf., Papineau 1992, pp. 13-14.

- i. People objectively make inductions: they do not just think they make them.
- ii. When people have made inductions, "their conclusions have indeed turned out true".

Papineau then concludes from these two premisses:

- iii. "In general, inductive inferences yield truths."

These three statements are, however, problematic:

- a) Popper, for instance, has denied the very common first assumption, but for the sake of argument, I will grant this supposition.
- b) The second assumption, on the other hand, is clearly false, there are numerous examples of inductions that lead to wrong conclusions (as is the case of the inference made by Russell's turkey). Moreover, it is not a simple matter to distinguish 'good' inductions from 'wrong' ones except *ex post facto*. Papineau tries to deal with this objection by narrowing the characterization of induction; he recognizes that "simple enumerative induction cannot *possibly* be a reliable⁷ method of inference." But then, exactly which inductive strategy is reliable? Papineau vaguely speculates that reliable inductions would be those constrained by considerations about projectable predicates, such that,

... the conclusions of such inferences will need to be beliefs of less than full degree, and perhaps also restricted to claims that certain judgments are approximately, rather than precisely, true.

(Papineau, 1992, p. 19)

- c) Whereas, the third assumption, it is plainly circular. Papineau infers (from his belief that many past inductions have yielded truths and by using an inductive rule of inference) that in general inductive inferences yield truths. Hence, Papineau uses (and assumes as reliable⁸) at the meta level the very inductive

⁷ An inferential strategy is reliable if its deliverances are in general true when its premisses are true.

⁸ Notice that Papineau is using induction as an inferential rule in an argument that he takes to be correct, therefore he is at least implicitly assuming that induction is a reliable inferential strategy, precisely what he wants to establish via his argument.

inferential rule that he is trying to justify, he uses induction to conclude that induction is a reliable inferential strategy.

Papineau deals with this criticism of circularity by distinguishing 'rule circularity' from 'premiss-circularity,' and by claiming that:

1. Rule circularity is acceptable, and that only premiss-circularity is vicious.
 2. That the circularity found in his previous inductive argument for induction is 'only' rule circular and therefore not viciously circular.
- Thus,

An argument is premiss-circular if its *conclusion is contained among its premisses*; an argument is rule-circular if it reaches the conclusion that a certain rule of inference is reliable by *using* that same rule of inference...

(Papineau, 1992, p. 15.)

Notice that Papineau's characterization of vicious circularity cannot be right, at least *prima facie*, since it could imply that all deductively valid arguments are viciously circular. Papineau is aware of this difficulty but he thinks it can be disposed of by properly constructing the concept of 'containment', something Papineau leaves undone. Leaving this aside, Papineau points that the warranting of any methods of investigation has to start somewhere,⁹ and that

... it can scarcely be a general requirement, on all legitimate forms of inference, that it be possible to show that they are reliable in some **non-rule-circular way. For this would disqualify even deduction** as a legitimate form of inference.

(Ibidem, p. 16) (Emphasis added.)

A sceptic would retort: 'and so what?' Our sceptic's leitmotif is precisely that our most basic scientific methods cannot have a non-circular warrant. He would argue that our most basic methods -including deduction- have the logical character of

dogmas, that these methods are not justifiable as reliable without circularity, and that therefore they may not be rational¹⁰. For the sceptic, these methodological dogmas provide our methodological starting point they are our methodological foundations. Papineau would answer that there is no reason to distrust the reliability of induction and consequently there is no reason to ask for its non-circular justification, while the sceptic will retort that the lack of non-circular justification for our methodological foundations provides by itself one reason for doubting the reliability of these foundations. Papineau, for example, says of his circular inductive argument for induction¹¹:

We should not expect it to perform the impossible task of knocking imaginary non-inductivists out of their non-inductivism - its task is only to allow **normal people, like ourselves**, to resolve the issue of whether induction is reliable.

(Ibidem, 1992, p. 18.)(Emphasis added.)

Hence, for Papineau, the sceptic is not one of the 'normal people'; the sceptic is not normal because he doesn't share Papineau's standards. For the sceptic, induction's lack of a non-circular justification means that induction's reliability is being taken for granted, since we are already assuming that induction is in general a correct rule of inference, when circularly using induction to conclude that induction is in general a correct rule of inference. This circularity is not, however, for Papineau reason enough to distrust induction's reliability, so there is a clash of standards about what is sufficient to warrant a doubt about induction's reliability.

Still, what is acceptable about rule circularity, that isn't okay about premiss circularity? Papineau says that his previous inductive argument for the reliability of induction is satisfactory because "it is a genuinely expansive argument, whose

⁹ Cf., Papineau (1992), p. 15.

¹⁰ Whether these methodological foundations are rational or not will depend on our theory of rationality, a belief in these foundations would be irrational, for example, if a belief or statement is rational iff it is justified without circularity. Various theories of rationality will be analysed in the second part of this chapter.

¹¹ Max Black already argued for a circular inductive defence of induction in the *Journal of Philosophy*, vol. 55 (1958), pp. 718-25.

conclusion... manifestly outruns its premisses..."¹² and therefore it is clearly not premiss-circular, this evaluation of rule circularity looks however suspiciously self-serving. Yet, it could be argued that premiss-circularity is vicious because if an argument intends to be probative then the premisses should be less problematic, more secure, than the conclusion. While rule circularity is not vicious, because it does not intend to be probative, it only wants to show that there is some sort of coherence amongst our system of beliefs and cognitive processes. But the sceptic does not want just this; he wants some sort of demonstration -even if not a conclusive one- that our most basic methodological processes are reliable. This demonstration is precisely what is lacking; Papineau can show induction to be reliable only if he assumes induction as reliable, that is, only by self-vindication.

Rule circularity seems to be a mask for the uncomfortable fact that some basic and crucial inferential processes have to be taken for granted as reliable. Rule circularity is a euphemism for the unsavoury fact that while some basic inferential processes seem to be reliable in the light of our intellectual system - an intellectual system that includes and requires these very processes - these processes may in fact be unreliable. It is an uncomfortable fact because it implies that the world could be quite different from our theoretical picture of it. It means that we may end rejecting some truths and accepting some falsehoods, even if we were to achieve maximal epistemic coherence, that is, a state where all our processes would seem reliable in the light of our reliably generated beliefs. It means that even in the case of maximal epistemic coherence our reliable processes may only seem to us to be reliable, but in fact may not be reliable. And this means that our success at survival may in part be the result, so far, of a fortunate accident.

Papineau asks us to accept rule circularity, because otherwise, if rational, we might have to give up induction and even deduction, and end in "philosophical catatonia" (ibidem, p.15). Not really, we would just have to resign ourselves to the fact that there is no escape from the circle or web of belief, even if this web is quite comprehensive. Or, to what is logically equivalent, we would have to resign ourselves to the logical fact, that some methodological dogmas - a minimal methodological dogmatism - has to be admitted because it is inescapable.

¹² Cf., Papineau, 1992, p. 15.

Now, Papineau (ibidem, p. 17-8) recognizes that one could use rule circularity even to justify counter-induction as reliable, as a result, many possible methods of inference could be self-supporting, although not all. For example,

While soothsayers may use dreams as a method of prediction, they do not **normally dream** that dreaming is a reliable method of prediction.

(Goldman, p. 45)(Emphasis added.)

Hence, dreaming as a method of prediction is not the kind of method that can 'normally' vindicate itself. But what does Goldman mean by 'normally dream'? If he means a recurring dream, this is too strong a requirement, since, for a soothsayer to circularly justify dreams as methods of prediction, it could be enough that at least once a soothsayer dreams that dreaming is a reliable method of prediction. And if Goldman means by 'normally dream', to dream correctly, then Goldman would be invoking evaluative standards to judge some dreams, and to judge some rule-circularities, and these evaluative standards would have to be spelled out and justified.

Rule circularity works only for the faithful: with those who make inductions, and who don't doubt inductive inferences but who still enjoy the credo being ratified. Circular rule justification is merely reaffirmation, inside the web of belief. In other words,

... the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle.

(Th. Kuhn, p. 94.)

Papineau as a true believer says of induction:

... neither the fact that this practice may be invalid, nor the fact that its reliability might only be discoverable in a rule-circular way, will be a barrier to our concluding that it yields knowledge.

(Papineau, 1992, p. 20.)

Papineau ends with circularity or dogmatism, thus Papineau cannot cope with our sceptic's request for non-circular justifications. And this negative result, this lack of non-circular ultimate justification, may open the gates to a possible relativism of various incompatible -and unjustified- ultimate methods. This negative result creates the logical possibility of a relativism of more than one self-vindicating coherent intellectual system, i.e., of more than one fully self-vindicating web of beliefs *cum* inferential processes. One such intellectual system could be the set of beliefs generated by an oracle who would declare himself as an infallible source of beliefs. Papineau excludes such oracle systems by saying that they are not **properly** self-vindicating. Papineau recommends that satisfactory intellectual systems should **properly** individuate belief-forming processes, to have as powerful a set of belief-forming processes as possible (cf., 1987, p. 204). The problem now arises of how to justify these evaluative judgments, and of whether these recommendations are not *ad hoc* or self-serving. Extra examples of possible alternative coherent webs of belief could be that of the 'scientific' creationist, or that of the counter-inductivist, hence belief coherence is not enough.

Papineau, however, believes that in a world like ours¹³ only one intellectual system will be fully self-vindicating or coherent, and that such an intellectual system will be the correct one.¹⁴ Papineau introduces a general meta-principle (a meta-principle that recalls Occam's razor) and says: "a complex theory won't be true if a simple theory fits the same evidence."¹⁵ Papineau then argues - with the help of this meta-principle - that in worlds like ours, different intellectual systems couldn't all be fully self-vindicating, because only the system incorporating the simplest theories, that fit the evidence, would be correct. Therefore, Papineau may conclude that a relativism of alternative coherent systems is not a danger.

¹³ A world like ours is a world that differs from the actual world only in some historical specifics, for example, a world where the Aztecs would not have been conquered in the XVIth century. Papineau's intuition is that a world like ours is a world with causal mechanisms analogous to those of the actual world, a world in which we can "discount outlandish causal possibilities." Papineau, 1987, p. 222.

¹⁴ Cf., Papineau, 1987, p. 219.

¹⁵ Cf., *ibidem*, p. 225.

The sceptic won't agree, he will point out that there are many accounts of what to understand by simplicity, that simplicity is a notoriously difficult notion, that it is a problematic criterion of choice among scientific theories. In addition, since it is a matter of debate what to understand by simplicity, the meta-principle could be used to select whatever theory is prejudged as convenient, the meta-principle can easily be used to warrant our prejudices¹⁶. Besides, Papineau's meta-principle seems to be saying that nature is simple, but what reason is there to suppose that this metaphysical assumption is true? What reason is there to suppose that simplicity is a sign of truth? In other words, why should we accept a simpler theory instead of its more complex rivals? Or is Papineau's vague meta-principle also going to be taken for granted?

Papineau also argues (for his belief that there is only one correct and fully vindicating system of beliefs) by saying, that to claim that a coherent or fully self-vindicating intellectual system could be erroneous in a world like ours, is tantamount

¹⁶ Papineau concedes that simplicity is a rather obscure concept, but Papineau believes that a proper notion of simplicity can be obtained by a detailed study of the history of science. Papineau says, "I don't want to appeal to some *a priori* notion of simplicity ... The appropriate notion needs to be filled out by detailed reflection on the progress of science." (Papineau, 1987, p 198.)

The question now arises of how are we to know the progress we are analysing is that of bona fide scientific progress and not, for example, that of creation science. It seems we will use a tacit notion of simplicity to select simple answers to our scientific questions, and then from a historical study of our 'simple' scientific answers or simple scientific theories we will try to infer what to understand by simplicity. Hence, we will end by merely getting what we started with, thus,

Suppose that we have arrived at a number of answers to high-level theoretical questions in chemistry, physics, astronomy, etc. Think of the pattern of such high-level answers as the experimental data. Then it may be that the only reasonably **simple** account which is consistent with that pattern is that high-level answers are in general **simple**. So **we arrive at the conclusion**, that high-level answers are generally **simple**, by an inference **whose reliability is explained by that conclusion itself**. (To see that this trick isn't as empty as it might seem, consider people who generally opted for complex answers rather than simple ones. Then their general run of theoretical answers would be complex. But such people wouldn't be able to bootstrap themselves to the assumption that the right answer is generally complex, **for that wouldn't be a complex account of their pattern of discoveries, but a simple one.**)

(Papineau, 1987, pp. 197-8.) (Emphasis added.)

Using which standards, and why, does Papineau affirm that the account of the complexity seekers wouldn't be a complex one, "but a simple one"?

to supposing that our world has contrived a malicious conspiracy against its inhabitants. Not necessarily. There are other options. For example, if we were to have an erroneous fully self-vindicating intellectual system instead of concluding that one lives in a conspiratory or malicious world, one could conclude that the world is so subtle that it appears as conspiratory. We may then go on to wonder whether we have tried hard enough to avoid error, or whether our faculties are up to the challenge of explaining and predicting such a world. Alternatively, we could question our epistemology and its characterization of a 'fully self vindicating' intellectual system.

The difference between our sceptic and Papineau may be in the end only a difference of temperaments, a distinction between cognitive pessimism and optimism. Papineau optimistically assumes that the world is understandable and explainable because so far it seems to have been understandable and explainable, and he thinks his rule-circular argument gives us reason to trust induction as reliable; while our sceptic is more cautious and doubts, because there are no grounds acceptable to him to trust induction. Still our sceptic does not deny induction, in fact, he may use induction, but since there is no non-circular justification of induction, he cautiously keeps on doubting it and accepts induction only passively, that is, without actually believing in it. This attitude is not new, Sextus already suggested it in his tropes of 'Agrippa'.

Now, if belief in induction cannot be justified without circularity, then we find ourselves with scientific and common sense conclusions that, if arrived by induction, cannot ultimately be warranted as true. If so, we go through the world hoping for the best, since at any moment our hopes could turn out to be radically misconceived. And although we cannot justify our hope in correct prediction, neither can we say that our predictive attempts are hopeless, then we might as well keep trying, and recognise that,

Hume was right in asserting that the conclusion of the inductive inference cannot be proved to be true; and we may add that it cannot even be proved to be probable. But Hume was wrong in stating that the inductive procedure is unjustifiable. It can be justified as an instrument... *of positing...*

Now it is clear that, though the inductive rule does not supply knowledge of a future event, it supplies a sufficient reason for action: we are justified in dealing with the anticipative posit as true, not because we can expect success in the individual case, but because **if we can ever act successfully we can do so by following the directive of induction.**

The justification of induction constructed may, therefore, be called a *pragmatic* justification: it demonstrates the usefulness of the inductive procedure for the purpose of acting. Actions can be made in the sense of trials, **and it is sufficient to have a method that will lead to successful trials if success is attainable at all...**

We adjust our actions to the case of a predictable world -if the world is not predictable, very well, then we have acted in vain.

(Reichenbach, 1949, pp. 475-81, 82.) (Emphasis added.)

Reichenbach is saying that *if nature is uniform, that if nature is predictable, if there is any strategy that will let us make reliable predictions, then the persistent use of induction will let us make reliable predictions.* The problem of induction now re-emerges, however, as the search for a warrant to this conditional; Reichenbach illustrates his argument through the following parable,

A blind man who has lost his way in the mountains feels a trail with his stick. He does not know where the path will lead him, or whether it may take him so close to the edge of a precipice that he will be plunged into the abyss. Yet he follows the path, groping his way step by step, for **if there is any possibility of getting out of the wilderness, it is by feeling his way along the path.** And we know: **if we can find a way through the future it is by feeling our way along this path.**

(Reichenbach, 1949, p. 482.) (Emphasis added.)

Still, how does the blind man know these conditionals (cf., the emphasised phrases in the quote above)? To illustrate the difficulties that arise when we want to justify such conditionals, consider a simplified case of induction, a case of enumerative induction, and suppose that out of a large number of observed instances of some

property A, all or a large fraction of A's (say, P/Q) have been observed accompanied with some other logically independent property B. And suppose that the usual conditions on legitimate inductions are satisfied, such as that the observed correlation between the A's and the B's holds over a wide variety of situations and without any known exception. Then, for Reichenbach induction is a method to arrive at conjectures about the true value, to the value in the world, of the quotient (say, to p/q), and these inductive conjectures can go on being corrected as new information comes in. The true quotient (p/q) would be the limit of the sequence of observed fractions P/Q as the number of observed fractions goes to infinity. The problem is that we don't know the chance of finding such a limit, nor even if there is such a limit¹⁷.

Reichenbach holds that if there is a limit, then the persistent use of induction by enumeration will in the end eventually approximate this limit to any desired degree of accuracy. Notice, however, that even if there is a limit we don't know how many attempts it will take to approach this limit to any desired degree of accuracy, so we will never know if our present inductive prediction is correct. Otherwise, we will never know if we have already found the sought after limit.

Moreover, this difficulty with a specific inductive rule becomes worse once we realize that there are indefinitely many other rules (called asymptotic rules) or alternative frequency sequences which agree with induction in the long run but which may differ widely with induction in the short term. These sequences differ from each other by a parameter that tends to zero as the frequency sequences go to infinity.

How can then we select the inductive rule (allegedly the best rule) from amongst this infinitude of different frequency sequences or rules? Without a criterion to choose from amongst these multitude of sequences our predictions in the short run (mainly the only predictions of practical and scientific import) would differ widely, and we wouldn't know which predictions to make, we wouldn't know which of our present predictions are correct, and our predictions would be *de facto* arbitrary. On the other hand, any criterion that may be devised to select amongst these various sequences

¹⁷ It might be argued that in a world like ours where we have found a past order, or a past stable ratio P/Q , it is highly unlikely that the ratio of A's and B's has no limit. Otherwise,

would in turn require justification and the problem of the justification of induction would re-emerge under another guise.

In other words, there is an infinitude of rules that may lead in the end to the same – and yet unknown- true quotient p/q , and to select amongst these rules we require some criterion, for example a criterion of simplicity, or some other alternative criterion. Then the problem re-arises, that is, that of justifying without circularity or regress the proposed criterion of simplicity, or any other alternative criterion.¹⁸

Furthermore, even if we were granted our criterion, or even if we somehow could justify it, we would still need to prove that the criterion of choice would deal successfully with paradoxes such as those of Goodman's projectable predicates, because of these difficulties, the pragmatic justification of induction is not conclusive.¹⁹ Therefore, it seems that at least for the time being, there is no alternative to a dogmatic assertion that we know the inductive principle or some equivalent.

ON THE JUSTIFICATION OF DEDUCTION

On the other hand, one could imagine a Humean criticising deductive proofs by noting that their justification leads to an infinite regress, because whenever we assume as correct a deductive step in a proof, we will, if rational, ask for its justification as correct, and then for a reason for thinking that this last justification is a good one, and so on.

Again, this Humean would then adduce custom and imagination as explanations of why we do not take seriously this infinite regress, yet as before, the justification of our most basic deductive presuppositions is not a psychological problem, it is rather a logical problem.

A deductive justificatory infinite regress was illustrated by L. Carroll (cf., p.431) in his well known '*What the Tortoise said to Achilles*' where he deals with a Tortoise's unending demands for justification of the deductive rule of *modus ponens*. Carroll

that given the evidence it is highly unlikely that our world is a chaotic world where there is no limit. This argument, however, is itself inductive.

¹⁸ Salmon has proposed such an alternative criterion, a criterion of 'linguistic invariance'.

shows that if asked for the justification of a deductive inference we end in an infinite regress, since every logical step requires further justification. Let's listen to a fragment of the dialogue between Achilles and the Tortoise while they discuss a "little bit" of the argument ("just two steps, and the conclusion drawn from them") of Euclid's First Proposition. The Tortoise starts the dialogue:

Kindly enter them in your note-book. And, in order to refer to them conveniently, let's call them A, B, and Z:

"(A) Things that are equal to the same are equal to each other.

"(B) The two sides of this Triangle are things that are equal to the same.

"(Z) The two sides of this Triangle are equal to each other.

"Readers of Euclid will grant, I suppose, that Z follows logically from A and B, so that any one who accepts A and B as true, *must* accept Z as true?"

"Undoubtedly!" (...)

"And if some reader has *not* yet accepted A and B as true, he might still accept the *Sequence* as a *valid* one, I suppose?"

"No doubt such a reader might exist. He might say 'I accept as true the Hypothetical Proposition that, *if* A and B be true, Z must be true; but I *don't* accept A and B as true.' (...)"

"And might there not also be some reader who would say 'I accept A and B as true, but I *don't* accept the Hypothetical'?"

"Certainly there might" (...)

"And *neither* of these readers," the Tortoise continued, "is as *yet* under any logical necessity to accept Z as true?"

"Quite so," Achilles assented.

"Well, now, I want you to consider *me* as a reader of the *second* kind, and to force me, logically, to accept Z as true."

(...) "- an anomaly, of course," the Tortoise hastily interrupted. (...)

"I 'm to force you to accept Z, am I?" Achilles said musingly. "And your present position is that you accept A and B, but you *don't* accept the Hypothetical -"

"Let's call it C," said the Tortoise.

¹⁹ Cf., Salmon, ref.

“- but you *don't* accept.”

“(C) If A and B are true, Z must be true.”

“That is my present position,” said the Tortoise.

“Then I must ask you to accept C.”

“I’ll do so, said the Tortoise, “as soon as you’ve entered it in that note-book of yours. (...) “Now write as I dictate:

“(A) Things that are equal to the same are equal to each other.

“(B) The two sides of this Triangle are things that are equal to the same.

“(C) If A and B are true, Z must be true.

“(Z) The two sides of this Triangle are equal to each other.”

(...) “If you accept A and B and C, you *must* accept Z.” [Achilles said.]

“And why *must* I?”

“Because it follows *logically* from them. If A and B and C are true, Z *must* be true. You don’t dispute *that*, I imagine?”

“If A and B and C are true, Z *must* be true,” the Tortoise thoughtfully repeated. “That’s *another* Hypothetical, isn’t it? And, if I failed to see its truth, I might accept A and B and C, and *still* not accept Z, mightn’t I?”

“You might,” (...) “So I might ask you to grant one more Hypothetical!”

“Very good. (...) We will call it

“(D) If A and B and C are true, Z must be true.

(...) “Now that you accept A and B and C and D, *of course* you accept Z.”

“Do I?” said the Tortoise innocently. “Let’s make that quite clear. I accept A and B and C and D. Suppose I *still* refused to accept Z?”

[... And the Tortoise proceeded to dictate a new Hypothetical]

“(E) If A and B and C and D are true, Z must be true.

“Until I’ve granted *that*, of course I needn’t grant Z. So it’s quite a *necessary* step, you see?”

“I see,” said Achilles; and there was a touch of sadness in his tone.

At the end, as Lewis Carroll pointed out, we invoke precisely what we are asked to justify: *modus ponens*. And we end up like Sextus’s dogmatist by circularly reaffirming, by merely repeating, the statement to be warranted. Worrall explains Carroll’s dialogue as follows,

... that someone accepts that p and accepts that p then q but refuses to accept q. One might try to convince him as follows: 'modus ponens in general is truth-transmitting -if p is true then if p then q is true then q must be true; here p and p then q are both true so you must infer q'. But clearly this is hardly likely to convince: if someone really refuses to infer q from p and if p then q, then it will not be surprising if he further refuses to infer that he must infer q from the fact that if an argument is of a valid form and he accepts the premisses then he must infer the conclusion and the fact that modus ponens is a valid form whose premisses he accepts in this instance. This latter inference clearly itself (doubly) involves modus ponens.

(Worrall, 1989, pp. 382-3.)

Carroll's tortoise doesn't believe in the *modus ponens* conditional (if you believe p and if you believe 'p then q' then you should believe q), a conditional that we expect any rational being to believe, hence, we will end by declaring Carroll's tortoise as irrational. The Tortoise, however, could defend itself from the accusation of irrationality by arguing that if we don't justify *modus ponens* without vicious circularity she is not obliged to rationally believe it. To justify the adoption of *modus ponens* in the object language, however, would require of the same rule, or some other just as problematic inferential rule, at the meta-level, and then the petitions of justification would go on. In other words, to prove that *modus ponens* is a rule that preserves truth in the object language requires some deductive argument in the meta-language, and this in turn would require that the meta-language has *modus ponens* or some other deductive rules, inferential rules which would in turn require justification. Therefore, we cannot prove without circularity or regress that *modus ponens* preserves truth hence we cannot justify deduction. Or else,

The basic point is that justifications of deduction themselves presuppose deduction. They are circular because they appeal to the very principles of inference that are in question...

Circularity emerges whenever an attempt is made to ground our most general notions of validity.

(Barnes and Bloor, pp. 41-3)

Thus, deduction is circularly self-justifying, and it is to be held dogmatically²⁰.

²⁰ Still D. Papineau (Papineau, 1987, chapter IX) and M. Dummett (cf., Dummett, chapter XVII) have offered a vindication of deduction by explaining the meaning of the logical constants, that is, by explaining how these constants work in making new sentences out of old.

Dummett recognizes that his semantic vindication of deduction **employs deductive forms** of inference, and that it is thus circular. He believes, however, that circularity is acceptable in explanations thus,

Our problem is not to persuade anyone, not even ourselves, to employ deductive arguments: it is to find a **satisfactory explanation** of the role of such arguments in our use of language.

... Characteristically, **in an explanation, the conclusion of the argument is given in advance; and it may well be that our only reason for believing the premisses of the explanatory argument is that they provide the most plausible explanation for the truth of the conclusion.** Hence the charge of circularity or of begging the question is not applicable to an explanatory argument in the way that it is to a suasive argument. A philosopher who asks for a justification of the process of deductive reasoning is not seeking to be persuaded of its justifiability, but to be given an **explanation** of it.

... Such an argument will, of course be deductive in character, but that will not rob it of its explanatory power: we already engage in deductive reasoning, and therefore will be ready to admit that the conclusion of a deductive argument which strikes us as valid follows from its premisses; hence, in a suitable case, we shall also be ready to admit that the premisses of such an argument provide an **explanation** for the truth of the conclusion, **even when the conclusion is to the effect that deductive reasoning is justified.**

(Dummett, p. 296.) (Emphasis added.)

Dummett has, however, substituted justification with explanation, that is, he has redefined justification as explanation.

Papineau in the same spirit offers a biological explanation of the origin of the meaning of the logical constants. This biological explanation (not justification) of deduction via the biological origin of the semantics of the logical constants, however, also appears as circular. It is said that the deductive semantics now present in our minds was allegedly biologically selected in the past, and it was selected because this semantics had in the past advantageous behavioural effects, behaviour which led to the thriving of our ancestors and to the preservation of this semantics. In other words, our deductive habits were selected because they led in the distant past to advantageous action. However, how do we know this? Answer: we know it via our science, a science that was confirmed and validated by deduction and some other belief forming processes (processes such as induction, memory and perception.) Papineau's vindication of deduction then presumes in the end deductive moves, so what Papineau is offering is also a circular explanation of deduction. Still, Papineau's effort may not be worthless, since it offers a casual explanation *-though not a non-circular justification-* of the origin of our deductive strategies. Thus, Papineau says,

... the vindications I have given for certain inferential moves will scarcely *persuade* anybody who doesn't already make deductive inferences to start making them, since the vindications themselves proceed via deductive moves,...

(Papineau, 1987, p. 168.)

A possible rejoinder to the previous criticisms of induction and deduction because of their lack of non-circular justification could be,

... (induction) could be irrational only if there were a standard of rationality which it failed to meet; whereas in fact it goes to set the standard: arguments are judged to be rational or irrational by reference to it ... When it is understood that there logically could be no court of superior jurisdiction, it hardly seems troubling that inductive reasoning should be left, as it were, to act as judge in its own cause.

Since there can be no proof that what we take to be good evidence really is so, (then) it is not sensible to demand one.

(Ayer, pp. 75-81.)

Ayer is saying that it is impossible to judge induction as irrational because induction sets the standards of rationality, but that is the crux of the matter. If not, how do we know this? In other words, how do we know that induction sets correct standards of rationality? Since Ayer does not provide an argument to support his contention, he is just taking for granted, as a matter of faith, the correctness, the rationality of induction. He assumes without argument that induction is rationally legitimate and that there is no higher legitimate "court"; if we grant him this, then of course there are no other rational standards that might invalidate induction (or deduction for that matter) as irrational. This is just a form of fideism or dogmatism since without argument (other than self justification) it is held that induction is rationally correct.

The difficulty with Ayer's move, as argued by Salmon, is that there are many other possible superior courts, besides the courts of induction or deduction, with the same jurisdiction. If we leave each form of reasoning 'to act as judge in its own cause' then there will be many conflicting judgements. For example, consider the following argument that gives support to the fallacy of affirming the consequent:

a) If affirming the consequent is a valid form of reasoning, then $2 + 2 = 4$.

b) $2 + 2 = 4$.

Therefore,

Affirming the consequent is a valid form of reasoning.

This argument has true premisses, it has the form of affirming the consequent, and it asserts the validity of this fallacious form of reasoning. One can analogously construct a counter-inductive argument in favour of counter-induction:

On a large number of occasions in the past people have counter-induced and have been led to false conclusions. Therefore, we conclude counter-inductively that whenever someone counter-induces his conclusion is true.

This argument supports counter-induction as a method for arriving at truths.²¹

If we knew that induction and deduction were the correct standards of rationality, then there would not exist other rational standards which also would be correct but which would disqualify the former as irrational. The problem is that we can show induction and deduction to be 'correct' only with viciously circular arguments, and they can share this form of circular defence with many other alternative forms of reasoning. Hence, we end with a relativism of various logically possible incompatible and dogmatic forms of reasoning, and to avoid this relativism we accept as legitimate only some circular defences or 'courts.' Nevertheless, why should only some forms of reasoning be given the privilege to viciously circular judge themselves? It seems that we allow this prerogative only to those of our inferential methods that accord with our intuitive prejudices of methodological correctness, and in this way, we reiterate our methodological instincts.

It would seem, from the discussion this far, that induction and deduction are in the same boat: neither can be justified. Yet, deduction has traditionally been considered as more basic, as less problematical and as indispensable whereas, induction has been seen (by some such as Popper) as an unnecessary methodological myth. Thus, while how hypotheses gain inductive support or confirmation is a subject of ongoing debate, that of valid deductive inference is a less controversial issue. Still, one might ask, is the exalted status given to deduction a mere prejudice?

OF THE AIMS OF SCIENCE

We discover analogous difficulties if we look for a rational justification for the value of some aims of science, in other words, has a scientist *qua* scientist correct aims, even if these epistemic goals were only implicit? And if so, how does one justify the desirability of these scientific goals? On the matter of the selection of goals or aims for science, Reichenbach, asked:

What is the purpose of scientific inquiry? This is, logically speaking, a question not of truth-character but of volitional decision... If anyone tells us that he studies science for his pleasure and to fill his hours of leisure, we cannot raise the objection that this reasoning is "a false statement" - it is no statement at all but a decision, and everybody has the right to do what he wants.... in books and discourses the word 'science' is always connected with 'discovering truth', sometimes also with 'foreseeing the future'. But, **logically speaking, this is a matter of volitional decision."**

(Reichenbach, 1938, p. 10; emphasis added.)

If Reichenbach is correct we cannot show via a logical argument that one proposal for the aim of science is better than any other, or else, Reichenbach seems to be saying that there is no such a thing as the correct 'aim of science'. Popper analogously said about the aims of science:

My criterion of demarcation will accordingly have to be regarded as a **proposal for an agreement or convention**. As to the suitability of any such convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common. **The choice of that purpose must, of course, *be* ultimately a matter of decision going beyond rational argument...**

Thus I freely admit that in arriving at my proposals I have been guided, in the last analysis, by **value judgments and predilections**.

(Popper, 1959, pp. 37-8; emphasis added.)

²¹ Cf., Salmon, 1964, *The Concept of Inductive Evidence*.

But why should we follow Popper's value judgments and predilections? Popper answers,

It is only from the consequences of my definition of empirical science, and from the methodological decisions which depend upon the definition, that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavours.

(Popper, 1959, p. 55.)

The intuitions of scientists will in the end decide which goal is a legitimate scientific goal, then how does one justify these intuitions? Popper seems to assume that most scientists share the same intuitions, but if not, how could a conflict of intuitions be rationally resolved? The basic problem here is how to rationally assess goals, predilections, intuitions, value judgments, conventions? Or, are scientific goals, ultimately beyond rational argument as the Popper of the LSD and Reichenbach seem to believe? ²² If so, can Reichenbach and Popper avoid scientific goal relativism? That is, can they avoid the thesis that there is no good reason (besides taste, intuitions, prejudices or conventions) for preferring some scientific goals to others, however, capricious or absurd? Laudan has voiced such concerns,

Clearly, if the mainstream tradition in philosophy of science preaches that the methods of science are conventions, and that the aims of science are largely matters of personal preference, it does not take much agility to find therein the makings for a thick relativist stew.

(Laudan, 1989, p. 371.)

Still, is scientific goal relativism something to be worried about? The answer depends on what our theory of scientific method is. If scientific methodological rules

²² One way to argue in favour of some cognitive aims would be to do it pragmatically, that is, it would be claimed that those cognitive aims that promote some ulterior pragmatic end, such as the biological survival of the species should be preferred. But, even if the means/ends relation between the cognitive and the pragmatic ends could be established, whoever

are thought to be the justified optimum means - amongst those available for approaching or attaining our scientific aims - then a relativism of aims could translate into a relativism of scientific methodological rules and this in turn could translate into a relativism of factual scientific theories²³. On the other hand, if our theory of scientific method is such that scientific aims are not to have any implications for the rational selection or appraisal of our scientific methodological rules and scientific factual theories, then a relativism of scientific goals could well be innocuous or irrelevant.

II THEORIES OF RATIONALITY

I will now examine problem P1, that is,

P1: Is rationality rational? Can rationality be self-justifying or 'comprehensive'?

In other words, is a comprehensive rationalism logically possible? Alternatively, is rationality defensible rationally, i.e. without regress, vicious circularities, dogmatic commitments, or logical paradoxes?

The answer to this problem depends on what one's requirements for rational belief (or rational actions or goals) are, for example,

Showing that a belief is not justified is, in the Cartesian tradition, tantamount to showing that it cannot be rationally accepted.

(M. A. Notturmo, p. 94.)

Similarly,

...on an empiricist view, such as Hume's (...) the strongest criticism of any particular theory was that it could not be justified or established properly -in this case by an appeal to sense experience.

(Bartley, p. 115.)

proposes this prescription, and if rational, would have to justify the desirability – again, without circularity – of the pragmatic end in question.

²³ I will dwell on Laudan's theory of scientific aims in chapter V, Laudan's theory intends to avoid scientific goal relativism, a relativism that Laudan takes seriously since his theory of scientific method assumes that scientific methods are the optimum means to the attainment of certain cognitive goals.

Thus both the Cartesian and the Humean traditions require for a rational belief that it be justified as true. These traditions differ only on what they consider as a legitimate source of justification. In the Cartesian tradition intellectual intuition -clear and distinct ideas- provide justification, while in the empiricist tradition sense experience provides justification. A conception of rational belief that demands justifications can already be found in Plato's Euthyphro, where,

Plato suggests that although Euthyphro holds strong views and is willing to act on them, he cannot justify either his beliefs or the action based upon them...

Once Socrates exposes the lack of justification for his belief, it is both irrational and irresponsible for Euthyphro to continue to hold it.

The general idea that emerges from the dialogue is that there is an essential connection between *being rational* in one's beliefs and actions and *having reasons that justify* them.

(Nathanson, p. 4.)

A justificationist view of rationality is also found in Epictetus,

To be a reasonable creature, that alone is insupportable which is unreasonable; but everything reasonable may be supported.

(Epictetus, Discourses, Chapter 2.)

Karl Popper has summarized a justificationist doctrine of rationality as follows:

Uncritical or comprehensive rationalism can be described as the attitude of the person who says 'I am not prepared to accept anything that cannot be defended by means of argument or experience'. We can express this also in the form of the principle that any assumption which cannot be supported either by argument or by experience is to be discarded. Now it is easy to see that this principle of an uncritical rationalism is inconsistent; for since it cannot, in its turn, be supported by argument or by experience, it implies that it should itself be discarded. (It is analogous to the paradox of the liar,

i.e. to a sentence which asserts its own falsity.) Uncritical rationalism is therefore logically untenable...

(Popper, 1945, p. 217.)

According to Popper the uncritical rationalist intends to answer affirmatively P1 but he cannot succeed because in the process he contradicts himself, thus uncritical rationalism is logically unattainable. Uncritical rationalism can be analysed in terms of the following two injunctions:

C1: Anything (belief, action or aim) justified by non-circular argument is to be accepted as rational.

C2: Only that which can be justified by non-circular argument is to be accepted as rational.

Now, from C2 it follows that if rational one should justify anything one accepts. In particular, one should justify C1 and C2, but C1 cannot be justified without presupposing it, because if one were to try to justify C1 by arguing in its favour, we would be begging the question, that is, we would be presupposing C1. In other words, one would be presupposing that argument and experience are rational justificatory strategies, precisely what C1 says, that is,

The argument [in favour of C1] would be generally convincing only to those persons who had *already adopted* the belief that arguments should count. To put the point in a stronger form: it is pointless to try to prove something to a person who does not accept that proofs should be accepted... It seems that an argument in favour of this requirement, in order to be effective, would presuppose a commitment to argument.

(Bartley, p. 94.)

Again, only those who already value argument will accept an argumentative defence of argument, only those who already believe in the attitude described by C1 will accept it. This holds irrespective of which might be the argumentative defence of argument.

The rationalist's attitude is a requisite for valuing argument or experience, and this attitude cannot be established by argument or experience, because if we defend C1

by argument or experience, we are already assuming C1. Then argument plus experience cannot establish itself, it cannot recommend itself, because it requires at least one assumption, the assumption that argument is valuable. This premiss can be defended by argument only by those that have already accepted a pro-argument attitude. Argument is thus limited; it is not self-contained or comprehensive, because all arguing presupposes this premiss, and because this premiss must be accepted as correct without justification, i.e., dogmatically.

Therefore, C1 and C2 cannot both be true. Let's first assume we discard C1 and try to keep C2. C2 should then be non-circularly justified, but C2 cannot be justified by non-circular argument, because if we offer an argument in its favour we are again presupposing that argument is valuable as a rational justificatory strategy, precisely what C2 says. Therefore, C2 rejects itself, and uncritical rationalism is logically impossible.

Another option would be to discard C2 and keep C1 just by itself, this alternative doesn't lead to a contradiction it just says that justified beliefs are rational, but it doesn't require us qua rationalists to justify all our accepted beliefs, actions or goals. Rationalism without C2 wouldn't aim to be comprehensive and it would be a very weak form of rationalism, so weak that according to C1 a non-justified (or an unjustifiable) absurd belief could still be rational.

Popper tells us that the canon C2 of uncritical rationalism is analogous to the paradox of the liar but this is not the case, C2 is just false. Thus if we assume C2 as true we arrive to a self-contradiction, while if we assume its falsity we do not arrive to any inconsistency. Therefore it differs from the paradox of the liar in that it is not an antinomy: "a sentence that it is true if and only if it is false" (Quine, p. 9.)

But even if the principles C1 and C2 required by 'uncritical rationalism' were per impossibile both true we would still have to deal (as Sextus, and more recently, as Fries showed in his famous trilemma²⁴) with the infinite regress, which a succession of demands for justification would lead to²⁵, and these demands would have to be

²⁴ Cf., Popper, 1968, p. 93.

²⁵ That is, if we assume a transitive form of justification such as deductive inferential justification.

satisfied according to this rationalism. One could then try to stop this infinite regress with some dogmas –that is, with some unjustified presuppositions- but this last alternative would be inconsistent with this rationalist's standards.

A doctrine that is self-referentially inconsistent is not uncommon in philosophy. The logical positivist's verifiability criterion of significance is well known to be self-referentially inconsistent.

... according to the logical positivists... Statements testable by the methods in the list (the methods of mathematics, logic, and the empirical sciences) would count as meaningful; all other statements, the positivists maintained, are 'pseudo-statements', or disguised nonsense. An obvious rejoinder was to say that the Logical Positivist's criterion of significance which was self refuting: for the criterion itself is neither (a) 'analytic' (a term used by the positivists to account for logic and mathematics), nor (b) empirically testable.

(Putnam, pp. 105-6.)

As is also self-referentially inconsistent Wittgenstein's assertion that "the totality of true propositions is... the totality of natural science." This statement is inconsistent because it itself does not belong to natural science, and if we assume it as true, we must conclude that it is false. Thus suppose Wittgenstein's argument says more exactly that,

- a. W: a sentence is true iff it is an accredited part of natural science.

And that,

- b. W is a true sentence.

Now,

- c. W is not itself part of natural science (in particular it cannot, in any interesting sense of verification, be verified by observation statements.)

And,

- d. W entails that W is true iff it is an accredited part of natural science. We infer then from (c) and (d) the falsity of W, but (b) says that W is true, then a contradiction.

CRITICAL RATIONALISM

Popper then proposed a modification of the previous Comprehensive Rationalism, Critical Rationalism:

(...) whoever adopts the rationalist attitude does so because without reasoning he has adopted some decision, or belief, or habit, or behaviour, which therefore in its turn must be called irrational. Whatever it may be, we can describe it **as an irrational faith in reason**. Rationalism is therefore far from comprehensive or self-contained.

(...) a critical form of rationalism, one which frankly admits its limitations, and its basis in an irrational decision, and in so far, **a certain priority of irrationalism**.

(Popper, 1945, p. 218; emphasis added.)

Critical rationalism in contrast to the previous uncritical one is non-comprehensive, and it gives a negative answer to our problem P1. Therefore, for Popper, rationalism is ultimately a matter of irrational faith; irrational by its own standards (if it is irrational or just non-rational, depends on whether the non-circular justification of beliefs, decisions, actions, etc., is a necessary and sufficient condition, or only a sufficient one, for an agent being rational. In what follows I will follow the former and stronger interpretation.)

Critical rationalism is non-comprehensive because this rationalist admits and is **conscious** of his limitations; he and the irrationalist differ in having different irrational commitments, critical rationalism's fideism can be portrayed as a deliberate begging of the question. The critical rationalist is aware of his commitments, while the classical comprehensive rationalist was unaware that he required an irrational decision, comprehensive rationalism was thus naive.

A crisis of integrity then arises for critical rationalism, since its rational identity requires a leap of faith, which by its own lights is irrational or at least non-rational. This form of rationality then provides a rational excuse for irrational commitments; it supplies the irrationalist with the *tu quoque* argument, an argument that says:

... (1) because of logical reasons, rationality is so limited that everyone must make a dogmatic irrational commitment; (2) therefore, the irrationalist (Christian, or whatever) has a right to make whatever commitment he pleases; and (3) therefore, no one has a right to criticize him (or anyone else) for making such a commitment...

(Bartley, pp. 272-3.)

This argument provides an excuse to avoid facing criticisms, it tells that given that rational argument about basic presuppositions, or ultimate commitments, is impossible, then any commitments are rationally possible, and that then even a maximal irrationalism is rationally possible. Popper's answer is that critical rationalism with his minimum of irrationalism is preferable, because,

The choice before us is not simply an intellectual affair, or a matter of taste. It is a moral decision.

... It is my firm conviction that this irrational emphasis upon emotion and passion leads ultimately to what I can only describe as crime. One reason for this opinion is that this attitude, which is at best one of resignation towards the irrational nature of human beings, at worst one of scorn for human reason, must lead to an appeal to violence and brutal force as the ultimate arbiter in any dispute.

(Popper, 1945, pp. 219-21.)

In other words, Popper believes that while the rationalist may not be moral²⁶, the irrationalist is often immoral, and that this gives us one reason to prefer critical rationalism (with its minimal irrationalism) to full fledged irrationalism, Popper then transfers the decision for critical rationalism to the uncertain domain of moral decision. On the other hand, Popper holds that moral decisions can be influenced, though not determined, by a rational analysis of the practical consequences of our

²⁶ The rationalist, however, may need to be moral in a minimal sense of morality, given that rationality is a cooperative process of inquiry which values dialogue, intellectual honesty and humility, respect to other people's arguments, etc.

moral decisions,²⁷ and by contrasting these practical consequences with the prescriptions of our conscience. Popper says,

(...) a rational analysis of the consequences of a decision does not make the decision rational; the consequences do not determine our decision; it is always we who decide. But an analysis of the concrete consequences, and their clear realization in what we call our 'imagination', makes the difference between a blind decision and a decision made with open eyes (...) in the case of moral theory, we can only confront its consequences with our conscience.

(Popper, 1945, p. 220.)

If we assume with Popper that irrationalism, because of its emphasis on passion and emotion, can often lead to what most of us call crime²⁸, then it could still happen that some consciences would prefer irrationalism even when fully aware of its probable immoral consequences, and given this possibility one can only hope that the conscience of most of us will as a matter of fact side with critical rationalism, but if this were not to happen, that would be the end of the matter for a Popperian.

Our epistemic situation so far is as follows: Popperian critical rationalism considers that a consistent theory of rationality must grant that at least its most basic commitment (i.e., its commitment to rationality) has to be accepted without justification that is it has to be accepted dogmatically or irrationally. We arrived to this conclusion because we required of a rational belief or commitment that it be justified *without circularity*, but if instead we were to allow for our *most basic* rational commitment(s) a *viciously* circular justification.

²⁷ Notice that such a rational analysis already presupposes a favourable valuation of an argumentative or rational attitude, therefore a pondered moral decision about whether to be critically rational or not will itself presuppose a favourable valuation of an argumentative attitude. Thus, this most basic valuation cannot be defended by Popper's moral argument, but this is all as well, since critical rationalism openly admits that it has to assume this valuation dogmatically.

²⁸ It is almost a truism that different individuals or communities often don't agree on what constitutes a crime, this becomes clear when one considers the examples of many Nazi SS who would not consider the Auschwitz camp as criminal, or of a XVIII th century slave trader or owner who neither would deem his metier as criminal.

Then we could avoid Popper's irrationalist conclusion. This option would transform our previous C2 to C2':

C2': Only that which can be justified by non viciously circular argument is to be accepted as rational, except for our most basic rational commitment(s) which can be justified by any circular argument.

This way out is logically identical to dogmatism, although psychologically different from dogmatism. This because a straightforward circular justification of some belief p would argue 'p because p' which is identical to a dogmatic iteration of 'p'. Now if the argumentative circle were to have a wider diameter such as 'p because q, and q because r... and z because p' then this circular argument would still be logically equivalent to a dogmatic iteration of 'p', though this wider argumentative circle would be psychologically less obvious. Thus, this alternative to irrationalist dogmatism is not really an alternative at all.

The question now arises of how to distinguish a viciously circular from a virtuously circular argument. A short answer is that a vicious circular argument is a circular argument that intends to be probative, and which is useless as a probative argument, because it needs²⁹ to presuppose that which it wants to prove. That is, in this type of argument, the premisses are as problematic, or as doubtful, or as implausible as the conclusion, and this according to the judgement of the community to which the probative argument is being offered³⁰, such a circular argument is not logically or probabilistically forceful for those who refuse to step into the circle.

²⁹ In other words, in this vicious form of reasoning, the circularity is inevitable, i.e., there are no independent non-circular evidential routes to the conclusion. Otherwise, in viciously circular arguments there is no alternative to the circular route to the conclusion, cf., Walton, p. 309.

³⁰ Thus,

An argument in persuasion dialogue can be useful to persuade or convince the respondent to whom it is directed only if the premisses are already secured as commitments of the respondent **so that for him they are more plausible than the conclusion that he doubts or questions.** This is the implementation in persuasion dialogue of the probative function of argument...This function generally means that in probative reasoning, there is a direction or flow of argument from the premisses toward the conclusion. Certainly this is true in persuasion dialogue because the **premisses must be commitments of the respondent that are, from his point of view, more plausible than the conclusion. They must be, to be useful to prove the conclusion to him successfully.**

(Walton, p. 321.)(Emphasis added.)

Virtuous circularity, on the other hand, is a circular argument without a probative or demonstrative function; a virtuously circular argument is an argument that only intends to show how various elements are interconnected in coordinated relations³¹.

While in a demonstrative argument –such as those of Euclidean geometry- the premisses should not only be more plausible, but also,

... be firmly established as better known to be true than the conclusion to be proved. This context, or one very like it, appears to be indicated in the remark of Aristotle (*Prior Analytics* 64b 30) where it is required of a demonstration that the premisses should be better known or established than the conclusion to be proved from them. In other words, Aristotle was saying that a *demonstration* is a special kind of argument where the premisses are better known than the conclusion. Therefore, in terms of evidence or knowledge, the premisses are prior to the conclusion in a demonstration. Hence it follows that the conclusion cannot be at the same time be prior to the premisses. The inquiry or demonstration is even more strongly inimical to circular reasoning, and apt to classify it as fallacious, than the persuasion dialogue.

(Walton, p. 324.)

³¹ Nicholas Rescher distinguishes different aspects in the rational enterprise two of these aspects are: cognitive rationality and practical rationality. Cognitive rationality is a prescription in favour of justifying our beliefs, while practical rationality or means/ends rationality is the aspect of rationality dealing with actions and can be summarized by the prescription:

If you are going to be rational about your actions, then choose the available justified optimum means M (where justified signifies that the reasons in favour of the belief in the optimality of M should be stronger than the reasons in the belief in contra) to reach your valuable end(s) E.

Rescher then argues (chapter III) that an example of virtuous circularity is provided by an argument from practical reason in favour of cognitive reason. Rescher's argument goes as follows:

- i) A valuable cognitive end E is to find rational answers to our questions, answers that are coherent with the available evidence and which are coherent with our well established theories.
- ii) A policy of justifying our beliefs or answers is the *justified* best available strategy or means for reaching E.
- iii) Therefore justifying our beliefs is the rational action to follow for reaching E, thus means/ends rationality recommends the justification of our beliefs or answers, that is, practical reason can be used to justify cognitive reason.

This argument in favour of justifying our beliefs presupposes an aspect of reason, practical reason, to argue in favour of another aspect of reason, cognitive rationality. Practical reason, however, in turn presupposes cognitive reason when it asks us to *justify our belief* in the optimality of M (notice the 'justified' in italics in premiss (ii) above.) Thus this argument is circular Rescher believes, nevertheless, that this argument is virtuously circular, because this argument shows how different aspects of rationality are inter-related and how these different aspects mutually support each other. According to our characterization of virtuous circularity, one would say that this argument is virtuously circular as long as it is only

For instance, a rhetorical circular argument that intends to be merely persuasive, another case of virtuous circularity is provided by causal argumentation, where there can be diachronic loops that indicate mutual causal relationships among a set of variables. Thus,

... it might be argued that an increase in population in a city causes an increase in modernization, which in turn increases migration to the city, which results in an increase in the number of people in the city. This reasoning is circular, but if it reflects a real situation where mutual causal influences are at work, it should be not be condemned as a fallacious sequence of reasoning.

(Walton, p. 254.)

Now an argumentative justification of a belief in the value of an argumentative attitude is a circular argument that intends to be probative and that presupposes that which is to be proved – i.e., it has premisses as plausible as their conclusion – therefore such an argument is viciously circular³².

POPPER'S NON-JUSTIFICATIONISM

An influential element of Popper's thought (specially of his later thought) is its idiosyncratic non-justificationism, that is, Popper believed that the method of science

intended as an explanation of how the rational enterprise forms a coherent web. Moreover, one would say that this circular argument is not virtuous if it pretends to have a probative or demonstrative function, since its premisses are as doubtful as its conclusion.

On the other hand, notice that Rescher's practical *argument* in favour of cognitive reason already presupposes the justificatory value of argument; precisely what was the ultimate dogmatic assumption of the Popperian critical rationalist, and as the critical rationalist argued, this key supposition cannot be argued for without presupposing it.

³² Otherwise,

...The overall justification of rationality *must* be reflexive and self-referential. To provide a rationale of rationality is to show that rationality stands in appropriate alignment with the principles of rationality. From the angle of justification, rationality is a cyclic process that closes in on itself, not a linear process that ultimately rests on something outside itself...

(N. Rescher, p. 43.)

and philosophy was mainly (if not wholly) negativist and non-defensive. This is clarified when Popper says,

...there is a method which (...) is the one method of all *rational discussion*, and therefore of the natural sciences as well as of philosophy. The method I have in mind is that of stating one's problem clearly and of examining its various proposed solutions *critically*.

I have italicised the words 'rational discussion' and 'critically' in order to stress that I equate the rational attitude and the critical attitude. The point is that, whenever we propose a solution to a problem, we ought to try as hard as we can to overthrow our solution, rather than defend it.

(Popper, 1959, p. 16.)

Moreover, to criticize a theory is not the same as showing that it cannot be justified, that is, that there are no positive reasons for it. Popper believes that these notions have often not been separated, criticism traditionally meant showing that some statement, goal, or method was not properly justified.

... the very idea of criticism is often misunderstood by justificationist philosophers: they tend to whittle down the idea of valid criticism to the narrow task of proving the invalidity of certain attempts to *justify* certain beliefs...

(Popper, 1983, pp. 26-7.)

Popper's critical approach is illustrated by his theory of science; a theory that rejects induction (for Popper induction is a mythical and defensive or justificationist method), an approach that accepts deduction as the only rational method, as the only organon of criticism (for Popper all of our reasoning is deductive or can be reconstructed as such.)

In Popper's view, empirical science grows by a process of conjecture-refutation, where conjectures are guessed answers to scientific questions or problems³³, and where a refutation is an inconsistency between a 'basic statement' and some

³³ Scientific problems arise, for example, when a previously accepted conjecture or hypothesis is refuted.

consequence of our conjectures -plus some initial conditions. The search for refutations is what Popper understands by scientific criticism, though refutations are rarely conclusive. This because observational statements are fallible, and because when a scientific theory is refuted, we have in fact refuted only a complex network of suppositions, and then an ambiguity arises when trying to assign to any of these suppositions the responsibility for the refutation (this is the famous Duhem-Quine problem.)

Now, Popper was not in his 'Open Society' a full negativist as shown by his belief that non-justified presuppositions are irrational, and that therefore critical rationalism was with its dogmatic ultimate commitment minimally irrational.

W. W. Bartley thought it was possible to reform Popperianism into a consistent and comprehensive theory of rationality (Bartley first called it a 'Critical Comprehensive' rationalism (or CCR), and later on he named it a 'Pan Critical' rationalism (or PCR), I will use these two appellatives indistinctly), into a theory that allegedly does not lead into a fideism of ultimate commitments. In other words, Bartley proposed a new rational identity one that allegedly would not lead into conflicts of rational integrity.

PAN CRITICAL RATIONALISM

The pan critical rationalist can be characterized as one,

... who is willing to entertain any position and holds all his positions, including his most fundamental standards, goals, and decisions, and his basic philosophical position itself open to criticism; one who protects nothing from criticism by justifying it irrationally; one who never cuts off an argument by resorting to faith or irrational commitment to justify some belief that has been under severe critical fire; one who is committed, attached, addicted, to no position.

(Bartley, p. 118; emphasis added.)

This pan critical rationalist justifies nothing and allegedly criticizes everything even his own rational attitude or position, he is not committed to any position, not even to

a belief in the value of argument. This doesn't mean that the PCrationalist is without convictions, but only that he is willing to submit his convictions to critical consideration.

Now, since the PCrationalist intends to abandon all justification there are no foundations, justification is substituted by criticism and this last is an open-ended endeavour. Thus, whatever is used to criticize is hold only tentatively and it can in turn be open to criticism, the process is thus potentially infinite.

This perspective admits without any tremors that the principles and standards of rationality cannot be justified rationally, nor does it attempt to justify anything at all using these standards, as a result nothing at all is justified, and there is no such a thing as justified belief inside this perspective.

The PCrationalist holds tentatively all his positions while she goes on searching for their failures, this is why she cannot now specify the form criticism might take in the future, or specify whether some position will be ever successfully criticized (which it might not be, because some positions could, in fact, be correct). Moreover, criticism of a theory or conjecture, can take the following forms:

We have at least four means of eliminating error by criticizing our conjectures and speculations. These checks are listed in descending order according to their importance and the rigor with which they may be applied.

- (1) The check of logic: Is the theory in question consistent?
- (2) The check of sense observation: Is the theory empirically refutable by some sense observation? And if it is, do we know of any refutation of it?
- (3) The check of scientific theory: Is the theory, whether or not it is in conflict with sense observation, in conflict with any scientific hypothesis?
- (4) The check of the problem: What problem is the theory intended to solve? Does it do so successfully?

(Bartley, p. 127.)

It has been affirmed by Musgrave that PCR has a very weak account of rational belief, that for PCR rational belief is merely criticizable belief, and thus according to Musgrave a PCrationalist would believe,

... that the moon is made of green cheese, since this is eminently criticizable. (How do I know ? Because it has been thoroughly criticized.) PCR is so weak that its bound to be rational by its own standards. The adherent of PCR has a 'Heads I win, tails you lose' strategy. If no criticism of PCR is produced, it remains *criticizable* and hence rationally believed. And if an excellent criticism of PCR is produced, this gives further proof that it is criticizable, and hence rationally believed.

(Musgrave, p. 296.)

Musgrave, however, is misconstruing PCR, because Bartley explicitly says:

A position may be held rationally without needing justification at all - *provided that it can be and is held open to criticism and survives severe examination.*

(Ibid, p. 119.)

While comprehensive rationalism was justificationist and comprehensive and therefore ultimately contradictory and naïve, and critical rationalist was non-comprehensive (since she admitted her limitations, since she admitted the need for an irrational kernel) but then provided an excuse for dogmas. Pan critical rationalism is, on the other hand, allegedly comprehensive and non- justificationist, and his answer to our P1 is affirmative. For the PCrationalist all positions are criticizable in the sense that:

... (1) it is not necessary, in criticism, in order to avoid infinite regress, to declare a dogma that could not be criticized (since it was unjustifiable);
(2) it is not necessary to mark off a special class of statements, the justifiers, which did the justifying and criticizing but was not open to criticism;
... (4) the critics -the statements in terms of which criticism is conducted -are themselves open to review.

(Ibid, p. 244.)

According to this position rationality is not logically limited, since there is no logical necessity for dogmas, or leaps of faith. The PCrationalist believes that rationality is

not ultimately impossible, since PCR is not inconsistent or false and because allegedly it is itself rational by its own lights. If so, then the *tu quoque* thrown at critical rationalism can be avoided by PCR, and with it PCR can also avoid arbitrary dogmatic ultimate commitments, because PCR doesn't allow a rational excuse for irrational commitment. Thus,

... for a pancritical rationalist, continued subjection to criticism of his allegiance to rationality is explicitly *part* of his rationalism.

...If a pancritical rationalist accuses his opponent of protecting some belief from criticism through irrational commitment to it, he is not open to the charge that he is similarly committed.

(Bartley, pp. 120-1.)

PCR being a generalization of the Popperian approach, inherits from it all of its characteristics except its non-comprehensiveness, and its lack of integrity. Our new intellectual hero is an all out fallibilist, a full methodological negativist, with one basic aim: to eliminate as much intellectual error as possible. He

...holds his claims open to review even when... **he is unable to imagine, let alone specify, what would count against them.**

(Bartley, p. 234.)

An obvious question now arises, what would be a criticism of PCR, what would count as a criticism of its principle of rationality i.e. of the statement: that all positions are open to criticism. Bartley answers,

...someone could devastatingly refute this kind of rationalism if he were to produce an argument showing that at least some of the unjustified and unjustifiable critical standards necessarily used by a pan critical rationalist were uncriticizable to boot, that here, too, **something had to be accepted as uncriticizable in order to avoid circular argument and infinite regress.**

(Bartley, p. 120.)(Emphasis added.)

This challenge has been taken up...

CRITICISM OF PCR

To get a flavour of the paradoxical character of PCR consider the following argument, due to Bartley himself and inspired by a critique of J. F. Post, an argument that Bartley finds unobjectionable:

(A) All positions are open to criticism.

And because of PCR's intended comprehensiveness it then follows,

(B) A is open to criticism. And,

Since (B) is implied by (A), any criticism of (B) will constitute a criticism of (A), and thus show that (A) is open to criticism. Assuming that a criticism of (B) argues that (B) is false, we may argue: if (B) is false, then (A) is false; but an argument showing (A) to be false (and thus criticizing it) shows (B) to be true. Thus, if (B) is false, then (B) is true. Any attempt to criticize (B) demonstrates (B); **thus (B) is uncriticizable, and (A) is false.** (Bartley, p. 224.) (Emphasis added.)

Hence, PCR is refuted³⁴ and this conclusion is a result of the self-referential character of PCR -a theory that intends to be a theory of all theories itself included, and recalls similar difficulties of classical rationalism, which also wanted to be comprehensive. These difficulties are akin to the paradox of the liar, Bartley reacts by saying,

I am, however, neither surprised nor disturbed to find that a *semantical* paradox of this sort can be produced from my statement of pancritical rationalism... My position refers to itself as criticizable: i.e., it is "self-

³⁴ According to Post this argument is unsound,

...because it relies at one point on the unstated assumption that if a position is false, then there is an argument showing it to be false...(which) is logically equivalent (by contraposition) to 'If there is no argument showing a position to be false, then it is true'. This principle is (...) in any case inconsistent with a realist conception of truth.

(Post, 1987, p. 265.)

referential”. Moreover, my position employs, although not exclusively, an interpretation of criticizability in terms of possible falsity –and thus involves the semantical concepts of truth and falsity. Finally, my position has always been expressed in natural language: i.e., it has not been formalized.

(Bartley, p. 219.)

Bartley believes, however, that the paradoxical nature of PCR could be dealt,

...by type and language-level solutions, Zermelo-type solutions, category solutions, radical exclusion of all self reference...

(Bartley, pp. 219-20.)

This is too vague, mere possibilia, and Bartley doesn't give any specifics of how his previous argument can be invalidated. Post has pointed that such a way out would entitle the acritical comprehensive rationalist to use it too. Thus,

... in response to Bartley's argument (or the *tu quoque*) that justificationism is not itself justified, and therefore is self-referentially inconsistent, the justificationist may reply that such a result really comes as no surprise, in view of the inevitable inconsistency of natural language, within which such a theory as his must be expressed. He may ... adopt a language-level solution to the paradoxes, according to which he should not be interpreted as holding a literally self-applicable position in the first place, so that it is unfair to demand a justification for justificationism itself.

... the justificationist could rightly reject the whole of Bartley's self referential argument against justificationism, so that much of the *Retreat to Commitment* would make no sense.

(Post, 1987, pp. 265-6.)

On the other hand, Bartley argues that the conclusion of his argument that (B) is uncriticizable doesn't turn (B) into a dogma to which a PCrationalist must be committed, because (B) could be made criticizable once the paradox is dealt with by the means suggested in the previous quote thus,

The mere possibility of such a solution to the semantical paradoxes makes (B) criticizable after all: it suggests a potential means for invalidating the argument that produces the conclusion that (B) is uncriticizable.

(Bartley, p. 225.)

J. Post retorts that such a strategy would decrease the content of PCR and it would go against PCR's goal of being a theory of all theories itself included, Post says that Bartley,

... would avoid the refutation, but at the expense of the decreasing the content of the theory, thus making it less criticizable. For a *theory about all theories including itself obviously has greater content than a theory about all other theories, and obviously takes the greater risk of refutation...* this sort of revision... is inconsistent with the aims of PCR...

(Post, 1971, pp. 52-3.)

Bartley's answer is that,

An argument about content cannot be used to eliminate in advance any language-level route out of the paradoxes. High content ... is indeed an important desideratum of any theory, but obviously does not possess overriding force.

(Bartley, p. 226.)

Furthermore, Bartley argues that even if it were to prove impossible to avoid the paradoxical character of PCR, Bartley would not have to abandon the claim that all positions are criticizable,

... My characterization of the rationalist would remain essentially intact...The only "uncriticizable" statements he would harbor would be "uncriticizable" in a different sense: those forced on him by semantical paradox in the course of *rational* argument using natural language; and he

would neither be committed to these nor have been led to them by faith. Nor would they have been forced on him by the need to stem an infinite regress. ... *nothing in the semantical paradoxes invokes limits to rationality or requires ultimate commitment-not even to the statement (B) that is "uncriticizable"*.

...The uncriticizability of (B) would in no way show that the rationalist must be committed to it, or that he must make a dogma out of it, or that he could use it to cut off argument about some contested position.

...Previously, the question was whether something had to be accepted as uncriticizable *in order* to stem infinite regress and avoid circular argument. Whereas the kind of uncriticizable statement that is forced on one by the semantical paradoxes is of no use in stemming infinite regress and circular argument.

Even if someone did make an article of faith or dogma out of (B), there is virtually nothing he could do with it. (B) has insufficient content to be used to justify other claims.

(Bartley, pp. 230-1.)(Bolds added.)

When Bartley says that the "uncriticizability of (B) would in no way show that the rationalist must be committed to it" he is characterizing criticizability as a psychological attitude, as a way of thinking, as a mental object in Popper's World II. We are being told that one can have a critical *attitude* towards statement (B) even when one is fully aware that (B) is a statement that must remain objectively uncriticizable if one is to avoid paradox. But then how can the PCrationalist hold open to criticism a statement that she already knows is in fact uncriticizable? This looks as only a psychological possibility, but to properly answer this last question it is necessary to further explore what Bartley understands by criticizability.

WHAT TO UNDERSTAND BY CRITICIZABILITY

Bartley has characterised a PCrationalist as "one who is willing to entertain any position and holds *all* his positions... open to criticism." The PCrationalist is then not enamoured of any position and for him no position has privileges or benefices vis a

vis criticism, whenever and in whatever form criticism might eventually arise. The question then surfaces of how a PCrationalist could hold open to criticism what she already knows is objectively uncriticizable, items such as: the previous statement (B), empirical truisms³⁵, necessary truths, the central deductive logic required by all criticism, or the most basic methodological assumption of the PCrationalist, i.e., the value of argument for criticism. Bartley provides an answer,

... one of the merits of pancritical rationalism... that it presents a theory about people, not statements... statements are intrinsically neither rational nor irrational... rationality is not a property of statements but is a matter of the way in which a statement is held, and also of the history of that statement, of the way in which the statement has been examined. Thus pancritical rationalism does not involve, and I have never developed, a theory of rationality as a property of statements.

(Bartley, pp. 233-4.)

Hence, PCR is the attitude; it is the mental disposition of certain ideal persons, of the rational ones, of those persons that take argument seriously, for the PCrationalist,

... all statements are criticizable -not just the rational and noninferential ones, (...) all positions (including necessary truths) are criticizable, ...

(Bartley, pp. 238-9.)

Now, since necessary truths are held to be criticizable it follows that for PCR criticizability and possible falsity are not always linked, otherwise, it is not always the case that for a statement to be criticizable, it is for it to be possibly false, although,

³⁵ Empirical truisms are statements such as: "I have a head" and "the present living Queen Mother is more than ten years old," these are statements of which we are certain, these are statements which we are incapable of psychologically doubting, cf., Watkins.

... for a large range of statements one main thing that one does in criticism is indeed to attempt to show falsity, and that one therein usually does assume possible falsity. That is, for a large class of statements, to hold a statement open to criticism is to conjecture or guess that the statement may be wrong and that some day some effective criticism, the nature of which we cannot even imagine today, may be produced against it. But that is not all there is to criticism.

(Ibid, p. 240.)

What else is there?

...[Criticism] may be also a matter of the revision, the narrowing or stretching and adjustment of concepts...Or it may be argued that someone did not prove what he set out to prove -not that what was proved was false or possibly false. Or that the statement of an argument or proposition is inelegant or uneconomical.

(Bartley, p. 240.)

For Bartley a position is an unrevisable or fixed presupposition of PCR if it cannot be abandoned without weakening critical argument,

A position would be unrevisable (in argument) if and only if there are no circumstances under which it can be abandoned without weakening the exposure of the system as a whole to criticism. Such unrevisable positions, if any, would mark the absolute presupposition of critical argument.

(Ibid, p. 253.)

These fixed positions are nevertheless still potentially criticizable, although they will never be successfully criticized, because to argue, to criticize them per force we will require them. How can we, for example, criticize the core or most basic principles of deductive logic? Do not we have to assume them dogmatically? (As Lewis Carroll pointed with his 'Achilles and the Tortoise'.) Isn't then the PCrationalist committed to deductive logic? Bartley grants that a minimal logic (he calls it logic I) is

presupposed by all argument,³⁶ and that it is thus unreviewable, unrevisable, but he insists that logic I is not uncriticizable³⁷; therefore, he concludes that Logic I is not hold dogmatically after all. Thus,

The fact that argument presupposes a minimal logic **as unrevisable in no way identifies a commitment** on the part of a pancritical rationalist. To be sure, the practice of critical argument and logic are bound up together. One can no more argue without a minimal logic than one can live without breathing or speak without language. None of these three -living, speaking, arguing- require irrational commitment to a dogma...

(Ibid, p. 260; emphasis added.)

We could of course reject 'logic I', but only if we reject the practice of argument with it. Now, Bartley appears to be confused when he says: "... in no way identifies a commitment on the part..." Bartley has a voluntarist theory of criticism in which criticism is the will to doubt every thing, and hence he understands commitment as a subjective state of mind, as an entity of Popper's World II. The interesting question is, however, whether this minimal logic is an objective dogmatic presupposition, i.e., whether it is a dogmatic presupposition in Popper's World III. In other words, does PCR need logic I as an ultimate, uncriticizable, and objective presupposition, even if unconscious? Again, the question is not whether the PCrationalist is in love with logic I, but whether he needs it to conduct any criticism, Bartley answered in the affirmative, when he said in his last quote that logic I is unrevisable.

³⁶ Bartley's minimal logic includes, for instance, *modus ponens*, *modus tollens* and the law of no contradiction; these presuppositions are part of Logic I because they cannot be abandoned without hindering our ability for criticism.

³⁷ So for Bartley revisability and criticizability mostly agree except in for example the case of Bartley's minimal logic, which is not revisable but allegedly is still criticizable. Worrall has argued that ultimately there is only one universal and timeless fixed kernel of principles of rational, or scientific, assessment (something like Bartley's Logic I, plus a few other rules such as: 'whenever possible, theories should be tested against plausible rival theories.' Cf., 'The Value of a Fixed Methodology', p. 273.) This kernel would be shared by the nuclei of all scientific research programs; Worrall asserts that this common minimal kernel is 'strictly fallible' but not 'seriously corrigible'. Worrall's position is then stronger than Bartley's on Logic I, which though subjectively criticizable it is not revisable.

Moreover, how can one be critical of logic I if one knows beforehand that it is uncriticizable? To say that one is subjectively critical of something one already knows that is objectively uncriticizable looks like a rhetorical way of saying that one is not enamoured of logic I (or of necessary truths, or of our previous statement (B)). It is a rhetorical way of saying that one wants to be open minded even about what one knows as objectively uncriticizable.

The PCrationalist wants to say that she is ready to keep on criticising logic I even when she knows that she cannot succeed because logic I is objectively uncriticizable, even though, the PCrationalist already knows that criticising logic I is a futile enterprise.

In other words, the PCrationalist knows that to successfully criticize logic I is an impossible goal, furthermore this is an impossible aim that can neither be approximated, because one either successfully criticizes logic I or one doesn't. Goals known both to be impossible to attain and to approximate are likely to be irrational, even if they are held to be valuable³⁸. Therefore, when the PCrationalist says that she can go on being critical of logic I she is aiming at an irrational goal and hence she is not being rational.

As another example, imagine that I realize that 'p or not p' is a logical truth, and that I therefore realize that this logical truth is not objectively criticizable (i.e., that I know that no criticism of this truth can be successful or even partially successful) it is then irrational to insist in criticising it.

Furthermore, would Bartley say that someone acquainted with the objective uncriticizability of necessary truths and who therefore strongly believes in the truth of 'p or not p' is irrational? Bartley would probably answer 'yes', since this person would have made a subjective commitment in favour of this disjunction. Now, to call

³⁸ More precisely, rational goals are valuable aims that are either reachable, or if known to be unreachable are then continuously or asymptotically approachable, this because impossible and not continuously approachable goals are aims for which there wouldn't be any methods to reach or approximate them, and such methods should be available if one is going to be means-ends rational. Means-ends rationality or practical rationality is captured by the following conditional scheme:

If you are going to be means-ends rational, and if you want to attain or approximate a valuable goal G, then choose the optimum means* amongst those means available to you to reach G.

* The belief in the optimality of the means chosen should be also a rational belief; the rationality of aims will be further discussed in chapter five.

somebody irrational for strongly believing in the truth of a proposition after realizing that it is a necessary truth (or an empirical truism) is at least counterintuitive.

Bartley's minimal logic constitutes then a fixed presupposition for the PCrationalist and it is analogous to what Wittgenstein called a 'hinge':

... the *questions* that we raise and our *doubts* depend on the fact that some propositions are exempt from doubt, are as it were like hinges on which those turn.

(*On Certainty*, 341.)

This even though Bartley says,

... we deny that it is logically necessary to trust something –a “hinge” as it were- that is beyond doubt.

(Bartley, in Radnitzky & Bartley, p. 212.)

There is no 'hinge' beyond doubt, if doubt is taken as an abstract volitional state of mind, as a state of open mind ness, but as Lakatos has pointed out:

... the basic weakness of this position is its emptiness. There is not much point in affirming the criticizability of any position we hold without concretely specifying the forms such criticism might take.

(Lakatos, footnote 29, p. 264.)

A 'hinge' is beyond doubt once it is realized that the practice of critical argument logically requires some fixed presuppositions, this becomes specially clear when one goes to ask how would the PCrationalist criticize his most fundamental methodological assumption: that argument is valuable, since argument is the crucial requirement of any criticism. Any rational criticism of this crux methodological assumption would per force use arguments; therefore it is not possible for the PCrationalist to successfully criticize the practice of argument. In conclusion, in spite

of Bartley, it appears that PCR has uncriticizable components or assumptions and that therefore PCR cannot be comprehensive or pancritical.

POST'S REFORMULATION OF THE PARADOX

J. Post (Cf., Post, 1987, pp. 262-4) has proposed an alternative formalisation of PCR, a formalisation that is a revised version of Post's older criticism of PCR (cf., Post, 1971) and which has taken into account numerous objections of Bartley to Post's older paper.

Post's new formalisation takes into account, for example,

...matters of personal and historical context and that [PCR] is not really a theory about statements in the first place, but about one's attitudes and/or positions even when the latter are not expressible in statements.

(Post, 1987, p. 261.)

This new formalisation can be summarized as follows:

(A1) Consider a person P, a context K, a time t, and an attitude, belief, or position X (expressible or not) which is problematic (or up for possible revision) for P in K at t. Then P holds X rationally in K at t only if: P holds X open to criticism at t, and (so far as P can then tell or guess) X has at t so far survived criticism.

In addition, A1 entails B1:

(B1) P holds X rationally at t only if P holds X open to criticism at t.

Let P* be the PCrationalist, now if P* is to hold B1 rationally at t, that is, if P* is going to hold B1 open to criticism at t, he will conjecture that B1 may be wrong and that,

(C1) There is a (potential) criticism of B1, which might someday be produced and be seen to be successful.

But if C1 is to be held rationally by P*, then by B1, P* must hold C1 open to criticism, and thereby conjecture that there is a (potential) criticism of C1, which might someday be produced and be seen to be successful, in addition,

... if criticism were to persuade P* that C1 fails –that there is no (potential) criticism of B1- then he could not genuinely hold B1 open to criticism, which is what C1 says. Hence in the present K, for P* a failure of C1 to be correct would mean that B1 fails to satisfy its own standard ... Thus in K, for P* every consideration which counts against C1 would count against B1, so that for P* every criticism of C1 is a criticism of B1.

(Post, 1987, pp. 262-4.)

That is,

(1*) For P* every criticism of C1 is a criticism of B1, whether or not the criticism might someday be produced and be seen to be successful.

Furthermore,

(2*) For P* no criticism of B1 is a criticism of C1.

This because,

P* would recognize that the existence of a (possibly successful) criticism of B1 would entail C1 (since that is what C1 says), so for P* this criticism of B1 could hardly count also as a criticism of C1.

(Post, 1987, p. 263.)

Now,

From (1*) and (2*) it follows at once that for the Critical Rationalist P*, there is no (potential) criticism of C1 after all, ... then for P* C1 is a strict counterexample to B1, which therefore is false (or invalid) even for P*...

(Ibid, p. 264.)

Post then concludes:

Next, suppose P* has good reason to hold A1 in K (say because P* rightly holds that A1 has so far survived criticism). Then P* has good reason to hold B1 and C1, assuming he knows that A1 entails B1 and agrees that in K, B1 implies C1. But C1 is either rational for P* by A1's standards or not. If it is, then B1 is invalid for P*, as just seen, as is A1, and P* must conclude that A1 does not survive the present criticism; hence A1 would be self-referentially inconsistent as regards survival. If C1 is not rational for P* by A1's standards, then C1 is an example of something which P* has good reason to hold if he has good reason to hold to A1, but which P* does not rationally hold by A1's standards; hence A1 would be incomplete, and inherently so (since A1 implies C1). Thus we are forced to conclude that unless the Critical Rationalist P* has no good reason to hold A1 in the first place, A1 is either self-referentially inconsistent or inherently incomplete...

(Post, 1987, p. 264.)

Where self-referentially inconsistent means,

A is a self-referential theory which implies that it possesses a certain property -the property of having survived criticism- which A turns out not to possess after all. Such a theory is said to be "self-referentially inconsistent."

(Post, 1971, p. 228.)

And where inherently incomplete means,

We call a principle or theory of rationality incomplete iff there is a statement we have good reason to hold but which it is not rational to hold according to the theory's standards. We call the theory inherently incomplete

when there is such a statement not "external" to theory but implied by the theory itself (as C is implied by A via B).

(Post, 1987, p. 254.)

Bartley reacted to this new formalisation of PCR by protesting (ibid, p. 238) that Post had not yet grasped the core of his position, and that therefore his theorem was irrelevant for PCR. Bartley argues that Post's A1 differs from the core statement of Bartley's PCR, that is,

(A') Every position which is held open to criticism and survives severe examination may be held rationally. (And there is no need to go into the question of its justification.)

(Bartley, p. 237.)

Bartley says that Post's A1 and his A' differ, because Post's A1 "reverses and crucially alters" A' and then,

Post's B does not follow from my A'. Nor does a reversed version of B follow from A'.

(Bartley, p. 237.)

Bartley also mentions that while he considers openness to, and survival of, criticism as sufficient conditions for rationality, Post regards them also as necessary, Post does this because,

... suppose criticizability and survival of criticism were not necessary but only sufficient for rationality. Then indeed it would be possible to be a rationalist. In fact it would be all too easy to be one. For then arbitrary dogma, shielded from every criticism, could not be condemned as irrational on the ground that it is uncriticizable; dogmatists and fideists could legitimately claim to be rationalists. This absurd outcome can be avoided only if one's theory of rationality states some necessary condition that arbitrary dogmas and leaps of faith will not satisfy.

(Post, 1987, p. 267.)

Bartley, however, avoids this last debate by saying that,

The question whether these criteria are necessary or sufficient could be argued at length. I do not propose to do so here. For as I have shown [cf., Bartley's previous argument showing the paradoxical nature of PCR] a similar paradox can be generated without reference to this question.

(Bartley, p. 238.)

MILLER'S DEFENCE OF PCR AS NON PARADOXICAL

Let's recall Bartley's shortened version of PCR at the start of this section, Bartley characterized PCR as:

(A) All positions are open to criticism.

And since PCR intends to be comprehensive it then follows,

(B) A is open to criticism.

This characterisation of PCR leads to self-referential paradox, as discussed previously in the first part of this section. Now, David Miller outlaws self-reference (and in this way he avoids PCR's paradoxical nature) by distinguishing between positions and statements and declaring that (B) is just a statement -and not a position- and as such it is not in the domain of (A), that is (B) being a mere statement it doesn't have to be criticizable on its own,

...I reject the thesis that criticizability is an automatic property of all statements. It is not an intrinsic property of statements at all, but an honour that must be bestowed on them by the development of appropriate methods of criticism. How, it may be asked, is this to be done? In many cases the answer can be only: by a consideration of the problems that provoked them... Those statements that are the response to no problem are accordingly the statements that it may be most difficult to criticize.

...CCR must not be understood to hold that every statement that a comprehensively critical rationalist counts as true (rationally accepts) **is on its own criticizable.**

...As far as statements... are concerned, what is important for the rationalist, I suggest, is that each statement that he accepts either is itself criticizable **or follows from a statement** that he accepts that is criticizable. Any position adopted must be criticizable, but it is no concession to the irrationalist to allow that some logical consequences of the position may not be criticizable.

(Miller, pp. 86, 89.) (Emphasis added.)

Miller's proposal is motivated by the fact that falsifiable statements can entail unfalsifiable ones,

All falsifiable hypotheses have amongst their consequences a host of unfalsifiable statements (ranging from tautologies and unrestricted existential statements to meaty metaphysics) that enter science as it were on the coat-tails of their parents. But these unfalsifiable consequences –to the extent that that is all that they are- are not scientific in their own right; their title is one of courtesy. If their parents are rejected from the realm of scientific knowledge, they will have to be rejected too.

(Miller, p.10.)

Metaphysical determinism is, for example, a consequence of Newtonian theory, and in the case at hand, *mutatis mutandi*, an uncriticizable statement is allegedly entailed by a criticizable position,

The comprehensively critical rationalist may not take up any position that he is not prepared to subject to critical examination. But though the critical examination of a hypothesis is normally conducted by critically examining **some** of its consequences, it is not conducted by examining **them all**. The rationalist is not at all obliged to try to criticize all the consequences of his ideas, and if, as may happen, some of them are not open to criticism at all –

which may mean no more than that he cannot think of any way, even potentially, of criticizing them –that need not disturb him...

In these terms [(A)] is a position that Bartley recommends that we adopt, and it is essential that it be criticizable. But [(B)] is just a consequence of it –an interesting consequence, in the light of what CCR says, and (one hopes) a true consequence; but it cannot be taken up as an independent position ... although [B] is a possible position on its own (though a strange one), it **cannot be adopted at the same time as [(A)] is. Nor, of course, is there the slightest need for it to be adopted along with [(A)], which brings it along for nothing.**

(Miller, p. 90.)(Emphasis added.)

Miller claims that to believe otherwise is to be victim of a philosophical prejudice, a prejudice that takes for granted the transmissibility assumption, an assumption that holds,

...that all properties, measures, and tokens of intellectual value or merit are transmitted from premisses to conclusion, in the same manner as truth, through the relationship of logical derivability or deducibility.

(Bartley, p. 261.)

Truth and probability are of course logically transmissible from premisses to conclusion via the deducibility relationship, but this is not the case of many other epistemic virtues or properties, such as falsifiability and criticisability.

Miller's demotion of (B) to statement status looks, however, suspiciously like an ad hoc manoeuvre or a stipulation that has to be accepted dogmatically. Why should we not take (B) as a position simultaneously to (A)? In other words, is there are a criterion that will allow us to distinguish between positions and statements? Still, when do positions entail positions, and not just statements? And which statements are criticizable on their own, and why? Furthermore, when Miller says that there is not "the slightest need for (B) to be adopted along with (A), which brings it along for nothing", he seems to be giving up the comprehensive intention of PCR, a critical

comprehensiveness which was PCR's claim and aim and which distinguished it from the older non-comprehensive Popperian critical rationalism.

If PCR abandons its comprehensive character it is giving up what was allegedly its great virtue, what was its main advantage over critical rationalism, and this diminished PCR could then be confronted by a new *tu quoque*,

If you pancritical rationalist don't hold criticizable some of your assumptions (or if you prefer statements), then neither I, an irrationalist, need open to criticism some of my dogmatic suppositions or statements.

If there is no adequate answer to these questions (and none is offered by Miller³⁹) then it looks that the matter will be settled by someone's (either an individual's or a community's) idiosyncratic judgment. It thus appears that Miller has transformed (B) into a dogma (or to use Miller's euphemism a "statement") only to avoid paradox. Bartley's and Miller's various moves look as what I. Lakatos used to call a degenerating problem-shift, a stubborn defensive strategy whatever the cost.

III RELATIVISTIC IMPLICATIONS

Several theories of rationality have been reviewed so far and we have found all of them wanting or at least in difficulties, thus:

- i) We concluded that classical comprehensive rationalism was false because it was inconsistent.
- ii) We found that critical rationalism was non-comprehensive and hence open to the attack of the *tu quoque* argument.

³⁹ In contrast, Popper proposed a theory of falsifiability in which, for example, 'pure existential', 'all-some' and tautological statements were unfalsifiable due either to their syntactical or semantical structure (cf., Popper 1959, section 66.) Miller, on the other hand, has not offered any theory of criticizability that would provide some sort of demarcation criterion between positions and non-criticizable statements. Except for a hint at a pragmatic criterion, thus Miller claims that while positions are created as possible solutions to problems statements don't intend to solve any problems. And what is a problem is something to be decided by the argumentative context at hand (private communication.) The criticism of statements or positions can only be done by a 'consideration of the problems that provoked them.' (Miller, p. 86.)

iii) Finally, we discovered that pan critical rationalism was paradoxical. It seems then that,

Any rationality theory is rationally acceptable by its own lights (circularity), or by other lights (potential infinite regress and actual circularity again), or is not rationally acceptable at all (irrationalism).

(Musgrave, p. 297.)

We also found ourselves with unjustified basic scientific methods (that is, with methods without a non vicious circular justification), methods such as induction and deduction. As Wittgenstein said:

At the foundation of well-founded belief lies belief that is not founded.

(*On Certainty*, 253.)

Or, as John Worrall has put it:

Indeed not only must the basic principles of scientific method ultimately be adopted dogmatically, so must those of deductive logic...

... if the sceptic really presses, then the only option is, I believe, the honest admission that ultimately we must stop arguing and 'dogmatically' assert certain basic principles of rationality. If (...) this honest admission entails relativism, then relativism wins.

(*Fix it and be Damned*, pp.382-3.)

Worrall's position⁴⁰ is similar to that of Popper's critical rationalism with its sophisticated faith in reason. If rationality and science require, however, of dogmatic

⁴⁰ One possible way to understand Worrall's position is to consider all of science as a scientific research program:

Even science as a whole can be regarded as a huge research programme...

(Lakatos, p. 132.)

Worrall would then claim that in the nucleus of this huge SRP will be some fixed methodological principles and cognitive values, this core, however, will have a dogmatic

presuppositions, this leads to relativism, because Popper's and Worrall's minimal dogmatism exposes the rationalist to the irrationalist's tu quoque argument, and this provides an excuse for diverse unjustifiable dogmas. All these dogmas will be irrational and arbitrary in the eyes of Reason⁴¹, and one then ends with the possibility of a Babel of mutually inconsistent dogmas, all viciously circular self-justifying, all alleging to be right -though only by their own lights. And since all of one's rational positions are ultimately justified or defended via these dogmas, one ends with a relativism of foundations, a relativism which can infect the whole intellectual structure resting on them. The process that leads to this foundational relativism might be summed up as follows:

- i) Whenever rational agents (or a scientific community) assess theories, they presuppose some standard (implicit or explicit) according to which these theories are judged to be true or false.
- ii) The question then arises of how to assess the standards themselves, and we discover that this leads us into a regress, and we discover that in the end, there is no further standard to which appeal can be made in order to determine if our most basic standards are correct.
- iii) Where the condition described in (ii) obtains, it is nonsense to speak of basic standards as non-circularly correct, such fundamental standards can only be dogmatically reiterated and described.
- iv) Hence, a decision to accept or reject some fundamental standard (once vicious circularity is excluded as a justifying strategy) must of necessity be arbitrary and a relativism of standards follows.

Thus, imagine two communities C and C* of which it is the case that there is a difference of fundamental standards. It follows that one cannot assert without vicious circularity that C (or C*) provides the proper standard, to do so would be illegitimate, and it also follows that there is no other way in which C can be judged superior to C*. Premiss (i), might be avoided by doing without standards, but then we would have subjectivism. One could also argue that though differences in ultimate standards are a logical possibility they are a psychological impossibility, but

character because it cannot be justified without vicious circularity, and not because of a conventional decision of some negative heuristic.

'scientific creationists' show that it is not psychologically impossible to have different ultimate epistemic standards, hence foundational relativism seems to be both logically inevitable and a psychological reality.

CONCLUSION

Why should we take the two questions at the beginning of this chapter seriously? We should, if we want to clarify to ourselves why the methods of inquiry, in general, and of science, in particular, have any thing to recommend them, if we want to understand the nature of rationality (and in particular, of scientific rationality.) But then, why is it important to understand the nature of rationality? Because, rationality is:

... a crucial component of the self-image of the human species, not simply a tool for gaining knowledge or improving our lives or society. Understanding our rationality brings deeper insight into our nature and into whatever status we possess.

(R. Nozick, p. xii.)

And because these sceptical doubts threaten our responsibility as inquirers. Assume, for instance, that we were compelled to adopt certain goals or to pursue them in a certain way, this would question our confidence that we are free and fully responsible agents.

Further, if we were not able to give an adequate answer to these problems it would seem that the rational project, in particular, the project of science, cannot be carried out in a wholly reflective manner, then either of three choices would be left:

A maximal irrationalism -which as argued by Popper might be morally unacceptable. Or the conformism of a radical suspension of judgment, in which we passively accept traditional beliefs and behaviours without actively believing or supporting them. Or the resigned acceptance that we cannot rationally justify rationality, that we cannot be, even in principle, wholly rational agents, that dogmas are logically inevitably and necessary. Therefore, we would end with a more modest

⁴¹ These dogmas are arbitrary only from the perspective of Reason and not in an absolute sense, since they may be governed by accidental social or biographical circumstances.

appraisal of ourselves as inquirers, that is, a more modest appraisal of the possibilities of argument.

Whatever our answer, to these problems, they may help us to arrive to a cognitive position with less self-deception, with less self-complacency. But even, if no answer were to be arrived at, the search would have been worthwhile since: the search, the struggle, is of itself valuable.

The search says more than the discovery.

(St. Augustine.)

The search of justification is limited because there cannot be a presupposition less starting point, and this logical fact is independent of any pragmatic limitations to the search of justifications. All our justificatory chains lead into an infinite regress, which can only be stopped with vicious circularity or dogmatism, because of this situation in the end our most basic beliefs are in fact dogmatically taken for granted.

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CHAPTER II

CAN A LOCALIST AND DESCRIPTIVE NATURALISM AVOID DOGMATIC FOUNDATIONS?

...In my naturalism, I recognize no higher truth than that which science provides or seeks.

(W. V. Quine.)

I

It could be argued that our approach was unreasonably over ambitious when discussing the logical limits to the justification (or criticism) of rationality and scientific methodology in the previous chapter, since we looked for answers to global questions, since we searched for ultimate justifications for our theories of rationality and for our scientific method(s) and aims. It could be argued that instead, one should have stayed contented with limited, with particular, or local problems, because our science, and the methods presupposed by it, have generally been successful.

Therefore, it could be claimed that it is unreasonable to entertain global doubts about our science and its methods and aims, and that it is then unreasonable to ask for ultimate justifications for these methods and aims.

Otherwise, it could be argued that since our science and its methods have been overall effective in predicting, in giving us control over, some aspects of nature; that since our science has in general provided us with reliable knowledge, why doubt our science and its methods, why search for their global or ultimate justifications?

Especially, since these sought justifications are likely to be unavailable.

A recommendation for local, for restricted questions, can be aptly illustrated by the well known metaphor of Neurath's, of a boat that is constantly repaired and improved while always navigating in the open sea; and never being taken to port for a complete overhaul. This boat can only be changed or repaired piece meal and this is done only when required; the proposal is that we do the same with our system of knowledge,

that we question and revise it only here and there and only if serious doubts were to demand it. The localist believes that local or particular justification is all we need to be rationally justified about our corpus of knowledge.

Localism is epistemologically optimistic since it believes in potential unlimited improvement: it assumes that our methods, goals and beliefs can in principle proceed indefinitely with a continuous and gradualist process of betterment. Localism then assumes that there are no large-scale errors or gaps in our current corpus of knowledge. Localism assumes that in general, our background knowledge is correct, it has to assume this, because this is the prerequisite to go on with a reformist approach. This conservative assumption of localism is the prerequisite for not doing a general overhaul; this optimistic assumption is the prerequisite for ignoring global questions. Thus,

We can change it [our conceptual scheme] bit by bit, plank by plank, though meanwhile there is nothing to carry us along but the evolving conceptual scheme itself.

(Quine, 1953b, p. 78.)

And in case our conceptual scheme were to be confronted with anomalous empirical evidence, then the localist conservatively recommends to accommodate the empirical anomalies with a minimum of alteration to our conceptual scheme.

Our boat stays afloat because at each alteration we keep **the bulk** of it intact as a going concern.

(Quine, 1960, p. 4.) (Emphasis added.)

Thus, we revise some of our particular beliefs while taking for granted the general validity of the bulk of our scientific procedures and results, while taking for granted our everyday common sense beliefs. These basic common sense presuppositions are akin to the ship's hull, they are what keep the boat afloat; thus, local questions (scientific and normative epistemic ones) are examined against background knowledge, a background that at least for the time being is considered as non-problematic and as consensual. For example, particular features of our methods of

inquiry may be evaluated against a framework of accepted common sense beliefs, scientific theory, and some basic methods (such as induction and deduction.) This framework is not questioned: *if the ship keeps on navigating* we just tinker with it. The assumption is made that those of our beliefs and methods that as a matter of consensus have worked can be taken for granted; that they are presumptively true or correct. That is, it is assumed that they are innocent until proven guilty, it is thought that to question them would be an unnatural and unfounded doubt. Thus,

We cannot begin with **complete** doubt ... A person may, it is true, in the course of his studies, find reason to doubt what he began by believing; but in that case he doubts because he has **positive reason** for it, and not on account of the Cartesian maxim. Let us not pretend to doubt in philosophy what **we do not doubt in our hearts**.

(Charles S. Peirce, pp. 228-9.) (Emphasis added.)

Peirce's idea appears to be that we should do not take seriously doubts that we cannot entertain psychologically (i.e., "in our hearts"), and Peirce believes that we should avoid global or complete doubt, he also claims that doubts must be justified. According to Peirce, when one has a genuine doubt, it is because one has specific reasons for doubting, and then one can examine these reasons to find whether they are good reasons for doubting. Peirce's proposal is to deal with real (i.e., local or restricted) and justified questions or problems, rather than with invented wider problems which we cannot entertain psychologically, as those wider doubts proposed by Descartes in his *First Meditation*.⁴²

⁴² For the pragmatist tradition, even amongst restricted questions, deliberation can be excessive, because too much deliberation interferes with a successful life; hence, some rashness is necessary for survival. Thus,

There is no more miserable human being than one in whom nothing is habitual but indecision, and for whom the lighting of every cigar, the drinking of every cup, the time of rising and going to bed every day, and the beginning of every bit of work, are subjects of express volitional deliberation.

(W. James, p. 122.)

Deliberation is for the pragmatist primarily a means of solving *particular* human problems and it is not carried for its own sake.

The pragmatist tries to avoid (or is it evade?) the sceptical questions of the last chapter by focusing exclusively on local or particular questions. The localist advises us to emulate the alleged attitude of Kuhnian 'normal' scientists who take the presuppositions of their paradigm for granted, and only doubt their paradigm if they have good reasons for doubting it, reasons which for normal scientists can be persistent and numerous important anomalies. The localist wants to reform philosophy so that it imitates normal science by proceeding in a piecemeal fashion, that is, by taking for granted those background assumptions that have the backing of experience, by taking for granted those assumptions that have the backing of scientific tradition.

If we call a 'normal world' a world that is consistent with our general common sense beliefs about how the world is, then the localist is saying that we should be interested in doing science and philosophy in 'normal worlds'. The localist is prescribing that we should not be interested in the difficulties of acquiring knowledge in some bizarre logically possible world inhabited by a malicious Cartesian demon, or in an outlandish world of brains in vats.

II

Quine has defended a holist thesis, and as result of his holism, Quine has questioned the synthetic-analytic distinction,

If (holism) is right ... it becomes folly to seek a boundary between synthetic statements, which hold contingently on experience, and analytic statements, which hold come what may. **Any statement can be held true come what may**, if we make drastic enough adjustments elsewhere in the system. Conversely, by the same token, **no statement is immune to revision**.

(Quine, 1953, p. 43.)(Emphasis added.)

Hence, for Quine, any statement is revisable, this implies that for Quine philosophical beliefs are also modifiable by experience, and then the boundary between science and philosophy (and in particular, between science and epistemology) becomes blurred. On the other hand, it is thought that science

proceeds by asking local or particular questions, and that when doing so, science takes for granted its background knowledge and methodology (except if good reasons to doubt some of its background presuppositions were to appear here or there.) Moreover, the pragmatist highly values science and its restricted investigations because of their practical results. As a result, Quine claims that a gradualist localism is the way to proceed in all our investigations, such as epistemological and scientific ones, and epistemology is to be appraised by the method(s) of science. And since science and philosophy are thought to form a continuum, sceptical challenges should arise *within* science, and we should use science to respond to them. Thus,

... **skeptical doubts are scientific doubts** (...) Epistemology is **best** looked upon, then as an enterprise within natural science. Cartesian doubt **is not** the way to begin.

(Quine, 1975, p. 68.) (Emphasis added.)

For Quine there is no 'first philosophy', that is, for Quine there is not a philosophy that is logically prior to any empirical knowledge. For Quine, there are not extra-scientific methods to assess from some place outside science, the epistemological merits of scientific theories, thus,

... **Naturalism: abandonment of the goal of a 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...** Naturalism does not repudiate epistemology, but assimilates it to empirical psychology (...) [The naturalist] tries to improve, clarify, and understand the system **from within**. He is the busy sailor adrift on Neurath's boat.

(Quine, 1981, p. 72.) (Emphasis added.)

Epistemology becomes the study of science from within science, and in this way epistemology loses its special character, for this doctrine (from now on ‘naturalism’) the empirical sciences⁴³, their methods and results are what guide philosophy. Thus,

Science itself, in a broad sense, **and not some ulterior philosophy**, is where judgment is **properly** passed, however fallibly, **on questions of truth and reality**.

(Quine, 1982, p. 295.) (Emphasis added.)

Epistemological naturalism⁴⁴ considers human knowledge a natural phenomenon to be studied the same way as any other aspect of nature, epistemological naturalism does not answer the philosophical sceptic; rather it says that Cartesian scepticism is psychologically and scientifically implausible. The naturalist takes for granted what the sceptic questions.

Naturalism could be characterized as the rejection of transcendental argument, that is, of non-empirical epistemological argument, naturalism recommends replacing *a priori* philosophy with scientific theory; and it claims that epistemology is just the study of science from within science.

III CRITICAL EVALUATION OF THE LOCALIST-NATURALIST THESIS

If the localist-naturalist approach is used to justify our scientific methods it is circular

Quine tells us in a quote above that science is in no need for “any justification beyond observation and the hypothetico-deductive method”, hence Quine values observation and the hypothetico-deductive method, which he considers as the correct research methods, since he also claims that science is “where judgment is properly

⁴³ Which empirical sciences are privileged as the archetypes to follow depends on the naturalist philosopher.

⁴⁴ In summary, Quine’s naturalism appears to be the result both of his holism and of the high value he gives to science and to its localist research strategy. Quine’s high valuation of science is shown, for example, in Quine’s last quote above, where he claims: “science itself, . . . , is where judgement is properly passed.”

passed... on questions of truth and reality.” The question now arises of how does Quine know that this is the *proper* method to judge “on questions of truth and reality”.

On the other hand, since Quine claims that natural science “is not answerable to any supra-scientific tribunal” (since there is no room for an “ulterior philosophy”), then for Quine any justification for what he takes to be scientific method has to come from within science. And given that the sciences in turn are selected and validated by scientific method, the justification of scientific method has to come, in Quine’s approach, ultimately from scientific method itself. Hence, we end by circularly justifying scientific method with scientific method, because there is no ‘first philosophy’, because epistemology is just an activity within natural science, because science is the *only* tribunal where questions of truth and reality are ‘properly’ settled. If it were answered, for example, that the available evidence, say the evidence provided by the history of successful science, warrants belief in scientific method, then this argument would be circular. Because we use scientific method to select what is to be taken as bona fide successful science, and to decide that the historical evidence so selected supports a belief in scientific method. We cannot validate in a non-circular way the methods of empirical science by appeal to some empirical science, Quine is aware of this fact, thus,

If the epistemologist goal is validation of the grounds of empirical science, he defeats his purpose by using psychology or other empirical science in the validation.

(Quine, 1969, pp. 75-76.)

Hence, for a Quinean, epistemology instead of seeking an alleged quixotic justification for our most basic methods and presuppositions will search to describe, to explain, to understand, via empirical science, the origin of our beliefs and the conditions under which we take them to be justified. In particular, it will seek to do this for our scientific beliefs and methods. Thus,

If we are out simply to understand the link between observation and science, we are well advised to use any available information, including that

provided by the very science whose link with observation we are seeking to understand.

(Quine, 1969, p. 76.)(Emphasis added.)

Epistemology thus becomes part of natural science, in the sense that the only legitimate epistemological questions are questions answerable or resolved by scientists using the methods of the empirical sciences, and any other epistemological questions are seen as traditional idle philosophical queries.

In this manner, Quine is trapped in a web of belief since he tries to "improve, clarify, and understand the system from within," he is a prisoner of one of many possible Neurath boats. He takes for granted, as the localist that he is, his scheme's background knowledge (in particular, his scheme's methodological assumptions), as well as the assumption of no large-scale errors or gaps in his scheme. Furthermore, when he claims that science is where, "judgment is properly passed," Quine is making an unjustified normative claim.

It may be retorted that our demands of justification for what Quine takes to be scientific method means that we doubt this method, and that these doubts must be insincere, because to doubt the hypothetico-deductive method is impossible psychologically,⁴⁵ or as Peirce would have said, because we cannot doubt it "in our hearts." The answer is that the psychological impossibility of these doubts is irrelevant, because the important question is whether these doubts are logically cogent. This was the point made by Hume about our almost irresistible inductive psychological propensities, propensities that nevertheless lack logical justification, so Peirce's advice is misdirected, since it doesn't distinguish the psychological context from the logical one. Moreover, Peirce's recommendation is itself unjustified, if not, why should one rest contented with only local or particular questions?

The pragmatist might answer: 'because local problems are solvable, while global, ultimate ones are insolvable', the pragmatist's injunction would then be: if you want

⁴⁵ Though, 'creation scientists' who propose an alternative scientific methodology exemplify that these doubts are not psychologically impossible.

to be means/ends rational, then deal only with solvable problems.⁴⁶ The pragmatist will insist that to ask for justification all the way down to the 'bedrock' is unreasonable, that it is unreasonable because methodological bedrock non-viciously circular justifications cannot be provided. In other words, the pragmatist recommends: don't ask what cannot be provided, stick to fruitful local questions, such as those of science.

Our sceptic will then go on to clarify that he is fully aware that our ultimate global questions are unsolvable, and that his aim is not to solve these questions, but only to point out that any answer to our ultimate methodological or axiological queries will require of some dogmas. Our sceptic wants to stress that even our criterion of rational identity itself has to be accepted dogmatically. He wants us to honestly recognize this logical fact, without masking it (say) with alleged virtuous rule circularity, or by surreptitiously substituting the search for justification with that of explanation.

For our sceptic, the naturalist's advice is self-complacent and it allows us to ignore uncomfortable questions, it allows us to forget that we require of some dogmatic ultimate presuppositions.

Then again, the sceptical doubts in the previous chapter, were backed by simple logical arguments, so at least in this way they were not gratuitous. Moreover, those doubts were motivated by the fact that there are irrationalists (for example, scientific creationists, who question some of the sciences and their methods), because of these irrationalists, the need then arises for justifying our scientific methods. Furthermore, those doubts were also motivated by the need to understand the nature of rationality and its logical limits, so as to better understand ourselves.

Two points now demand further analysis:

⁴⁶ Instrumental or means/ends rationality allegedly advises that, *if one wants to be rational, and if one wants goal A, then one should look for the justified optimal means, amongst those available to us, to attain or continuously approximate the desirable and sought goal A.* Therefore, if A is an impossible and not continuously approximable goal, then there won't be any means available to attain or get close to A, and then A would be means/ends irrational. This point will be further discussed in chapter V.

- 1) Does a localist-naturalist meta-methodology intend only to describe and explain how scientists proceed when revising their scientific theories and methods?
- 2) Does a naturalist theory of scientific method recommend that we follow a piecemeal procedure?

Does a localist-naturalist meta-methodology intend only to describe how scientists proceed when revising their scientific theories and methods?

If a naturalist scientific meta-methodology were merely to describe the methods and goals of some of the sciences, or if it were just to describe how scientists proceed during their scientific investigations, then this would not be enough to answer our traditional epistemological questions, since we also want to know if scientists *ought to* follow any of their described research strategies.

The normative aspect of methodology is illustrated by the fact that in the past methodologists have criticized some aspects even of the leading scientific theories of their time, they criticized them because these past dominant scientific theories failed according to these methodologists' canons. For example, Einstein *qua* methodologist thought that scientific theories should be deterministic even though quantum mechanics (the dominant theory in its field) is – at least *prima facie* - not deterministic.

Furthermore, if we were to try to get the 'ought' of normative epistemology from the 'is' of psychology (or some other empirical science) we could be suspected of committing the 'naturalistic fallacy' (a fallacy famously discussed for example by G. E. Moore.) Thus, a purely descriptive approach is not possible, because description is itself a cognitive activity with a normative dimension. For example, one selects for description those theories with characteristics that one considers as desirable in a scientific theory.⁴⁷ The theories chosen as archetypical scientific are deemed worthy of description, and from this description, the naturalist hopes to infer methodological standards. In sum, the naturalist requires – if rational - some methodological standards to select his substratum of putative scientific theories, and then from a

descriptive study of this substratum he infers methodological standards, the whole process is circular, and so the naturalist ends getting only those norms that he started with. In other words,

In a naturalistic epistemology, theories are selected as scientific if they vindicate entrenched methodological assumptions; and we decide which methods to accept in accordance with a descriptive study of those selected theories⁴⁸.

If not, why not describe the work of creation scientists and from an analysis of this description infer scientific method? Since this last option will be very likely considered unpalatable the naturalist would have to justify his selection of putative scientific theories, that is, he would have to justify the methodological canons that led him to his theoretical selection. Hence, Quine should tell us why what he considers as archetypes of the sciences are genuine sciences. Quine, however, claims that,

Naturalization of epistemology does not jettison the normative and settle for the indiscriminate description of ongoing procedures. **For me normative epistemology is a branch of engineering.** It is the technology of truth-seeking, or, in a more cautiously epistemological term, prediction. **Like any technology, it makes free use of whatever scientific findings may suit its purpose.** It draws upon mathematics in computing standard deviation and probable error ... It draws upon experimental psychology in exposing perceptual illusions, and upon cognitive psychology in scouting wishful thinking ... **There is no question here of ultimate value, as in morals; it is a matter of efficacy for an ulterior end, truth or prediction.**

(Quine, 1986, *Reply to M. White*, p. 665.)(Emphasis added.)

⁴⁷ These desirable theoretical characteristics could be known only tacitly.

⁴⁸ This is a modification of the following quote,

[In a naturalistic epistemology] methods are accepted if they vindicate entrenched theoretical assumptions; and we decide which theories to accept in accordance with accepted methodological standards.

(Hookway, 1990, p. 223.)

Again, how does Quine know that what he takes to be “scientific findings” are *bona fide* scientific results? How does he know that “truth or prediction” are valuable cognitive ends? Quine believes he knows this because he surely has applied, even if only tacitly, some methodological standards to decide this matter, and then the normative aspect of methodology creeps in when deciding which results to call scientific. Tacit norms also creep in when Quine decides that “truth or prediction” are valuable cognitive ends.⁴⁹

Quine will probably argue that a descriptive study of the empirical sciences will show that these are their aims, but again since the sciences don’t select themselves, how were the sciences selected? If the sciences were selected using some methods efficacious for the attainment of some cognitive ends, then the ends of the sciences were already there, in the methods and ends that helped to select them⁵⁰, thus we end discovering and describing the very same methods and ends that we prejudged are the methods and ends of science.

In sum, Quine's naturalism cannot be wholly descriptive, because a full-blooded descriptivist naturalism would be incapable of getting started, since all description requires some methodological standards, or norms, to recognize what is relevant and valuable of description. Or else, a descriptive naturalism requires some super-naturalistic cognitive methods and goals, it requires a vantage point outside science, it needs a moderate first philosophy.

This becomes especially clear, once one realises that even if the successful (say, in pragmatic terms, i.e., the empirically adequate) scientific theories were to somehow select themselves, a couple of questions would remain:

- i) That of whether the methods presupposed by these pragmatically successful scientific theories are the *proper* scientific methods, and
- ii) That of whether these pragmatically successful scientific theories constitute knowledge.

⁴⁹ By the way, these are two very different cognitive aims, which of the two is the genuine scientific end? That it is important to decide whether the goal of science is truth or prediction, is shown by the on going debates between realists and instrumentalists, for example, in Chapter V, we will see that Laudan claims that truth is an irrational cognitive goal.

⁵⁰ I will develop this point in Chapter V, when discussing Laudan’s theory of scientific aims.

Quine assumes an affirmative answer to these last questions, but by doing so, he is taking for granted, in spite of himself, *a priori* philosophy: pragmatism.⁵¹

Does a naturalist theory of scientific method recommend that we follow a piecemeal procedure?

If naturalism is a normative injunction in favour of a piecemeal procedure, that is, if it is an injunction for dealing with problems only when they arise, without questioning entrenched theoretical and methodological assumptions; if naturalism recommends a tinkering localism when dealing with philosophical and epistemological questions (because allegedly this is how science proceeds), then how does naturalism justify his prescription for localism?

The localist-naturalist might answer that such demands for justification are precisely the kind of questions that localism excludes, if so, localism appears as a self-serving prescription. For the sceptic, naturalism is an *ad hoc* strategy that avoids what it cannot answer, thus, the naturalist asks us to,

... rest content with a policy of piecemeal tinkering whose legitimacy as a way of approaching truth cannot be established. If this is all that can be said,

⁵¹ Thus, Quine claims that,

We cannot detach ourselves from [our conceptual scheme] and compare it objectively with an unconceptualized reality. Hence it is meaningless, I suggest, to inquire into the absolute correctness of a conceptual scheme as a mirror of reality. Our standard for appraising basic changes of conceptual scheme **must be**, not a realistic standard of correspondence to reality, but a **pragmatic standard**. Concepts are language, and the purpose of concepts and of language is **efficacy in communication and in prediction**. Such is the ultimate duty of language, science and philosophy, **and it is in relation to that duty that a conceptual scheme has finally to be appraised**.
(Quine, 1953, p. 79.) (Emphasis added.)

Hence, even though Quine maintains that we cannot say which conceptual scheme is objectively correct or true (and in this sense none are better), he claims that we can still compare conceptual schemes in terms of their shared aim for efficacy in prediction. In the case of scientific conceptual schemes, this comparison can also be made because Quine believes conceptual schemes also share some basic methodological strategies, such as the hypothetico-deductive method. In other words, Quine believes that all scientific conceptual schemes share some basic methodological and axiological presuppositions, but how does Quine justify this belief of his?

naturalistic epistemology appears to **acquiesce in skepticism rather than try to overcome it.**

(Hookway, 1990, p. 223.)(Emphasis added.)

Again, why should we accept the naturalist's advice: only local questions! Consider that traditionally, it has been considered philosophically legitimate to ask whether scientific procedures as a whole are justified. The localist will likely retort as follows,

Yes, inquiry can be a risky and fragile process, we must to certain extent trust to luck. However, do we have some positive reasons for mistrust in our process of inquiry? Or else, is this justification required for the growth of knowledge? If it is not, should we care to have this justification?

The answer is that this justification is required if we want to know whether our process of knowledge acquisition is reliable. Moreover, the arguments of the last chapter could be seen as providing some reason for mistrusting our process of inquiry, since they showed logical deficiencies –for example, inconsistencies or self referential paradoxes- in our theories of rationality, and also because they showed that our most basic cognitive methods, deduction and induction appear to be justifiable only *via* viciously circular arguments. The naturalist could still retort that,

... given the success of science so far it seems **more prudent** to doubt our theories of rationality, or the criticisms made to them, than to distrust our science.

And the naturalist would recommend that,

... science is innocent unless proved guilty while our metascience is guilty unless proved innocent.

(Carnap, as paraphrased by Hookway, 1988, p. 198.)

But, why is the naturalist strategy more prudent? Is this evaluative judgement also going to be taken for granted? The naturalist will likely insist that,

The ship *keeps navigating*, how it does it, we still don't fully understand, but this is no reason to undermine our hope in its going on and in eventually understanding it better. It is true that this hope lacks positive reasons to back it (except for a history of some **successes**), but at least it also lacks negative arguments against it, except for the absence of a non-viciously circular justification.

However, how do we know that the ship will keep on navigating? It could well sink at any moment, on the other hand, how does the naturalist know that what he considers a history of some scientific "successes" is really that, i.e., a history of *objective* scientific successes, and not a history of something else. In other words, how does the localist know that our world is a 'normal' one? All we know is that so far our world seems to have been normal, from this to conclude that it has in fact been normal is taking for granted a prejudice. But even if our world has in fact been normal, will it keep being normal? The naturalist's belief in the normality of our world – and in the persistence of this putative normality - may be natural or spontaneous, but so are the sceptic's doubts, this as shown by the fact that these traditional sceptical questions keep on recurring.

For the Quinean our most basic cognitive methods are in no need of justification, what require justification are, instead, the 'unnatural' doubts of the sceptic: to doubt what has served us so well for so long requires a justification. The Quinean holds the following conditional principle P:

P: If it works, don't justify it, because it doesn't need a justification.

However, how do we go in P from the antecedent to the conclusion? How is this principle going to be justified? Either this principle is an *a priori* prescription, or it can be justified empirically. Now, to justify it empirically we would require of the

very same methods (such as the hypothetic-deductive method) that this principle claims don't need justification. The principle is then in the end saying that it itself doesn't need of an empirical justification, then P has the character of a stipulation, of an *a priori* prescription, a character which goes against the Quinean dislike for 'first philosophy'. Moreover, the Quinean appears to advise: Forbidden to ask questions which we cannot answer! Forbidden to question, what we consider obvious! But,

... believing something to be obvious does not obviate the need to defend it, or at least the need to acknowledge that belief as an assumption ... that one makes.

(Worrall, 1999, p. 348.)

The need to justify the obvious becomes especially pertinent when one considers that according to an evolutionary perspective it could be biologically advantageous (energy and time wise) to find obvious what is strictly wrong, but close enough (survival wise) to the truth.

Thus, consider that biological evolution selected our cognitive system for optimal efficiency *vis a vis* promoting biological survival and reproduction in a prehistoric terrestrial environment of middle-sized objects, and that as our investigations take us into the micro and macro cosmos, farther and farther away from our original problem situation, our cognitive architecture could prove insufficient. In other words, it is doubtful that the cognitive capacities that proved adequate to hunt a mammoth will also be sufficient to explore Mars, to do philosophy, and to develop a unified field theory in physics. In this way,

A naturalized epistemology begins by setting aside the classical justificatory questions of the adequacy of our knowledge-gathering practices, but ends up providing the basis for a new suspicion that there are deep limits for our knowledge in all but the most implausibly homogeneous and manageable of possible worlds. Indeed, it would be an odd accident if our subjective canons of scientific acceptability turned out to match in all respects the objective character of the universe. Why should our cognitive capacities be adequate for all domains? ... We are... unlikely to

have entirely correct and complete theories; **our innate cognitive biases may cause us to accept some falsehoods and reject some truths.**

... There seem to be possible worlds that would be too complicated for us or a society of experts to represent feasibly... The breadth and depth of putatively possible knowledge may be intrinsically too great for a both manageable and complete world view...

(Cherniak, pp. 127-9.)(Emphasis added.)

This conclusion becomes more plausible once one recalls that evolution selected those of our ancestors with cognitive capacities correct enough to promote their biological survival and reproduction, and that evolution did not necessarily select reliable truth producing and truth transmitting cognitive capacities even for dealing with the middle sized objects of our ancestor's primeval savannah⁵². Therefore, our contemporary biology undermines two of naturalism's key assumptions:

- i) That piecemeal improvement can proceed indefinitely.
- ii) That there are no large-scale errors in our conceptual scheme.

The naturalist criticized the sceptic for entertaining unjustified or idle doubts, and ironically we now discover that science itself provides justified sceptical doubts, doubts analogous to those of the old sceptic.⁵³ Now, to reject these last doubts someone could speculate on,

... a particular type of cosmology, one that ensures a preestablished harmony of man with the universe. It would be a peculiar coincidence in need of much explanation if, for every domain, every one of the interesting true theories, and all of them together, should just happen to be simple enough to be usable by, and intelligible to, us.

(Cherniak, p. 129.)

⁵² This because, natural selection, being interested only in survival, had to cut corners to save energy and time.

And he could go on to make an assumption of *veracitas Dei* (as Descartes and Thomas Reid did) to underwrite his belief in a pre-established harmony of man's mind and the cosmos, to back his hope that our cognitive means are adequate for our cognitive ends. These conjectures, however, will likely be unsavoury to the naturalist, because of their speculative metaphysical character.

IV IS JUSTIFICATION ONLY ARGUMENTATIVE?

The localist-naturalist can still argue that behind the sceptic's doubts examined so far lurks the assumption that justification is only argumentative i.e., the belief that a proposition is justified by inferring it -say, deductively or inductively - from some premisses, and only thus. Therefore, it follows that if there are logical limits to argumentation, then there will be also logical limits to justification. Our sceptic has confined justification only to inferential relations amongst propositions and our sceptic has required that the justified believer have a conscious reason⁵⁴ for thinking that his belief is true.

The naturalist, on the other hand, *also* welcomes 'externalist' non-argumentative justifications, such as those provided by, say, some psychological unconscious processes. It is claimed, for example, that beliefs caused or generated by overall reliable truth generating psychological processes (or beliefs transmitted, from previously justified beliefs, by generally reliable belief transmitting processes), in an environment normal for the formation or transmission of such beliefs, are justified. For this doctrine, *reliabilism*, beliefs would be justified even if the subject were unaware of the belief generating and transmitting processes or faculties going on in his mind, and because of this unawareness of the *justificans* the believer will in general have no reason for thinking that his beliefs are true or likely to be true, but will nonetheless be justified in accepting his beliefs. Examples of possible reliable 'source' processes are perception, memory, reasoning and intuition, while examples

⁵³ The consequences of some sceptical arguments provided by evolution theory will be further explored in chapter III.

⁵⁴ Or at least, the justified believer should have his beliefs justified by reasons that can be made conscious -after adequate self examination or reflection- that is, the justifying reasons should be capable of becoming conscious.

of possible reliable ‘transmitting’ or inferential processes are deduction and induction.

Reliabilism deals successfully with a scepticism concerning observational statements, since for reliabilism observational or basic statements can be justified if they are generated by some reliable non-inferential psychological processes, such as the processes of perception of a healthy subject in a standard situation. While in the argumentative conception of justification, only other statements can justify basic statements, a requirement which leads us into the familiar sceptical quandary: an infinite regress of justificatory statements, and to stop the regress of statements, circularity or dogmatism. Popper, for example, deals with this trilemma by concluding that a form of conventional dogmatism is unavoidable, that is, some basic statements have to be taken as true *pro tem* by a convention made by a scientific community. A conventional agreement, though, that could be revised and substituted by another conventional agreement⁵⁵, if serious criticism of the first conventional basic statement were to arise. Still, the basic statements at which we stop the regress have the character of dogmas in the sense that they are accepted as true –again, even if only temporarily- without an argumentative justification. Popper arrives at this doctrine of his, because he believes that statements can only be justified by other statements, and therefore he believes that psychological processes even if reliable can at most cause or motivate our decision to accept some basic statements, he would say that the reliabilist confuses justification with causation or motivation⁵⁶.

Every test of a theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other which we *decide to accept*.

... The basic statements at which we stop, which we decide to accept as satisfactory, and as sufficiently tested, have admittedly the character of

⁵⁵ Assuming the scientific community can reach an agreement about which basic statements are not problematic for the time being.

⁵⁶ Popper’s position can perhaps be clarified *via* an ethical analogy, thus imagine a criminal who would causally explain his crime by showing that his action was the result of an emotional process (say, love or benevolence) that in general, and in standard situations leads, to good actions. Would we say that this causal explanation justifies as good his crime? Many of us would find an affirmative answer as counterintuitive. On the other hand, the adage says: *to know all, is to forgive all*, forgive perhaps, but not justify as good or right.

dogmas, but only in so far as we may desist from justifying them by further arguments (or by further tests).

... Experiences can *motivate a decision*, perhaps decisively, and hence an acceptance or a rejection of a statement, but a basic statement cannot be *justified* by them –no more than thumping the table.

(Popper, sections 27-9.)

Hence, it seems that reliabilism can avoid an scepticism of basic statements, while the traditional argumentative or internalist doctrine of justification cannot avoid scepticism.

The reliabilist, however, has to deal with sceptical difficulties of his own once the sceptic asks for a justification of the beliefs in the reliability of the so-called reliable processes. Thus, the justification for the beliefs about the reliability of some processes will be provided by some other belief generating ‘reliable’ processes, and to stop a regress the reliabilist, like the argumentative internalist before him, will end with circularity or dogmatism concerning the reliability of some process. The reliabilist will stop the regress by arguing that our cognitive processes, such as our inductive processes, are reliably self-supporting,⁵⁷ or by arguing that various of our cognitive processes are supported by some more basic self-supporting cognitive processes, or by arguing that our cognitive processes mutually (i.e., circularly) support each other,

An important component of a reliabilist theory of knowledge would surely be a list of reliable faculties: perception, memory, introspection, inference, and perhaps others. But how could one justify the addition of a faculty to the list except by use –direct or indirect- of that very faculty? **And is that not as viciously circular as declaring a source reliable by accepting its reports at face value and inferring that it issues truth? Such reasoning is unreliable and in any case unacceptable.** We may perhaps avoid vicious circularity by allowing a faculty to gain support from the use of other faculties. But these would need support of their own and how could they gain it except by each leaning on the others? Reliabilism is thus driven

to seek refuge in a wide enough circle, which it must regard as benign, perhaps in virtue of its wide diameter.

(Sosa, E., p. 95.)(Emphasis added.)

However, both a viciously circular argument with a wide diameter and one with a small diameter are equally logically unacceptable, if there is any difference between these two circles it would be just a matter of psychological obviousness. The wide diameter circle may be regarded as “benign” (i.e., as a bona fide probative argument) only because its circularity remains hidden, only because its fault is not apparent, but if so, this looks as a deceptive or hypocritical strategy, it looks like a simulation game.

For example, assume that one has belief *B* that our memory has been in general a reliable belief producing cognitive process. Now, if someone asks for a justification of *B*, we could justify it by saying that belief *B* is generated by our memory cognitive processes. That is, we would justify *B* by invoking our memory - i.e., circularly - and if in addition, we were to infer that our memory cognitive processes will probably continue being reliable, we would have to assume also that our inductive cognitive processes are reliable.

Moreover, the reliabilist assumes that a belief *B* is justified in case cognitive processes that are in general reliable produce *B* (or transmit *B* from other justified beliefs.) Now, if the reliabilist is in turn going to justify his theory of justification he will argue either:

i) That the reliabilist theory of epistemic justification is justified because possible overall reliable cognitive processes, such as reasoning plus imagination, generate the reliabilist’s theory of justification. It is, however, problematic to argue that reasoning and imagination –once taken beyond our strongest intellectual intuitions- are by themselves in general reliable belief generating cognitive processes, given that it is almost a truism that reasoning and imagination have often lead us into absurd theories or beliefs.

Or,

⁵⁷ Cf., chapter I above, for Papineau’s reliabilist rule circular justification of induction.

ii) The reliabilist will end up with an argumentative internalist justification of his theory of justification, an argumentative justification that will ultimately lead again into the sceptical trilemma, of infinite regress, circularity, or dogmatism. Hence, in the end, the reliabilist finds himself in the same sceptical muddle from which he tried to extricate himself.

RECAPITULATION

Epistemological naturalism criticizes the sceptic for entertaining unjustified global doubts and naturalism tries instead to avoid scepticism by taking for granted as non problematic our background scientific knowledge and by recommending only a localist or piecemeal correction of our corpus of knowledge, these corrections will be motivated by limited and justified questions or problems.

It was argued that epistemological naturalism is the result of a holist thesis plus a high valuation of empirical science and it was argued that the epistemological naturalist:

- i) Cannot justify without vicious circularity the most basic methods of science nor epistemological naturalism's localist recommendation.
- ii) That if epistemological naturalism intends to be a description of genuine scientific methods then naturalism tacitly takes for granted (i.e., without circular justification) some epistemic norms.
- iii) That natural science itself (i.e., evolutionary biology) produces traditional sceptic doubts, and that therefore epistemological naturalism cannot avoid scepticism.
- iv) That naturalism can neither avoid sceptic doubts by substituting an argumentative theory of justification with a reliabilist theory.

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CHAPTER THREE

CAN OUR COGNITIVE FACULTIES BE GIVEN A BIOLOGICAL LEGITIMISATION?

Darwin's theory has no more to do with philosophy than any other hypothesis in natural science.

L. Wittgenstein

What is wrong with evolutionary epistemology is not that the scientific facts are wrong, but that they don't answer any of the philosophical questions.

H. Putnam

INTRODUCTION

We concluded in a previous chapter that the search for an argumentative justification of our most basic cognitive methods ultimately led us into Sextus' trilemma: infinite regress, circularity or dogmatism, in spite of this, some naturalist philosophers have claimed that,

... the methods of science are rooted in selective necessity,... **The nature and development of science is constrained and informed by the biologically channelled modes of thinking imposed on us by evolution** – a consequence of the reproductive struggles faced by humans today, and even more a consequence of those struggles faced by humans in the past.

... In other words, in order to understand why science is as it is – why laws, why predictions, why falsifiability, why consiliences - we need to look at the principles of scientific reasoning or methodology ... what I argue is that these principles have their being and **only justification in their Darwinian**

value, that is in their adaptive worth to us humans –or, at least, to our proto-human ancestors. In short, I argue that the principles which guide and mould science are rooted in our biology,...

(Ruse, 1986, pp. 149, 155.)(Emphasis added.)

I will examine in this chapter whether it is possible to justify as reliable⁵⁸ our cognitive capacities from an evolutionary perspective, I will argue that an evolutionary justification is not possible without vicious circularity, and I will argue that what evolution theory can instead hope to do is to explain the origin of our intellectual faculties. Whether evolution can provide a genetic explanation of the reliability of our cognitive capacities, however, is also debatable, since there are reasons to doubt the rationality⁵⁹ of a belief in an evolutionary explanation of our intellectual powers.

We have several cognitive faculties or intellectual capacities: inferential capacities such as inductive and deductive capacities, and ‘source’ capacities such as memory, perception and intuition⁶⁰. We consider these capacities of ours, when functioning

⁵⁸ A cognitive faculty is reliable if the great majority of its deliverances are true when it functions properly and in a standard situation.

⁵⁹ As will be discussed further below, a belief in an evolutionary explanation of the reliability of our cognitive capacities is suspect of being a-rational, if not irrational.

⁶⁰ Intuitive judgements are pre-analytic judgements but not arbitrary ones, and intuitive judgements can be improved by training and by gaining in expertise. The following examples will help illustrate the important role played by our cognitive intuitions.

a) A first example is provided by Nelson Goodman's projectable and non-projectable predicates. Goodman showed that it was not possible to rule out predicates like ‘grue’ on purely formal grounds. And this then left common sense, and finally intuition, as the only possible way for scientists to distinguish between projectable predicates and non-projectable ones.

b) Duhem also argued for scientists' intuitions or 'good sense' as a necessary addition to deductive logic. Good sense would provide, for instance, a way to decide what to preserve and what to reject when a lattice of hypotheses is empirically refuted as a whole.

These motives which do not proceed from logic and yet direct our choices, these 'reasons which reason does not know' [...] constitute what is appropriately called good sense.

(Duhem, p. 217.)

c) Furthermore, intuitive judgements are also found in the subjective Bayesian approach, which requires inputs of prior probabilities, these priors, however, can be unreasonable and lead to bizarre results:

properly⁶¹ and in an adequate environment, as reliable either as transmitters, or as expanders and as sources of truth. The problem now arises of how to justify this belief in the reliability of our cognitive faculties. The quest for the justification of the methods, or criteria, of science is not an idle one as shown by the fact that the methods of science are nowadays challenged by alternative cognitive standards, for example, those of creationism. Moreover, creationism's alternative standards deny much of the framework of accepted scientific knowledge, hence it is important to try to justify as correct the cognitive standards of science.

I will examine in what follows the influential answer to this problem provided by 'evolutionary epistemology', a constellation of doctrines under the name of evolutionary epistemology, however, appears in the literature. This constellation can be analysed as consisting of two related but different families (cf., M. Bradie.) One family looks for analogies between the evolution of life and that of the history of ideas, in particular, that of the history of our changing science. The doctrines in this first subset are mainly interested in understanding the growth of knowledge, on an individual or on a species basis. And for this they invoke analogies to natural selection. The other subset (EE for short), the one to be discussed here, intends to understand, explain and *justify* our basic mental cognitive abilities and innate

... differences in the prior probability function can lead to violent differences in the actual degrees of support assigned to theories, ..., these differences can amount to what would ordinarily be considered as gross irrationalities (...) The extent to which this is true is in fact rather shocking. Arthur Burks has in fact shown that there are even 'counter inductive prior probability functions'. That is, there is a certain logically possible prior probability metric such that if a scientist had that metric then as more evidence came in for a hypothesis (using the term more evidence on the basis of our normal inductive judgments) then the scientist would assign lower and lower weight to the hypothesis for a very long time.
(Putnam, 1981, p. 192.)

The need then arises to distinguish between reasonable and unreasonable priors and for this, the scientists' intuitive judgements have to be welcomed. This because,

There does not seem to be any good reason to think that there would be a set of rules which could distinguish between reasonable and unreasonable priors and which would be any simpler than a complete description of the total psychology of an ideally rational human being.
(Putnam, 1981, p. 192.)

methodological propensities as *only*⁶² the result of evolutionary mechanisms as *nowadays* understood. The focus is now on the understanding of the development of these innate methods of thesis substantiation amongst the organisms in a lineage rather than on that of the evolution of the factual theses themselves.

EE claims that biological evolution has predisposed us to think in a way that is constrained by a certain fixed adaptive mental architecture, or at any rate, the claim is that if this mental architecture is not entirely fixed then it remains changeless for periods of historical magnitude. It is claimed that our most basic cognitive methods are reflections of innate dispositions and the result of natural selection working on the products of some chance events, such as random mutations. Natural selection discards most of these mutations, because they prove deleterious to the organism where they appear, but some are kept because they have survival value, some are kept because they enhance fitness, and as result, these selected mutations spread through the population.

According to this evolutionary view the emergence of man -including her intellect- is the result of the natural selection of the biologically best adapted or fittest over a span of many generations. These fittest have characteristics or adaptations that enable them to survive and to reproduce better than the unfit do. EE claims that reality shapes reason via evolution and that this in turn explains the reliability of our reason when dealing with the real.

Reason tells us about reality because reality shapes reason, selecting for what seems “evident”.

(Robert Nozick, p. 112.)

In other words, the shaping of our reason by reality could help explain the success of our science and the apparent, or actual, partial harmony of our mind and the world. This evolutionary view is a distant echo of an illustrious earlier view: Plato's theory of anamnesis, where the modern cognitive intuition would correspond to the ancient recollection.

⁶¹ How to characterise ‘proper function’ is a difficult problem, cf., for example, Plantinga’s chapter 11.

⁶² That is, without invoking any other possible natural or supernatural causal agents.

The claim is that there is a constant, genetically transmitted, underlying uniformity to human thought, it is maintained that our science is constrained and channelled by our biology.

Otherwise, the thesis of evolutionary epistemology (EE) is that we have innate capacities and dispositions to understand, discover or learn, let's say, Peano's axioms and some principles of logic (such as the laws of identity, and non-contradiction.) It is also claimed that we have an innate methodological propensity to make right inductions (for example, that we have a predilection for projectable predicates) and that we have an inborn fondness for theoretical simplicity and for consilience (that is, for hypotheses with greater explanatory power.)

EE holds that we have these capacities and dispositions *only* because they proved biologically advantageous to our ancestors, otherwise, EE holds that these capacities and dispositions had a clear survival value to our ancestors in the struggle for existence and therefore were biologically selected and passed on to us. Now, this selection could have been specific or an unintended by-product of other selected traits, for example, consider the case of pleiotropic genes,

...in which one gene affects two or more distinct traits or systems. It will sometimes be the case that a gene has positive effects in one system and negative effects on another... The genes of albinism in arctic animals provide a nice example. The white coats these genes produce are obviously adaptive. However, the same genes typically produce serious eye problems, and albino animals generally can't see as well as their coloured conspecifics.

(Stich, p. 65.)

One could then imagine our intellectual capacities to have been linked to some selected positive trait *via* (a) pleiotropic gene (s) and we could imagine this gene having been selected by virtue of its link to this positive trait. The biologically negative or indifferent intellectual attributes then could have become perpetuated by virtue of their link with the positive trait via the pleiotropic gene. Hence, the most we can really conclude is that our cognitive instincts are not biologically grossly

maladaptive to undermine the adaptive value of any positive traits they may be tied to. Ruse, however, comments,

I would still be uncomfortable about supposing that so fundamental an aspect of human nature as our reasoning abilities was entirely a non-adaptive by-product of the evolutionary process. This is simply not the way that evolution works. When you have major features which seem to have adaptive virtues -and if reasoning does not have such virtues, I do not know what would- then you expect to find natural selection has been at work.

(Ruse, 1995, p. 173.)

Whatever might be the case, primitive hominids, which by some accident ended with some cognitive capacities and intellectual preferences –or so the evolutionary story goes- were more successful at reproduction. Hence these hominids eventually predominated, or at least were to be well represented, and became our likely ancestors, and consequently, these faculties and preferences are now innate in our descendants and us. These putative faculties now manifest themselves as half conscious intellectual abilities, intuitions, aversions and drives: as cognitive instincts. Notice that it is not being claimed that our mathematics, logic, philosophy or our empirical science were biologically selected. This biologisation is maintained only for a small methodological foundation, and our knowledge claims would be an epiphenomenon resting on this small foundation. Notice also that all that we are told about these innate capacities is that they lie behind scientific methodology, and that they lie behind some intellectual human drives and aversions, and notice that all the evidence about their existence comes from some behavioural regularities of ours, methodological or otherwise.

If correct, this evolutionary epistemology may help explain the ontological ‘tunnel vision’ claimed by Quine as a characteristic of human science,

... a physical theory of radically different form from ours, with nothing even recognizably similar to our **quantification or objective reference, might still be empirically equivalent to ours, in the sense of predicting the same**

episodes of sensory bombardment on the strength of the same past episodes ... our science has developed in such a way as to maintain always a manageably narrow spectrum of visible alternatives among which to choose when need arises to revise a theory. **It is this narrowing of sights, or tunnel vision that has made for the continuity of science, through the vicissitudes of refutation and correction.** And it is this also that has fostered the **illusion of there being only one solution to the riddle of the universe.**

(Quine, 1975, p. 81) (Emphasis added.)

Contra Quine, there could well be only one solution to the riddle of the universe, otherwise, how does Quine know that this alleged “tunnel vision” is an illusion? On the other hand, the existence of a tunnel vision is debatable given what we know of theoretical change (for example, in the conceptual and ontological axes) during scientific revolutions and, some would add, given also what we know of scientific methodological and axiological change during the development of science⁶³.

Otherwise, since it is uncontroversial that change in scientific theories, and also to *some degree* in scientific methods and goals, has happened in the sciences, in what sense can we still talk of a tunnel vision?

Still, if we were to grant to Quine the existence of this tunnel vision, EE could then help explain this tunnel vision via EE’s claim that human culture, and science in particular, are constrained and channelled into certain fixed paths, by mental capacities and dispositions programmed in our minds by biological evolution. If so, our biologically constrained modes of thinking must be wide or flexible enough to allow for the recorded variations in scientific concepts, ontology, methodology and aims. These cognitive instincts of ours must deal only with the most general features of our scientific reasoning and not with the specifics, the minutiae, of alternative methodologies, this must be so, since there is no consensus about fine methodological points.

⁶³ For example, L. Laudan in his *Science and Values* argues that all scientific theories, methods and aims can change.

The existence of these cognitive instincts gets some empirical support from anthropological, psychological and ethological studies, thus: ⁶⁴

i) The existence of innate human abilities is suggested by the universal capability of healthy humans for language, for musicality, and for basic arithmetic conceptualisations and operations.

ii) We seem to have hereditary inclinations to think causally and analogically. So,

We show untaught appreciations of similarity and difference, as are needed in order to think analogically. Likewise in the case of causality and other modes of thinking. (...) Humans, like other primates, automatically associate heights with falling, snakes with danger, (...)

(Ruse, 1986, p. 165.)

iii) We seem to have congenital conceptualisations, thus all healthy humans unconsciously classify colours in four basic categories, blue, green, yellow, and red.

Thus,

...unconsciously, we break colours up into four basic categories,... People of radically different cultures, thinking quite unrelated languages, use much the same classificatory schema.

(Ruse, pp. 143-4.)

The conclusion is that we are not born with passive *tabula rasa* minds, and that instead we are born with some innate inclinations to certain thinking patterns and to certain patterns of behaviour.

Now, from the perspective of EE, our innate capacities have a contingent character; EE tells us that these instincts could all be strictly wrong and improved. That is, according to EE given different chance mutations, our present intellectual capacities might not have arisen, and even if these chance mutations had already arisen, these capacities might not have been selected given a different sequence of primitive habitats, because they could have been of little or no use in such habitats.

⁶⁴ Cf., Ruse (1986) pp. 141-68.

On the other hand, our most basic logical intuitions provide for us the conditions for thinking rationally, and the rational denial of these deep-seated logical intuitions of ours is very difficult to conceive, if at all⁶⁵, then impasse. Either EE is wrong or we are the victims of confusion, a confusion to which we are inevitably urged by our prehistoric biological programming.

Taking an evolutionary perspective, we must acknowledge the contingency and explicability of our present ways of thinking, and in particular our present ascriptions of necessity. But if we do regard some things as necessarily true we thereby deny their contingency and cannot countenance the possibility of alternatives to them. We must simultaneously appreciate the contingency of the fact that the limits of our thought lie just where they do while remaining unable to think beyond those limits. It is not easy to hold consistently to both points of view simultaneously, and we inevitably find ourselves moving back and forth somewhat unsurely between them. That is perhaps inevitable when we try to stand outside the evolutionary process and see it as a whole, *sub specie aeternitatis*, while the terms we use to try to understand that process and our place in it are themselves products of the very process we are trying vainly to transcend.

(B. Stroud, 1981, pp. 247.)

Is it possible to understand human beings as part of nature and still maintain that we have cognitive access to *a priori correct* norms? That is, do we have the putative ability to know some truths or some methodological norms based on reflection alone? EE claims that what we consider *a priori* norms or truths is only so from a

⁶⁵ One might speculate, for example, that there could exist alternative evolved intelligent beings somewhere else with bizarre ultimate methodological rules and aims. But, could we argue, and even communicate, with beings that would reason with a collection of basic cognitive intuitions disjoint from ours? While a partially differently wired rational mind may be conceivable (a mind, for instance, that would not reason numerically), one that would deny the necessary truths that we hold, one that would have ultimate cognitive principles contradictory to ours, would be considered by us to be confused or insane, or if not, as ineffable and unintelligible. A minimal set of cognitive intellectual strategies and intuitions shared with ours is going to be a necessary characteristic to ask of any understandable rational agent, of any agent with whom we could debate. Our most basic cognitive instincts, therefore de facto function as if they were universally binding, as if they were the objective right ones, which, on the other hand, they could well be.

synchronous psychological perspective, EE claims that *a priori* knowledge is the result of a contingent ancestral evolutionary implant; on the other hand, *a priori* truths appear to us to have a necessary character, a necessity which they could objectively have.

Now, if it were granted that our cognitive capacities have a biological origin, would this evolutionary genetic explanation of the programming of our brains or minds *justify* as correct the content of the program itself?

I

In what follows, I will argue for a negative answer to the last question, but first, I will try to articulate what an argument from EE in favour of a belief in the reliability of our cognitive capacities, as transmitters or producers of truths, would look like.

The justificatory EE argument would go as follows:

- a) We have good reasons to hold that our cognitive system, and in particular our scientific methods, are on the whole reliable as producers (for example, of true observational statements) and as transmitters (i.e., as inferential rules) of truths.
- b) It is rational to believe that the theory of evolution is if not strictly true, at least is probably close to the truth (or if not, that it is at present, amongst all its competitor theories, the empirically most adequate theory.) The belief in the theory of evolution is rational, because this belief has been well corroborated or confirmed by the empirical evidence -as produced, selected and interpreted by our in general reliable cognitive system.
- c) The theory of evolution claims that we are the result of natural selection, of biological evolution.

EE adds to these premisses the following ones:

- d) Our cognitive system is *only* a result of the evolutionary mechanisms entertained by *contemporary* biological evolution theory ⁶⁶.
- e) Organisms selected as fittest for survival have close to optimal characteristics or systems ⁶⁷.
- f) An evolutionary optimal cognitive system would be an overall reliable cognitive system. ⁶⁸

It now follows from premisses (a) through (f) that,

C: It is rational to believe that our cognitive system is in general a reliable cognitive system.

Let's take, for the time being, all the previous premisses for granted and focus in the structure of this argument. We notice that premiss (a) is equivalent to the conclusion C, and we discover that to obtain C we require of (a), because without (a) we wouldn't obtain (b), and this last premiss in conjunction with (c), (d), (e) and (f) provides C. Hence, there is circularity and this circularity is inevitable given that in this argument there is no alternative evidential route leading to C, that is, without (a). Furthermore, the argument is viciously circular because: i) being circular it intends to be probative, i.e., it pretends to have a justificatory character, and ii) because at least one of its premisses [premiss (a)] is as problematic, as doubtful or as implausible as the conclusion C. Thus,

⁶⁶ Thomas Nagel has challenged this premiss, he claims that there is no reason to believe that "every noteworthy characteristic of human beings, or of any other organism, must have a Darwinian explanation". Nagel adds,

Why not take the development of the human intellect as a probable counterexample to the law that natural selection explains everything, instead of forcing it under the law with the improbable speculations unsupported by evidence?
(Nagel Thomas, 1986, p. 81)

But then how to explain our intellectual faculties and their possible reliability? Nagel finds it incredible that the appearance of our minds is a natural accident (cf., Nagel, 1997, p. 132), and therefore Nagel speculates that our cognitive capacities are the result of so far unknown natural laws and constraints.

⁶⁷ This premiss and the following one have been disputed by P. Stich, cf., pp. 63-70, he argues that the denials of these two premisses are wholly compatible with contemporary evolutionary theory, and not unlikely with respect to it.

⁶⁸ This last premiss has also been questioned by A. Plantinga, his argument is discussed further below.

An argument in **persuasion dialogue** can be useful to persuade or convince the respondent to whom it is directed only if the premisses are already secured as commitments of the respondent **so that for him they are more plausible than the conclusion that he doubts or questions...** Certainly this is true in persuasion dialogue because the premisses must be commitments of the respondent that are, from his point of view, more plausible than the conclusion. They must be, to be useful **to prove the conclusion to him** successfully.

(Walton, p. 321.) (Emphasis added.)

On the other hand, in a demonstrative argument – such as in those of Euclidean geometry – the premisses should not only be more plausible, but also,

... be firmly established as better known to be true than the conclusion to be proved. This context, or one very like it, appears to be indicated in the remark of Aristotle (*Prior Analytics* 64 b 30) where it is required of a demonstration that the premisses should be better known or established than the conclusion to be proved from them. In other words, Aristotle was saying that a *demonstration* is a special kind of argument where the premisses are better known than the conclusion. Therefore, in terms of evidence or knowledge, the premisses are prior to the conclusion in a demonstration. Hence it follows that the conclusion cannot be at the same time be prior to the premisses. The inquiry or demonstration is even more strongly inimical to circular reasoning, and apt to classify it as fallacious, than the persuasion dialogue.

(Walton, p. 324.)

A viciously circular argument is then pointless both as a persuasive and as a demonstrative argument. In the previous argument from EE in favour of a belief in the reliability of our cognitive capacities, these capacities, and the scientific method they make possible, are used to justify (*via* the biological theory of evolution) these same capacities and scientific method. Thus, the theory of evolution is identified as scientific, it is tested, and it is evaluated with the help of the very same methods we

would like to justify, at the end, what has been done is to show that some scientific methods are behind the very same scientific methods. The argument is viciously circular because it purports to give a reason for trusting our cognitive methods, but the argument itself is trustworthy only if those cognitive methods (at least those required by the premisses of the argument) are indeed trustworthy. Hence, we only get back what we put in. If not, how do we know which are the archetypes of reliable scientific theories? If not, why not use 'creation science' instead of Darwinism to provide us with the answer about the origin, and with the justification, of our cognitive instincts?

The EE theorist has presupposed that we know that Darwinism is a well-corroborated essentially correct *bona fide* scientific theory. But this conclusion of his, if rational, must have been inferred by applying methodological rules, the very same rules he wants to justify via Darwinism. Therefore, there is here a vicious argumentative circle: we assume as a premiss the conclusion to be proved, and hence the premiss is as doubtful, as problematic as the conclusion. This reasoning, if used to accredit our methodological principles, is fallacious, and this would be the case even if the argumentative circle were not obvious because it were very broad and inclusive.

The circularity is especially clear in the case of induction; thus, the evolutionary perspective assumes that the cognitive intuitions that worked in the past will go on working in the future. However, why so? Because, of an inductive inference. The inclination to make inductive inferences exists in our minds (EE would say) because of its past survival value, and this inductive inclination of ours in turn predisposes us to assume that induction's survival value will be conserved in the future. The inductive bias of ours, however strong, cannot justify itself, because,

That past facts led to assumptions being built into us that fit them does not mean that those facts will continue to hold and those assumptions will continue to serve us.

(Nozick, 1993, p. 123.)

We are back to a situation akin to Hume's psychological explanation of our inductive or causal reasoning, though this time the explanation is biological. We are told that we have been pre-programmed with certain thinking patterns, but from this putative biological destiny, we cannot conclude that we are rationally justified in employing these thinking patterns. That is, there is no non-circular evolutionary reply to the sceptic who would question our inductive intuitions.

Still some 'circularities' have been defended arguing that we sometimes find them in standard explanatory or causal sequences such as autocatalytic reactions (i.e., chemical reactions where a reaction product catalyses at some time t_1 its own generation at a later time t_2 .) So,

... **circles of explanation** may be perfectly acceptable. My being good at tennis explains my desire to play, which explains my hours of practice, which explains my being good at tennis.

(A. H. Goldman, p. 45.)(Emphasis added.)

That is, my being good at tennis (at t_1) explains my desire to play (at t_2), which explains my hours of practice (at t_3), which explains my being even better at tennis (at t_4), where $t_1 < t_2 < t_3 < t_4$. Now this sequence is not circular, it is rather a causal diachronic chain. Similarly, Goldman's following argument again is not a circular justificatory argument, but rather a causal explanatory chain.

Use of the principle of inference [to the best explanation] explains our belief in natural selection and its metaphysical framework, which explains how that cognitive mechanism evolved for its capacity to preserve truth, which **explains** both why we use the principle and why we **ought** to use it.

(A. H. Goldman, p. 45.)(Emphasis added.)

Again, use of the principle of inference to the best explanation (at t_1) explains our belief in natural selection (at t_1), which explains how the principle of inference to the best explanation evolved (at t_0 .) And this in turn explains why we use the principle (at t_1), where $t_0 < t_1$. Goldman's last argument may explain why we use the principle

of inference in question but it does not answer, “why we ought to use it”. Where does the ‘ought’ come from? It is not provided by the evolutionary causal explanation, except if it could be shown that the principle in question preserves truth, and EE cannot do this without circularity. Otherwise, a causal explanation of the principle of inference to the best explanation doesn’t prescribe that a rational agent ought to believe this form of inference.

The most we can infer from these examples of Goldman’s is that causal diachronic chains (not circles of explanation) are standard explanatory strategies. Let’s recall, on the other hand, that what we are looking for is not for explanations but for justifications of our cognitive capacities, and that our question was whether circular justifications were acceptable.

EE may offer causal or genetic explanations for the innate contents of our minds, EE may explain how our mental capacities arose, but EE does not provide us with grounds for accepting the validity or reliability of our intellectual faculties. A question then arises:

Ought we to follow the dictates -if any- of evolution’s mind programming?

Otherwise, can the crucial normative aspect of epistemology be validated biologically?

This question is pertinent, because if we were to blindly follow the intellectual intuitions and drives encoded in the collective subconscious of our phylogenetic line in a remote past, we would be mere automata. Thus, one would believe in logic merely because one was biologically programmed to do so, and not because logic is correct. Thus,

I can have no justification for trusting a reasoning capacity I have as a consequence of natural selection, unless I am justified in trusting it simply in itself -that is, believing what it *tells* me, in virtue of the *content* of the arguments it delivers.

... the recognition of logical arguments as independently valid is a *precondition* of the acceptability of an evolutionary story about the source of that recognition. This means that the evolutionary hypothesis is acceptable only if reason does not need its support. At most it may show why the existence of reason need not be biologically mysterious.

(Nagel, 1997, p. 136.)

Again, Goldman's last argument doesn't prove or logically support the principle of inference to the best explanation, because to justify this principle as correct is different from causally explaining its origin, that is,

Evolutionary epistemology commits a mode of *genetic fallacy*. The "genetic fallacy" confuses the course of historical development with one of probative justification, for example, by arguing from the fact that a doctrine has a somewhat reputable (or disreputable) origin that it must be tenable (or untenable). The Darwinian epistemologist in effect argues in just this way, moving from historical survival to the presumptive correctness of methods. Surely no such *transcategorical* inference from the *factual* issue of historical considerations to conclusions regarding the issue of the *normative* validation of a method can possibly be valid. One cannot move from the historical order of temporal development to the logical order of probative concatenation.

(Rescher, p. 135.)

In contrast, the origin of a group of cognitive faculties can sometimes provide a reason to doubt the reliability (i.e., provide a 'defeater'⁶⁹) of such a group of intellectual capacities. For example, suppose I were to believe that I was created by a malevolent Cartesian demon, a demon that designed me so that I believe mostly false beliefs. Then this belief about the origin of my beliefs will provide me with a defeater for my spontaneous belief in the reliability of my cognitive capacities. Now, to have a defeater for my belief in the reliability of my cognitive capacities (R, for short) it is not necessary that I believe that in fact a Cartesian demon interferes with my mind, to have a defeater it is enough that I could not pronounce myself, one way or the other, about such a demonic scenario. In such an agnostic situation, I would have a reason to be agnostic about R and about any beliefs generated with the help of my cognitive capacities.

⁶⁹ A 'defeater' of some belief is roughly a reason (or possibly also an experience) to doubt the reasons for that belief, for a better characterisation of the concept of defeater, consult Plantinga's forthcoming paper, in Beilby (ed).

And we discover that belief in EE provides a defeater for R just as the Cartesian demon did in the fanciful previous scenario. Thus, let's grant that our cognitive capacities were selected to promote our survival, our survival, however, can be promoted by cognitive capacities that have as an aim something less than truth, this because truth is not always biologically useful, i.e., cognitive capacities that had as an aim truth could be uneconomical time and energy-wise.

If our intellectual capacities have a biological origin then these capacities are the result of selective compromises, of dirty adaptations. Natural selection, biological evolution, is not interested in true belief but in appropriate behaviour, that is, in behaviour conducive to fitness, i.e., in behaviour maximising survival and reproduction.

Hence, even if it could be somehow proved that our minds are born wired with some epistemic and axiological presuppositions, and furthermore, even if it could be shown that this programming was a result of Darwinian natural selection, this would not be enough to show that these innate mental contents of ours are the right, the correct ones⁷⁰. Because,

Complete veridicality is probably not, in evolutionary terms, cost effective. Organisms that must act to survive must process information. They must do so reasonably well, and reasonably fast. Quick computing cuts corners... Both physiological and cognitive adaptation is asymptotic; the residual gap, however small, renders **all** 'knowledge' uncertain.

(N. Tennant, p. 33.)(Emphasis added.)

Consequently, about the correctness of our cognitive instincts, some agnosticism might be prudent,

... the horrid doubt always arises whether the convictions of man's mind, which has been developed from the mind of the lower animals, are of any value or at all trustworthy. Would any one trust in the convictions of a monkey's mind, if there are any convictions in such a mind?

(Charles Darwin.)

⁷⁰ For example, wishful thinking could well be the result of biological evolution, given its adaptive value in terms of emotional well being.

And because of the rapidity with which cultural change has proceeded our present brain must have got its present capabilities very early in its evolution. Hence, an evolutionary explanation of our cognitive faculties would warrant serious doubts about the reliability of our cognitive faculties when theorising beyond a limited domain.

The question is whether not only the physical but the mental capacity needed to make a stone axe automatically brings with it the capacity to take each of the steps that have led from there to the construction of the hydrogen bomb, or whether an enormous excess mental capacity, not explainable by natural selection, was responsible for the generation and spread of the sequence of intellectual instruments that has emerged over the last thirty thousand years.

(Nagel Thomas, 1986, p. 80.)

Therefore, there would be no reason to trust the results of our intellectual capacities in endeavours such as biological evolution. Hence, an evolutionary explanation of our cognitive capacities would then also warrant serious doubts about the correctness of this evolutionary explanation itself. EE is then self-undermining, self-defeating.

II

The probable unreliability of our cognitive faculties -if these capacities were to have *only* a Darwinian origin- merits more attention. Thus, there are contrary opinions about the value of the probability (P) of our cognitive capacities being reliable (R) overall, when functioning properly, and conditional on the correctness of EE: P(R/EE). Which opinion is favoured about the value of P depends in turn on which theory is held about the causal connections between beliefs, desires and behaviour⁷¹, if one holds, for example, that from adaptive behaviour nothing follows about the

⁷¹ Cf., Plantinga's chapter 12 for more on these conflicting positions.

truth-value of beliefs then we will favour a low value for P(R/EE). Contrariwise, if adaptive behaviour makes probable true beliefs, then P will have a high value.

Quine believes that P has a high value, because he thinks that belief and behaviour are linked closely, in such a way that abundant and basic false beliefs will lead to radical maladaptive behaviour and extinction, thus,

Creatures inveterately wrong in their inductions have a pathetic but praiseworthy tendency to die before reproducing their kind.

(Quine, 1969, p. 126.)

While others like Darwin (cf., Darwin's quote above), and Patricia Churchland appear to believe that the value of P is low, possibly because they think that behaviour and belief are only weakly connected. Thus,

Boiled down to essentials, a nervous system enables the organism to succeed in the four F's: feeding, fleeing, fighting and reproducing. The principal chore of nervous systems is to get the body parts where they should be in order that the organism may survive... Improvements in sensorimotor control confer an evolutionary advantage: a fancier style of representing is advantageous *so long as it is geared to the organism's way of life and enhances the organism's chances of survival*. Truth, whatever that is, definitely takes the hindmost.

(Churchland, P., p. 548.)

Nevertheless, one can speculate that it is unlikely that our methodological instincts can be far from the mark, that they can be seriously wrong:

- i. Because our cognitive capacities appear to have served us well⁷² in the study of multiple aspects of the world, and in evaluating an enormous range of different hypotheses of diverse generality and depth. Moreover, our cognitive instincts seem to form a coherent web.
- ii. If evolution is not understood as just searching for adaptation, but rather as searching for adaptability,⁷³ and if such a reinterpreted Darwinism were correct, then the innate contents of our minds would have been selected because they provide us with adaptability to a wide spectrum of possible niches and not merely adaptation to some primitive circumstances. That is, our mental cognitive capacities would not be just the right-enough ones for some narrow set of habitats. And since, one would suppose that the ideally right intellectual expectations and rules of method provide the most adaptability value, we could expect our deepest cognitive intuitions and inclinations to be the correct ones, or at least not to be far from the correct ones⁷⁴.
- iii. The prospects for evolution to have landed on the best or the correct method(s) are high when one considers that the selective process of methodological evolution has a relatively reduced range of methodological possibilities from which to choose. Because the range of possible ultimate methods of thesis evaluation is relatively small, when compared with the astronomical number of possible substantial theses to be evaluated.

Given the conflicting good arguments about the value of $P(R/EE)$, it seems sensible - it seems rational- to opt for agnosticism about $P(R/EE)$. Agnosticism about $P(R/EE)$ implies agnosticism about R ⁷⁵, which in turn implies agnosticism about **any** belief B generated with the help of R . In particular, agnosticism about $P(R/EE)$ implies

⁷² However, to rationally assert that, “our cognitive capacities have served as well” we need these same cognitive capacities, and again there is circularity, therefore this argument is not conclusive.

⁷³ C. H. Waddington, Hahlweg and Hooker defend evolutionary progress as increased adaptability.

⁷⁴ One must keep in mind, however, that the development of cognitive capacities is just one of the many possible strategies open to a biological lineage in its way to more adaptability. Thus, other species have achieved high adaptability by emphasising efficient reproduction (i.e., brief gestation periods with abundant offspring) instead of intelligence.

⁷⁵ If one is a believer of EE , and if there is no argument against agnosticism about R .

agnosticism about belief in EE itself. The rational recommendation is, therefore, to suspend judgement about EE, in other words, it is being said that it is a-rational to hold a belief in EE. This argument can be analysed as follows:

- a) The probability $P(R/EE)$ that our cognitive capacities⁷⁶ are reliable given EE cannot be known.
- b) We then have a good reason to suspend judgement about R itself, that is, about the reliability of our cognitive capacities (assuming there is no further evidence in pro or against the reliability of all or some of our capacities.)
- c) However, if somebody doubts R then he has a good reason to doubt **any** beliefs⁷⁷ validated by R, in particular, one has a good reason to be agnostic about EE. This reservation about the correctness of EE will hold, except if there were some ulterior argument (a 'defeater') undermining the reasons to doubt EE. Now, any such defeater argument will involve at least some belief B, as a premiss, and any such belief B will also be doubtful, because R itself is doubtful. That is, B will be also the result of our doubtful cognitive capacities, and consequently B will be in doubt, as will be, the defeater involving B. Thus, in the end, EE defeats itself and this self-defeat is

⁷⁶ This premiss is a simplification since it is well possible that the reliability of some of our cognitive capacities is less doubtful than that of others. Plantinga recognizes this and says,

... we have been lumping together all of our cognitive faculties, all of our sources of belief, and all the sorts of beliefs they produce. But perhaps these different sorts of faculties should be treated differentially; clearly the argument can be narrowed down to specific faculties or powers of belief-producing mechanisms, with possibly different results for different cases. And surely the argument does apply more plausibly to some cognitive powers than to others...

... even if you think Darwinian selection would make it probable that certain belief-producing mechanisms –those involved in the production of beliefs relevant to survival- are reliable, that would not hold for the mechanisms involved in the production of the theoretical claims of science, such beliefs, for example as E, the evolutionary story itself.

(Plantinga, pp. 232-3.)

Consequently, the believer in EE ends if not with a defeater for any belief, yes with a defeater for belief in EE itself.

⁷⁷ It may appear as too strong to say that any beliefs backed by R would be doubtful, since then even a belief in necessary truths would also be doubtful. Well, if R claims that all our cognitive capacities are reliable, and if R is doubtful, then there would appear reservations also about those cognitive capacities that permit us to discover the irresistibility of necessary truths.

undefeated. In summary, there is no way to defeat our doubts about EE, once we become aware that EE throws doubts on R (i.e., on the reliability of our cognitive capacities) and on any beliefs generated via R.

- d) EE then undermines itself, because if EE were true, then we would have reasons to doubt it.
- e) Therefore, the rational recommendation is to suspend judgement about EE.

Otherwise, it is a-rational to hold a belief in EE, notice that what is a-rational to believe is evolutionary epistemology (the thesis that biological evolution, as *nowadays* understood, can *by itself* explain the reliability of our cognitive capacities.) And that it is not being said that it is a-rational to believe in biological evolution itself. On the other hand, from the a-rationality of a belief in EE it doesn't follow that EE is false, not even that it is rational to believe in the falsity of EE.

Now, if one believes -as most of us do because of an instinctive faith- that our cognitive faculties are in fact overall reliable⁷⁸, then we get a contradiction with the previous agnostic recommendation about R. An agnostic recommendation we ultimately got from EE, this contradiction would then provide an argument against the correctness of EE, i.e., an argument for the irrationality of a belief in EE.

If the recommendation for agnosticism about EE is granted, then the problem arises of how to explain the origin, and how to justify our cognitive capacities. Concerning the origin of our intellectual faculties we are left with only three main alternatives:
a) Leave it as a mystery, b) a supernaturalist (theist or deist) explanation, or,
c) Nagel's speculation about some yet unknown natural processes or laws of nature as responsible of our cognitive capacities.

As for the justification of our capacities, one must conclude that EE fails to provide a non-viciously circular justification for our cognitive capacities, and that even if it did, this justification would be dubious given the suspicion on EE of a-rationality (and even of irrationality). Then, either the justification is to be found somewhere

⁷⁸ This animal faith in R could be expressed by asserting that R has intrinsic warrant, i.e., by saying that R doesn't get its warrant on the evidential basis of any other beliefs, in particular not from a belief in EE. In other words, it would be held that R has so much intrinsic warrant that R cannot be defeated by the fact that P(R/EE) is inscrutable. But this is another way of

else⁷⁹, or we might have to resign ourselves to accept our cognitive capacities as reliable without a non-viciously circular justification, that is, dogmatically.

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saying that R doesn't require justification, because we cannot or will not doubt it. If so, what is being said is that R is a dogma.

⁷⁹ An alternative justificatory strategy could go as follows:

Our cognitive capacities -and the basic research methods that they back or generate- have shown their fitness by their continuous historical use in a *prima facie* rational community, i.e., a human community, in particular in a scientific community. Otherwise, the continued use of these cognitive capacities -or of the methods they generate- by a rational community warrant them as reliable.

Survival of some cognitive instincts in a *rational* community, however, assumes precisely what we want to explain and justify: rationality. To *rationally*, appraise, to revise and improve our cognitive methods we would have to rely on some *justified* basic rational standards. For example, one would need basic methods such as induction, and standards of what constitutes good empirical evidence; one would need these basic methods to be able to learn from experience. These basic methods are the preconditions for passing empirical judgement on the comparative effectiveness of any other method.

With these justified methodological foundations we could then proceed to learn *rationally* (via a cultural process) more about the failures and adequacies of any other methods. For this, though, we require of this justified methodological fulcrum, the problem is that the non-viciously circular justification of this methodological fulcrum appears not to be available.

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CHAPTER FOUR

DOES LAUDAN'S META-METHODOLOGY PROMISE MORE THAN IT CAN DELIVER? (PART I.)

This chapter is the first of two chapters in which I will examine Laudan's more recent theory of science (the one he published on and after 1984.) To facilitate analysis, I will focus on the methodological aspect of Laudan's proposal in this first part, while I will concentrate on its axiological facet (i.e., on Laudan's theory of scientific aims) in the next chapter.

INTRODUCTION

A traditional problem of the philosophy of science is that of providing a rational explanation for scientific theory change, i.e., an explanation that avoids relativism. In this context, there are two different forms of relativism to be avoided:

a) Historical relativism. This doctrine asserts that the history of science is punctuated by distinctive phases, and it asserts that these various scientific phases (or paradigms) cannot be rationally compared or evaluated as better or worse *vis à vis* each other, therefore which scientific phase or paradigm one prefers is something decided by extra rational considerations. For the historical relativist scientific change is akin to a change of fashion or to a change of collective taste.

Historical relativism could happen if the transition between different scientific phases were to happen, for example, with scientific substantial theories, methods and cognitive values all changing at the same time to different and incompatible ones, all changing simultaneously to those idiosyncratic of a subsequent phase. In such a case, scientific inter-paradigmatic change would be such that there would not be a fixed methodological or axiological fulcrum or 'hinge' from which to evaluate rationally the claims made by the various paradigms.

In other words, historical relativism can be the result of a holist view of scientific change, a view where transitions between scientific phases involve simultaneous and significant variation in scientific theories, methods and aims. In such a case, it will

make no sense to ask which of these different scientific paradigms is the best one as viewed from nowhere, i.e., to ask which is the best one as viewed from an objective standpoint. Nor will it make sense to talk of trans-paradigmatic scientific progress towards some supra-paradigmatic goal, since there is no such inter-paradigmatic goal. For this historical relativist, all scientific paradigms are equally correct, and any paradigm is only the best one from its own perspective, that is, each paradigm is only the best one during its own period of hegemony; each paradigm is only the best one when examined by its own community of believers.

Nonetheless, historical relativism could happen even if there were some inter-paradigmatic a-historical methodological standards. Thus, historical relativism will still follow if these inter-paradigmatic, a-temporal, standards (when applied singly) were to provide a too ambiguous advice about scientific theory preference, or if these various standards, when applied together, were to advise conflicting theory choices. In such scenarios, trans-paradigmatic rational standards would be insufficient to advice by themselves scientific theory preference, and theory choice would then require of subjective criteria.

b) Foundational relativism, this form of relativism is the result of accepting as correct and without a non- circular justification science's ultimate methodological or aim presuppositions. These unjustified ultimate foundations then have a dogmatic or axiomatic character. Relativism follows because it is logically possible to have many alternative dogmatic foundations all of which would be equally respectable or disrespectable from reason's point of view, or else, which of these unjustified alternative scientific foundations to prefer would be rationally arbitrary. Now, from this rational arbitrariness it doesn't follow that a rational agent or rational community would be indifferent about which scientific foundation to accept. A scientific community could prefer, for example, some dogmas to others, because of some psychological drives, or because of some historical or sociological accidents. Notice that one can end with foundational relativism, even if one were to have trans-paradigmatic ahistorical standards, and even if these standards were unambiguous, and even if they wouldn't provide conflicting advice, foundational relativism will

still follow, if these inter-paradigmatic standards were accepted as correct without a non viciously circular justification.

I will argue that Laudan has tried, unfortunately without success, to provide a theory of scientific change that avoids both types of relativism. In Laudan's diagnosis, both of these relativistic consequences of previous meta-methodologies, were the result of assuming a tiered or hierarchical model of rational justification, relativistic consequences that were accentuated when to the hierarchical supposition was added a second assumption: that of a holist view of scientific change.

THE HIERARCHICAL MODEL OF SCIENTIFIC CHANGE

The hierarchical model distinguishes three different scientific levels: at bottom, the level of factual theories, then a level of methodological rules, and above the other two levels, a tier of scientific aims. According to this hierarchical model the upper levels justify - and can sometimes bring to rational closure differences of opinion at - the lower levels, thus, in this hierarchical model justification always flows in a descending direction.

It is sometimes possible, for example, to rationally close scientific debates at the factual level by invoking shared methodologies, while differences of opinion at the methodological level –which Laudan believes have been widespread throughout the history of science⁸⁰ –can be brought sometimes to a rational end by appealing to shared goals. This last assertion is the result of methodological rules being thought of as the justified optimal means for the achievement of some scientific ends.

Scientific goals sit in this hierarchical model at the top echelon as supreme judges, this privileged place of scientific goals means that if any differences concerning scientific aims were to arise between different scientific groups, then there would be no extra justificatory levels to which to appeal to. Hence, in this hierarchical model rationality cannot close debates about scientific aims, because without a referee to arbitrate goal differences, different and incompatible goals can be all equally correct,

⁸⁰ John Worrall questions this supposition, Worrall argues that there have been methodological differences amongst scientists only if one characterises methodology very

and goal relativism follows. Hence, this hierarchical tradition doesn't provide a proper way to close scientific disagreement at the goal level,

... there is a point where the model [the hierarchical model] breaks down badly and repeatedly: specifically, when scientists disagree about (some of) their basic cognitive aims or goals. Since those goals are at the top of the justificatory ladder in this model, there is apparently no recourse, no court of independent appeal, when scientists differ about axiological matters. Yet differ they do.

(Laudan, 1984, p. 42-3.)

This goal relativism could then infect with relativistic consequences the lower levels, consider, for example, some scientific community promoting a set of scientific goals G1, and a different scientific community propounding an incompatible set G2, then the first community could end justifying methods M_i , while the second community could end justifying a different set of methods m_i ⁸¹. These methodological differences could in turn translate as differences at the factual-theoretical level, theoretical differences, which would be without a rational solution.

Laudan distinguishes two main inherited hierarchical models of scientific change, these are:

- i) The model of logical positivism cum Popper, and
- ii) The holist model of Kuhn et al.

Laudan believes that the logical positivists cum Popper held a gradualist or piecemeal hierarchical model of scientific change⁸², in this view, scientific change

laxly and if one doesn't distinguish between genuine –which could be only implicit- and explicit methodological pronouncements. Cf., Worrall (1988) pp. 268-72.

⁸¹ It is not sure that two scientific communities with different goals would end with different methodological rules, because different sets of scientific goals can sometimes correspond to the same collection of scientific methods. For instance, instrumentalists and realists although they differ on some scientific goals (say, truth vs. empirical adequacy) frequently share the same methodological rules.

⁸² Worrall (1988) p. 268-9, questions that positivists held a hierarchical model as described by Laudan, Worrall argues that the positivists didn't hold that axiology governs methodology.

usually involves one level at a time, and scientific change rarely happens simultaneously at the three levels of factual theories, methods and goals. Moreover, Laudan points out that for the positivist tradition scientific aims were a matter of convention or subjective volitions⁸³, thus if divergences in cognitive goals were to arise the positivist tradition couldn't solve these differences rationally, and as a result, the hierarchical positivist tradition couldn't avoid both a historical and foundational relativisms.

On the other hand, in Kuhn's model of scientific development (as described in his classic, 'The Structure of Scientific Revolutions'), there are periods of scientific consensus -or of 'normal science'- followed by periods of disagreement, or of 'scientific revolution'. Episodes of scientific revolution are in turn followed by other periods of consensus and so on, and a hierarchy of goals, rules and factual theories, all relative to a paradigm, has supremacy during the periods of consensus. Kuhn's conception is a hierarchical model with a non-gradualist view – i.e., with a holist view - of scientific change. Kuhn's holism (or the 'big-picture' view of scientific change) asserts that all three levels of the hierarchical model change 'all at once' and significantly during scientific revolutions.

But paradigms differ in more than substance, (...) They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science (...) The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.

Just because it is a transition between incommensurables, the transition between competing paradigms **cannot be made a step at a time**, forced by logic and neutral experience. Like the gestalt switch, it must occur **all at once** (though not necessarily in an instant) or not at all.

(Kuhn, 1970, pp. 103, 150.)(Emphasis added.)

⁸³ Cf., Reichenbach's , Popper's and Laudan's quotes on this topic, in the section 'Of the Aims of Science' at the end of I.1 in Chapter I, above. Also, Laudan (1984) pp. 48-9.

Leaving aside the apparent inconsistency between a change that occurs “all at once,” “though not necessarily in an instant”, this Kuhnian holism entails that there is no fixed point from where to evaluate the various paradigms, and historical relativism then follows. Historical relativism follows, because there are no a-temporal supra-paradigmatic standards from where to evaluate the various paradigms, the ultimate standard is the changing assent of the relevant scientific community⁸⁴. Thus,

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... Yet, whatever its force, the status of the circular argument is **only** that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle. **The premisses and values shared by the two parties to a debate over paradigms are not sufficiently extensive for that. As in political revolutions, so in paradigm choice –there is no standard higher than the assent of the relevant community.**

(Kuhn, 1970, p. 94.)(Emphasis added.)

⁸⁴ In the Postscript to his ‘Structure of Scientific Revolutions’ and later in chapter 13 of his ‘The Essential Tension’ Kuhn claims, however, that there are some objective supra-paradigmatic standards, scientific aims such as empirical accuracy and scope, consistency, simplicity and fruitfulness. Kuhn adds, though, that these goals are insufficient to govern by themselves theory choice, and thus in Kuhn’s view theory ranking will always be partly the result of subjective factors. The objective standards are insufficient because they are claimed to be ambiguous, that is, they can be interpreted in different subjective ways, and because it is also claimed the various standards can give conflicting advice, and therefore these standards require of weights, weights that will be decided also subjectively. In other words,

... every individual choice between competing theories depends on a mixture of objective and subjective factors, or of shared and individual criteria.
... there are always at least some good reasons for each possible [theoretical] choice.

(Kuhn, 1977, pp. 325, 328.)

Because of this insufficiency, because of this weakness, of Kuhn’s objective standards, a relativism of scientific theory choice cannot be avoided even with the help of Kuhn’s objective standards. One could then envisage bizarre ‘rational’ theory rankings with, for example, Einsteinian Relativity ranked after Newtonian Physics, or even after Aristotelian Physics, and these absurd rankings would be as rational for Kuhn as the orthodox ordering of Einstein’s theory better than Newton’s, and Newton’s theory better than Aristotle’s. For more on Kuhn’s relativism, consult J. Worrall (2000), in particular, section 3.

Kuhn's relativism is especially clear when one recalls that Kuhn also claims that those scientists that after a revolutionary period decide to hold to the old paradigm are as rational as their colleagues that opt for the new paradigm, otherwise,

On Kuhn's view, 'neither proof nor error is at issue' in these cases, there being 'always some good reasons for every possible choice' – that is, both for switching to the revolutionary new paradigm and for sticking to the old. Hence the hold-outs cannot, on his view, be condemned as 'illogical or unscientific'. But neither of course can those who switch to the new paradigm be so condemned.

(Worrall, 2000, p. 126.)

Thus, for Kuhn everyone is right, the 'elderly hold-out' and the revolutionary alike, and there is no "point at which resistance becomes illogical or unscientific."⁸⁵

On the other hand, the Kuhnian theory has difficulties explaining rationally how a new consensus -a new period of normal science- arises after a period of revolutionary turmoil. The question is how and why do the various revolutionary factions -into, which a scientific community has fragmented- coalesce into loyalty to a new reigning paradigm. Thus,

Because he [Kuhn] believes that interparadigmatic dialogue is inevitably partial and incomplete, and because he thinks that the partisans of different paradigms subscribe to different methodological standards, Kuhn can readily explain why many scientific debates are protracted and inconclusive affairs. (...) What it cannot explain so readily, if at all, is how –short of sheer exhaustion or political manipulation- scientific disagreements are ever brought to closure. If rival scientists cannot understand one another's point of view, if they have fundamentally different expectations about what counts as a "good" scientific theory, it seems utterly mysterious that those same scientists should ever (let alone often) reach a point where they agree

about which paradigm is acceptable. But without such agreement, the onset of normal science, whose existence Kuhn went to such lengths to document, becomes utterly unintelligible.

(Laudan, 1984, pp. 16-7.)

I LAUDAN'S RETICULATED MODEL OF SCIENTIFIC CHANGE

Laudan has proposed an alternative theory of scientific change, his reticulated model, a theory that intends to overcome the alleged deficiencies in the positivist and Kuhnian explanations of scientific change. Laudan's reticulated model intends to provide a rational explanation for the transitions, in the empirical sciences, between periods of consensus and dissensus, and vice versa. This reticulated model supposedly does all this, while eluding both historical and foundational relativism⁸⁶.

⁸⁵ Cf., Kuhn, 1970, p. 159.

⁸⁶ That Laudan desires to avoid historical relativism is made clear when he criticizes, for example, the Kuhnian approach for its historical relativistic consequences. Thus,

... the immediate point to stress is that Kuhn portrays paradigm changes in ways that make them seem to be abrupt and global ruptures in the life of a scientific community. So great is this supposed transition that several of Kuhn's critics have charged that, despite Kuhn's proclaimed intentions to the contrary, his analysis inevitably turns scientific change into a nonrational or irrational process. **In part, but only in part,** it is Kuhn's infelicitous terminology that produces this impression.

(...) When scientific change is construed so globally, it is no small challenge to see how it could be other than a conversion experience.

(Laudan, 1984, pp. 70, 72.) (Emphasis added.)

And that Laudan desires to avoid foundational relativism is made clear when he criticizes Worrall for ignoring foundational relativism, thus,

The central claim of the epistemic relativist, at least where standards and methods are concerned, is not that those standards change but that -whether changing or unchanging- those standards have no independent, non-question begging rationale or foundation. Even if man had been using exactly the same inferential principles ever since the dawn of science, the relativist would doubtless ask, and properly so, 'What is their justification?'

I believe that there is an answer to the relativist's challenge to show how methodological or epistemic principles can be justified; indeed, much of *Science and Values* was an attempt to sketch out one such response.

... I thus categorically reject the suggestion that the thesis that the methods of science change in itself gives aid and comfort to relativism. **What does give comfort to relativism is a failure to address the question: 'How are methodological rules or standards justified?'**

(Laudan, 1989, pp. 369-70.) (Emphasis added.)

On the other hand, Laudan's reticulated model intends to be a naturalised meta-methodology of science,

I do hold that **methodology can be and should be as empirical as the natural sciences** whose results it draws on. (That is precisely what I mean by a "reticulated" view of scientific rationality.)

(Laudan, 1987b, p. 231.) (Emphasis added.)

Laudan's meta-methodology, even while intending to be a naturalised meta-methodology, also aims to keep the normative character of traditional epistemology, thus,

Quine, for one, seems to accept that there is little if any place for normative considerations in a suitably naturalized epistemology. I daresay that Quine regards his relegation of epistemology to a sub-branch of "descriptive psychology" as a matter of boldly biting the naturalistic bullet; but in my view, the abandonment of a prescriptive and critical function for epistemology –if that is what Quine's view entails- is more akin to using that bullet to shoot, yourself in the foot.

(Laudan, 1990b, pp. 45-6.)

Because of these desiderata, Laudan also calls his reticulated model a 'normative naturalism' (cf., Laudan 1987a.) and he claims that his meta-methodology provides,

... a sketch of a *naturalistic* theory of methodology which preserves an important critical and **prescriptive** role for the philosopher of science, and which promises to enable us to choose between rival methodologies and epistemologies of science. What it does *not* promise is any *a priori* or incorrigible demonstrations of methodology; to the contrary, **it makes methodology every bit as precarious epistemically as science itself.**

(Laudan, 1987a, p. 29.) (Bolds added.)

Laudan's reticulated model is a gradualist non-hierarchical model of scientific change. It is gradualist in that it is not holist, and it is reticulated in that its view of

justification is non-hierarchical. Moreover, this reticulated model considers scientific methodological rules as the most efficient means available (efficient on light of the historical evidence) for achieving some rational cognitive ends. Let's analyse these assumptions:

I.1 Laudan's reticulated model keeps the same triad of the hierarchical model: scientific factual theories, methods, and goals. For Laudan's reticulated model, as for the older hierarchical one, the relevant aspects of scientific change can be explained just by the elements of this trio and their interactions. But the reticulated model differs from the hierarchical one, in that it abandons the hierarchical's idea of an unidirectional justificatory flow, it discards the idea of justification only flowing downwards from scientific goals to scientific methods, and then from scientific methods to factual theories. For the reticulated model, both justification and scientific change can flow in any direction, for example, from theories to methods and from these to goals.

Laudan proposes, for example, the following reticulated historical scenario: Newton's corpuscular theory of light had reigned unopposed for some time, when the wave theory of light emerged, and after a transition period, the wave theory became the new dominant optical conception. And this change from Newton's corpuscular theory to the wave theory of light in turn promoted a change from a Newtonian inductive methodology to the hypothetico-deductive one characteristic of the wave theorists. Finally, this change of methodology next promoted a change of epistemic goals, a change from an epistemic goal that condemned directly unobservable theoretical entities -a condemnation supposedly characteristic of Newtonian inductivism- to a new epistemic axiology that accepted, under certain circumstances, directly unobservable scientific concepts.

In other words, the reticulated story claims that we started with the Newtonian theories-methods-goals triad of (T1, M1, G1) and ended with the wave triad of (T2, M2, G2). And this transformation supposedly followed the following steps: (T1, M1, G1) to (T2, M1, G1), then to (T2, M2, G1), to finally end with (T2, M2, G2). Notice that the overall change from (T1, M1, G1) to (T2, M2, G2) is thought to have happened gradually with only one element of the triad changing at a time. Scientific

change is not hold to happen, as the holist model will have, with the all the elements of the trio changing simultaneously⁸⁷.

I.2 Both Laudan's reticulated model and the hierarchical model (this last, as portrayed by Laudan) view the methodological rules of science as hypothetical imperatives, as conditionals Q, of the form:

*Q: If you value epistemic goals Ai, then follow M, because the historical evidence shows that M is the available optimum⁸⁸ strategy for the attainment of Ai.*⁸⁹

Now, if I accept the antecedent of conditional Q, why should I accept the consequent? Otherwise, then, why should I follow method M? The answer is found in a more basic conditional presupposed by the previous conditional Q, this more basic conditional S says:

S: If you are going to be rational, then use that method - amongst the methods available to you - that you have good reasons to consider as optimal for the attainment of your valued goals.

This last conditional, which describes a conception of means-ends rationality, is behind all of Laudan's conditional Q's. Thus we can re-state Q as:

Q': If you are to be rational, and if you value epistemic goals Ai, and if M is the empirically warranted optimum method (of those methods available to you) for the attainment of Ai, then follow M, because this is the rational strategy to follow.

⁸⁷ Cf., Laudan, 1984, pp. 56-60, for various putative historical examples.

⁸⁸ Justification of some means as optimal is a requirement, because methods are rational not because they deliver the goods, but because they are justified as optimum for the delivery of the goods. Thus, one could imagine someone arriving at his goals by the most effective method possible and still reaching them in a non-rational way. His best method, for instance, could have been found by accident, or it could be the result of unquestioning tradition, this method would then be lacking the endorsement of reasons. On the other hand, a method justified as optimal is not the same as the optimal method, hence, an action or method might be the optimal one but not be justified as optimal, and a method can be justified as optimal and still not be the optimal method.

⁸⁹ This cognitive utilitarianism has been characterized as follows,

First, one specifies something as the goal of science. That is, scientists are taken as aiming at the production of theories of some particular kind... Second, some principle or set of principles are specified for comparing rival theories against a given evidential background. Such principles (frequently referred to as a methodology) rate the extent to which theories actually achieve or are likely to achieve the goal in question.

(Newton-Smith, p. 4)

This view of methodological rules as hypothetical imperatives makes them criticizable, or testable, empirically via the history of science -given a set of desirable cognitive aims - now, if methodological rules are empirically criticizable, then these rules can be changed empirically. Thus, Laudan claims methodological rules are as conjectural and fallible as the theories of the empirical sciences.

One difference concerning methodological rules, between the older hierarchical model and Laudan's alternative, is that while in the older model, goals were a matter of conventions or volitional decisions, Laudan instead provides an embryonic theory of rational epistemic goals.⁹⁰ Laudan believes that his theory of cognitive goals permits us -at least sometimes- to rationally choose, or exclude, some scientific aims.⁹¹ For example, Laudan believes that scientific goals can sometimes be empirically criticized and changed by examining the historical evidence to find whether these goals have been realized, and ergo to find whether these scientific goals are realizable. If there were good reasons, for example, to conclude that some desirable scientific goal is unrealizable, then Laudan would say that this fact provides a good reason to abandon it.

I.3 On the other hand, in Laudan's model, methodological rules have a normative character, even if only a conditional or hypothetical one, thus, Laudan's methodological rules tell us which strategy we ought to follow, in case we value means-ends rationality, and in case we value some cognitive aims.

Since methodological rules are taken by Laudan's reticulated model to be empirically criticizable, and since these rules are held to have a hypothetical normative character, Laudan has recently re-named his model 'normative naturalism' (Cf., Laudan, 1990.) The term naturalism refers to Laudan's pretension that his reticulated model provides a theory of scientific method as empirical as science itself. He claims that the

⁹⁰ Laudan's theory of scientific aims will be discussed in the following chapter.

⁹¹ Laudan has a maximal approach to means-ends rationality, that is, he understands rationality in what Newton-Smith (cf., p. 258) calls a maximal way: action to be rational requires besides instrumental value, of a positive endorsement of the goals and procedures involved.

assumptions of the methodology of science should⁹² be adjudicated the same way that scientific claims are adjudicated. Thus Laudan says:

Crediting or discrediting a methodological rule requires us to ask ourselves whether the universe we inhabit is one in which our cognitive ends can in fact be furthered by following this rule rather than that. Such questions cannot be answered a priori; they are empirical matters. **It follows that scientific methodology is itself an empirical discipline which cannot dispense with the very methods of inquiry whose validity it investigates.**

(Laudan, 1984, p. 40.)(Emphasis added.)

Laudan recognises, however, that such an empirical methodology would be a circular enterprise since it “cannot dispense with the very methods of inquiry whose validity it investigates.” Laudan has added recently,

Epistemic naturalism ... holds the claims of philosophy are to be adjudicated in the same ways that we adjudicate claims in other walks of life, such as science, common sense, and the law. More specifically, epistemic naturalism is a meta-epistemological thesis: it holds that the theory of knowledge is continuous with other sorts of theories about how the natural world is constituted. It claims that philosophy is neither logically prior to the other forms of inquiry nor superior to them as a mode of knowing. **Naturalism thereby denies that the theory of knowledge is synthetic a priori (as Chisholm would have it), a set of “useful conventions” (as Popper insisted)...**

(Laudan, 1990b, p. 44.)(Emphasis added.)

II CRITICAL COMMENTARIES

I will concentrate my critical comments in this chapter on two elements of the reticulated model’s triad: on theories and methods (T, M). Moreover, my criticism in

⁹²One wonders, though, how this prescription is going to be justified, it looks as an a priori

this chapter will focus on whether Laudan's reticulated model succeeds in avoiding both historical and foundational relativism. This is something one would expect Laudan's reticulated model to accomplish, since Laudan criticizes Kuhn for the historical relativistic consequences of his theory of science, and since Laudan also censures the positivist cum Popper, and also J. Worrall, for not dealing with, what I have called, foundational relativism.

II.1 Can Laudan's reticulated model avoid foundational relativism?

Normative naturalism makes various methodological presuppositions, which cannot be warranted without circularity, an infinite regress, or dogmatism. And this contradicts normative naturalism's main purported goal: to justify our cognitive methods, in particular,

a) Laudan's view of methodological rules as hypothetical imperatives presupposes scheme S, above, but how are we to justify this conditional? That this conditional needs justification is made clear by considering alternatives to it, for example, one could propose the following absurd looking alternatives:

S': 'if rational look for the justified least optimal means to your valued ends.' Or,
S'': 'if rational pray to attain your valued ends.' Or,
S''': 'if rational let chance –say, by playing at dice- decide which is the best means to your valued ends.'

Laudan does not justify his scheme for means-ends rationality, and therefore, his position assumes without justification one of many possible conceptions of means-ends rationality. This conception of means/ends rationality plays a crucial role in Laudan's scientific meta-methodology, since Laudan's reticulated model requires Laudan's means-ends rationality scheme to warrant, and to empirically criticize, scientific methodological rules.⁹³ This crucial assumption is left unjustified, possibly because a non-circular justification of this assumption is unlikely to be available.

injunction, and if so, it goes against the spirit of naturalism.

⁹³ Laudan's scheme for means-ends rationality is also required by him to conclude that unrealizable scientific aims are irrational (cf., next chapter.)

One would end by saying, ‘but it is obvious that the rational thing to do is to follow S!’⁹⁴ The alternatives S’, S’’ and S’’’ are just absurd’. This is of course a circular argument; S is preferred just because it is closer to our shared intuitions than S’, S’’ or S’’’.

Still, someone could argue that if we were to substitute S with S’, S’’ or S’’’ we would very likely be soon extinct. And that therefore, if we value our survival, we should follow S (or if not, an alternative scheme very close to it) because S is the optimum means for securing our survival. This argument, however, presupposes (though now at the meta-level) precisely what we want to justify, hence circularity or regress. In conclusion, S has a likely a priori character, and when S is accepted, it is accepted as correct without a non-viciously circular justification.

Due to this lack of a non-circular justification for S, if a believer in S were to criticize a believer of any of the alternatives to S as irrational, this last one could always retort with a *tu quoque* argument, that is,

You cannot criticize me, because you also do not justify without circularity your conditional S. I have as much right to my dogma as you to yours. All of us are ultimately dogmatic, and we only differ in which dogmas we end preferring. If because I prefer an alternative to S I am irrational, then so are you due to your preference for S.

b) If we grant to Laudan that methodological rules are the empirically warranted optimum means to some valued cognitive aims, and if we grant that these methodological rules pass judgment on empirical claims or theories, then a justificatory regress arises. Because if M1 is supported by some evidence E1, one can then ask how was evidence E1 itself warranted. And to answer this last question we would need some other methodological rules M2. Rules M2 would then warrant evidence E1, and some other evidence E2 would in turn warrant rules M2, the question about the warrant of E2 would then reappear, and to answer this last petition

⁹⁴ I will argue in the next chapter, however, that S is not an adequate representation of means-ends rationality. I will argue that S should be modified to S2: if rational search for the warranted optimum means for the attainment of, *or for the approximation to*, your valued aims.

of warrant we would go on to invoke methodological rules M3, rules which in turn would be supported by evidence E3, and so on ad infinitum.

Laudan acknowledges the possibility of this infinite regress and to avoid it, Laudan ends by presupposing our putative universal inductive convictions, R1, thus, Laudan says,

... we can avoid the regress provided that we can find some warranting or evidencing principle **which all the disputing theories of methodology share in common. If such a principle -accepted by all of the contending parties -exists, then it can be invoked as a neutral and impartial vehicle for choosing between rival methodologies.**

... I believe that we have such a criterion of choice in our normal inductive convictions about the appraisal of policies and strategies. In brief, and for these purposes, those convictions can be formulated in the following rule:

(R1) 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, 1987a, p. 25.)(Emphasis added.)

R1 stops the regress and provides the ground for trusting that our scientific methods will go on working in the future, that the future can in this matter be relied on to resemble the past. About the justification of R1, Laudan offers a couple of suggestions,

Two points are central: (1), (R1) is arguably assumed universally among philosophers of science, and thus has promise as a quasi-Archimedean standpoint, and (2), quite independently of the sociology of philosophical consensus, **it appears to be a sound rule of learning from experience. Indeed, if (R1) is not sound, no general rule is.**

(Laudan, 1987a, p. 26)(Emphasis added.)

Clearly consensus amongst methodologists in favor of R1 (even if this consensus existed, which it doesn't as exemplified by Popperians) would not be sufficient to justify R1, since clearly everybody could well be wrong, given that the consensus could be just the result of a contingent sociological fact, or it could be only the result of a psychological accident of our make up.

About Laudan's second justificatory suggestion, he does not provide any argument for the conditional "if (R1) is not sound, no general rule is", and this conditional is not obvious, as Popperians exemplify, so it should be justified⁹⁵. Nor can Laudan empirically establish that R1 is "a sound rule of learning" without circularity, that is, without assuming our inductive convictions R1. One could imagine such a circular empirical argument in support of rule R1 taking the following form:

The following of rule R1 consistently promoted our cognitive ends in the past, while rival rules failed to do so. Therefore, R1 is likely to be a more effective strategy for the promotion of our cognitive aims than actions based on rival rules.

This argument is rule circular⁹⁶ because R1 is assumed to conclude that R1 is likely to be more effective than rival rules. Furthermore, when Laudan endorses R1 as "sound", he is sneaking in an implicit prescription: comply with this sound rule of learning! And this prescription cannot be inferred from just a descriptive study of the history of science. This implicit prescription in favour of R1 is then also left unjustified. Thus,

⁹⁵ Laudan's conditional is an example of the pragmatic argument in favour of induction, an argument first proposed by Reichenbach, and later by Salmon. This argument has various difficulties but the most serious is that there is infinitude of rules that satisfy the conditional and to choose between them we require of some criterion, for example, a criterion of simplicity. And then the problem re-arises, how to justify without circularity this criterion of simplicity, or any other alternative criterion? Furthermore, even granting the criterion of simplicity or some other criterion, the selected rule amongst this infinite set of possible rules, would still have to prove that it can deal successfully with paradoxes such as those of Goodman. Because of these difficulties the pragmatic argument in favor of induction is not conclusive. Cf., Salmon, ref., and Chapter I, section I.1, above.

⁹⁶ Cf., Chapter I, for a criticism of Papineau's rule circular justification of induction.

If one is allowed to take for granted our 'normal inductive convictions' then there is little problem in justifying them. Surely the whole point of philosophy of science has been to try to articulate and defend these 'inductive convictions'. Since Laudan seems to endorse those convictions, **he is of course taking a non-descriptivist stance: someone lacking them as a matter of fact would not, as a matter of evaluation, act in a genuinely scientific way.**

(Worrall, 1999, p 355.)(Emphasis added.)

Another possible justificatory strategy would be to warrant belief in (R1) by a form of conventionalism or by assuming R1 to be an example of the synthetic a priori, Laudan, however, has rejected explicitly both conventionalism and apriorism, thus Laudan has criticized Popper for his conventionalism about scientific methods and goals, and Laudan has likewise criticized Chisholm for his apriorism about scientific methods, hence conventionalism and a priorism are not an option for Laudan.

c) Laudan's most basic methodological assumptions (which include deductive logic) cannot be open to empirical questioning because the process of empirical questioning itself requires them. Otherwise, some ultimate methodological assumptions are likely to be universal and to remain fixed, because they are essential for any human scientific research endeavor. Laudan himself provides, a possible test for the putative a prioriness of Laudan's posits,

If the naturalist is led to espouse methods which turn out as a matter of fact to be persistently bad indicators of a theory's future performance, then **experience** gives us machinery for recognizing the breakdown of those methods and doing something to patch them up... he sees in the capacity of "scientized" philosophy to **correct** itself the dispensability of other, "higher" forms of grounding.

(Laudan, 1990b, p. 58)(Emphasis added.)

It does not seem possible, however, that Laudan, for example, can even in principle give up his inductive rule, as a result of *empirical* correction or refutation, since to empirically criticize or correct this assumption of his, to test it, to refute it, Laudan

would need to presuppose the soundness of this same assumption, or if not, he will need to assume the soundness of some other even more basic assumption, because,

... The *questions* that we raise and our *doubts* depend on the fact that some **propositions** are exempt from doubt, **are as it were like hinges on which those turn.**

(Wittgenstein, *On Certainty*, 341.)(Emphasis added.)

These most basic assumptions have an *a priori*, an Archimedean point character, these methodological foundations are not corrigible empirically like scientific theories are, these assumptions are not really rationally revisable in light of any future evidence. If not, under which empirical conditions would Laudan be ready to give up these basic assumptions? And using which methods would he rationally assess this evidence? Since we can't criticize something with nothing, we need to assume our most basic presuppositions in order to identify a possible deficiency in them, therefore, these ultimate presuppositions are not amendable, because a process that relies on them cannot question these ultimate presuppositions. In other words,

...particular empirical observations can't overthrow general principles except in light of still other and superior general principles that give the observations the necessary leverage.

... **Not everything can be revised, because something must be used to determine whether a revision is warranted** -even if the proposition at issue is a very fundamental one.

(Nagel Th., pp. 22, 65.)(Emphasis added.)

Laudan could retort that he never intended a wholly empirical theory of scientific methodology, that he only intended a meta-methodology as empirical as natural science itself. Laudan could therefore be ready to grant the *a priori* character of some of his presuppositions, thus, Laudan says,

I am not claiming that the theory of methodology is a **wholly empirical activity**, any more than I would claim that theoretical physics was a wholly

empirical activity. Both make extensive use of conceptual analysis as well as empirical results. But I do hold that **methodology can be and should be as empirical as the natural sciences** whose results it draws on.

(Laudan, 1987b, p. 231.)(Emphasis added.)

This is a vague recommendation because it is a matter of debate how empirical the natural sciences are, on the other hand, why “should [methodology] be as empirical as the natural sciences”? Laudan doesn’t justify his injunction in favour of a scientific methodology as empirical as the natural sciences, therefore behind his naturalistic program also lays this basic unjustified posit. Still Laudan claims that,

... our methodological rules represent our best guesses about how to put questions to nature and about how to evaluate nature’s responses. Like any theory, **they are in principle defeasible**. And like most theories, they get modified through the course of time.

(Laudan, 1989, p. 374.)(Emphasis added.)

Now, methodology is an ambiguous concept⁹⁷ since it can refer both to:

- i) Basic methodological rules such as modus ponens and Laudan’s R1.
- ii) Non-basic methodological recommendations such as “Do double blind experiments”.

Laudan’s position on this topic could then be understood as saying that: on the whole, scientific methodological rules are empirically corrigible and modifiable through time, but not understood as saying, that it is possible to empirically correct our most basic methodological assumptions.

Again, if what Laudan were saying, is that some basic and fixed methodological rules, could be used to empirically criticize, revise and justify other less basic rules, then this view would be unproblematic, and it would agree with the meta-methodological opinion of a critic of Laudan’s reticulated model such as John Worrall⁹⁸. Thus,

⁹⁷ Worrall has argued that methodology is a vague term (cf., Worrall 1988, p. 270.)

⁹⁸ Core or invariant methodological criteria plus some factual discoveries can lead to revisions of methodological canons not in the core, Worrall, for example, argues that the

There is evidence from the history of science of the revisability of our 'methodological principles' only if these are understood in Laudan's very broad and *highly* substantive sense. The principles from the narrower domain may be substantive, but **there is no evidence that the possibility** need be taken seriously that they might be revised.

(Worrall, 1989, p. 387.)(Emphasis added.)

Notice, however, that Worrall must have had to assume some methodological standards to identify and assess the pertinent evidence, so as to be able to conclude that "there is no evidence that the possibility"... And this is the point, to defend or criticize our ultimate foundations we have of necessity to presume them, therefore these ultimate foundations are not empirically revisable.

As a pragmatist Laudan will reject the Cartesian search for ultimate justifications and argue that his approach is a tinkerist approach –à la Neurath. The tinkerist approach leaves unquestioned a corpus teórico of background knowledge, and follows a gradualist strategy, that is dealing with problems or questions only if there are warranted doubts. The tinkerist doesn't try to provide justifications for our background assumptions, at least not while these background assumptions seem to be unproblematic. The slogan of the tinkerist is: 'if it works don't fix it,' this strategy, however, appears as a means to conceal the lack of justification of the background knowledge.

Now, recall that Laudan has proposed a meta-methodology because he wants to avoid the relativism result of a lack of criteria to adjudicate amongst methods and aims, if so, he should be also interested in dealing with the warrant for his ultimate criteria, and if these justifications are unavailable, then he should admit it openly and resign himself to a non-eliminable kernel of fixed methodological dogmatic foundations.

factual discovery of the placebo-effect plus a fixed core methodological canon ("whenever possible, theories should be tested against plausible rival theories") led to a new

II.2 Can all scientific methods and aims be changeable, without falling into historical relativism?

The reticulated model claims to avoid historical relativism, even while, sustaining that all scientific theories, methods and aims are rationally modifiable. This claim, however, is problematic, thus,

a) Laudan maintains that our present methods turn out better when evaluated from our present perspective; he says that scientific progress occurs by our current lights, by our current standards,

We take science seriously precisely because it has promoted ends which we find cognitively important. More than that, it has become *progressively* more successful as time goes by. If you ask "Successful according to whom" or "Progressive according to what standards" the answer, of course, is successful by **our lights; progressive according to our standards**. Science in our time is better (**by our lights of course**) than it was 100 years ago, and the science of that time represented progress (**again by our lights**) compared with its state a century earlier.

(Laudan, 1987a, p. 28.) (Emphasis added.)

Laudan thus considers scientific progress as perspectival, that is, when we examine past science from our present cognitive perspective, we conclude that there has been a succession of past progressive steps, with all these previous steps leading to our contemporary scientific conception. This view of scientific progress recalls Kuhn's position that scientific progress was relative to the various scientific paradigms. Progress, as understood by Laudan, is progress relative to whatever methodological rules and aims happen to be dominant now, but to defeat historical relativism one would need universal and ahistorical scientific standards and aims, that is, to defeat historical relativism one has to answer,

methodological recommendation in favour of double blind experiments. Cf., Worrall, 1988, pp. 273-4.

... whether our present point of view is *right* to say that our present methods are better than the methods of science of three centuries ago. And a positive answer to that question requires some principles considered as outside the historical process.

(Worrall, 1989, p. 381.)

And these universal and ahistorical principles do not seem to be available in Laudan's doctrine of in principle transitory scientific theories, aims and methodological rules, or at least this seems true *prima facie*, since there is some ambiguity in Laudan's declarations, thus he claims,

...the history of science -unlike that of many other disciplines- offers an impressive record of actions and decisions moving **closer through time** to a realization of ends that most of us hold to be important and worthwhile.

(Laudan, 1987a, p. 28.) (Emphasis added.)

The doubt is whether these cognitively “important and worthwhile” ends are only those of us now (with different scientific communities having different favourites), or if they are atemporal. If this last reading were the correct one, then how to interpret Laudan's assertions about all scientific factual theories, methods and aims being empirically modifiable, and about progress being only relative to “our lights” and “our standards”? One way to solve this inconsistency would be to read Laudan as saying that while most of our scientific methods and aims are empirically modifiable, our most basic methods are not. But if so, then what to make of the following quote from Laudan,

The view of science now emerging in some quarters (including my own) is **Heraclitean through and through**, insisting that science -diachronically viewed- changes its content, its methods, and its aims from time to time.

(Laudan, 1996, p. 143.)(Emphasis added.)

This quote, and others like it, would then be just hyperbolic, or rhetorical, flourish, but if so, Laudan's model would have a fixed methodological and axiological core, a

core that would rationally evaluate methodological change beyond the core and thus avoid historical relativism, but then the reticulated model would become just a variation of the view that asserts that our most basic scientific methodology hasn't changed.

On the other hand, if we are to take Laudan's pronouncements at face value, then his view would not avoid historical relativism, and furthermore there would be an inconsistency between what Laudan intends his theory to be ("Heraclitean through and through") and what in fact it can be, only partially Heraclitean.

b) In the 'hierarchical' conception methods warrant theories and not vice versa, while in Laudan's reticulated model, a factual theory may force a revision of methodological criteria, and in the process justify the new, the revised, methodological rules. Laudan thinks that, for example, the acceptance of the wave theory of light (T2) exemplifies such an event, in his account, Newton's corpuscular theory of light (T1) plus Newton's inductivist methodology (M1) had been both long dominant, when the wave theory of light appeared, and most scientists eventually came to accept the wave theory -due to some of its epistemic attributes- as the best available optical theory. Laudan then goes on to claim that the wave theory once accepted, subsequently forced the deposition of Newton's inductivist methodology, and its substitution with the hypothetico-deductive method (M2.) Allegedly, this methodological change happened, because it was discovered that the wave theory's use of non-directly-observable theoretical entities (such as the luminiferous aether) was inconsistent with the recommendation made by M1 against this type of theoretical entities, therefore the wave theory promoted the change to M2, since M2 allowed such unobservable entities.

This is an attractive story, and yet, the initial sanction of T2 by M1 could not have been rational, because the dominant scientific methodology at the time was M1, and allegedly M1 condemned directly unobservable entities such as the aether, and T2 precisely assumed the aether.

If not, how could M1 have correctly warranted a theory with characteristics that it interdicts? How could M1 commit *hara-kiri* *via* the wave theory? That is, how exactly came T2 to be rationally accepted, while the scientists of the time accepted M1? Put differently,

...if the initial acceptance of the wave theory was rational, then Newtonian inductivism (as described by Laudan) was not really in force at the time.

(Worrall, 1984, p. 266.)

If *malgré tout*, Newtonian methodology legitimized the wave theory, then Newtonian methodology couldn't have condemned *tout court* directly unobservable theoretical entities. It could be instead that Newtonian methodology only advised against them⁹⁹, allowing directly unobservable theoretical entities if they provided some epistemic bonus, such as novel predictions.¹⁰⁰ But then there would not have been any reason to give up Newtonian methodology because of the wave theory's luminiferous aether, in other words,

If, on the other hand, Newtonian methodology permitted theories like the wave theory, then nothing new emerged about the wave theory once it had been generally accepted that would have required any methodological changes.

(Worrall, 1999, p 352.)

One way out for Laudan, would be to say that the change from M1 to M2 was only in the explicit or rhetorical declarations of the scientific communities involved, and to go on to claim that there was no change in the genuine, even if implicit, methodological rules followed by these scientific communities.¹⁰¹ But then, Laudan's example of methodological change as a result of theoretical change would be only a change in explicit methodology, and a change in explicit methodology can be compatible with fixed, genuine and implicit, methodological standards. These implicit standards would manifest themselves as methodological intuitive judgments,

⁹⁹ This interpretation of Newtonian inductivism gets some backing from the fact that Newton's theory itself assumed a universal force of gravity, which is an archetypal case of a directly unobservable theoretical entity, cf., Worrall, 1988, p. 272.

¹⁰⁰ What to understand by a 'novel prediction' is a matter of ongoing debate, cf. Worrall, 1988, p. 272, and Psillos, pp. 106-7.

¹⁰¹ The change in these communities' explicit methodological pronouncements would bring these explicit pronouncements closer to the genuinely held and implicit methodological canons of these communities.

hence, the wave theory would have been accepted, because scientists intuitively preferred the wave theory.

But if so, what has Laudan's reticulated model accomplished? It has provided only an explanation of change in scientists' rhetorical methodological statements. But surely the relevant philosophical problem is to explain change –if any- in the genuinely held scientific methodologies; this kind of change is the one, which would have possible relativistic implications. And since the explanation of change of genuinely held, even if only implicit, methodology is not provided by Laudan's reticulated model, it would have to be done by some other model, such as that of the 'fixed genuine methodology' camp, and the reticulated model would then be a disposable adornment of this other model.

Moreover, even if we grant to Laudan both his historical example and his reticulated model, several questions still arise:

Why should one change the methodological canons (i.e., M1) instead of rejecting the wave theory? The incoherence between M1 and T2 could have been removed by abandoning T2 and not by changing the methodological canons M1. Moreover, was the alleged methodological revision process (from M1 to M2) governed by some rules? If this process wasn't governed by any rules,

... if it happens in some particular instance that, say, T is retained and M rejected, this is simply what happens as a matter of fact. Just as in Kuhn's 'big picture', if a minority of scientists take the opposite view and hold on to M and hence reject T (or even if another minority fail to see any clash or tension between T and M!) then all one can say is that they do indeed form a minority.

(Worrall, 1999, p.345.)

The rational answer would be to claim that M was abandoned because of some deeper –possibly only implicit- normative criteria of good reason governing conflicts between theories and methodological rules. That is, relativism can be avoided only by assuming:

- i) That there are correct meta-methodological rules arbitrating conflicts between factual theories and methodological canons, and
- ii) By also assuming a prescription to follow these meta-rules.

Now, if these meta-rules, plus the prescription to follow them, are not going to be taken for granted, the problem now arises of how to justify them. This justification has not been provided by Laudan, thus these meta-rules sit, in Laudan's reticulated model, as unjudged judges¹⁰², and this lack of justification leads again into foundational relativism, on the other hand, if these unjustified meta-rules were also to change in time, we would end saddled as well with historical relativism.

CONCLUSION

Laudan has not justified without vicious circularity his most basic presuppositions, therefore he cannot avoid foundational relativism, furthermore:

Laudan can't either avoid historical relativism, if as he says, all scientific methods are changeable. Otherwise,

Laudan's model can avoid historical relativism, but only by granting that some genuinely held methods and aims –which could be only implicit- don't change, that is, Laudan can avoid historical relativism if his reticulated model only deals with change of rhetorically held methodological rules.

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¹⁰² If... it is accepted that at some level, in this case meta-methodological , there are some fixed principles of good reason -good reasons for rejecting T rather than M, say, in the particular circumstances C- then the view avoids relativism, but only by virtue of failing to be naturalistic: at least some principles are assumed to **lie outside the scientific game** as unjudged judges.

(Worrall, 1999, p. 345.) (Emphasis added.)

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CHAPTER FIVE

DOES LAUDAN'S META-METHODOLOGY PROMISE MORE THAN IT CAN DELIVER? (PART II: LAUDAN'S THEORY OF SCIENTIFIC AIMS.)

INTRODUCTION

In this last chapter, I will comment on Laudan's theory of scientific aims, but first I will briefly summarize Laudan's meta-methodological position, in order to get a clear view of the role assigned to scientific aims in Laudan's meta-methodology.

Laudan argues that if relativism is to be avoided, then cognitive aims, theories and methods should be capable of rational adjudication¹⁰³. Hence, Laudan has proposed in his *Science and Values* -and in other more recent texts- a meta-methodology of science, which attempts to avoid relativism by providing a rational justification for the methodological, and axiological aspects of scientific change.¹⁰⁴

Laudan also claims that previous philosophers such as Popper, Carnap, Hempel and Reichenbach "opened themselves up to the relativist challenge" either because these philosophers considered the methods of science a matter of convention,¹⁰⁵ or because these philosophers thought that the aims of science were settled by 'volitional decisions'.¹⁰⁶ Alternatively, because these philosophers thought that the only thing one could rationally ask of a set of cognitive aims is for this set to be internally consistent.¹⁰⁷

¹⁰³ Cf., footnote # 86 in Chapter IV.

¹⁰⁴ Laudan considers relativism as not desirable and hence he considers rationality as valuable. This is important to remember because his meta-methodology intends to be a naturalistic one. The question is whether the normative recommendations made by Laudan's theory are provided only by a descriptive or empirical study of the history of science. Or whether the normative judgements made by Laudan's theory are instead the result of tacit intuitive evaluations.

¹⁰⁵ Cf., section 'Of the Aims of Science,' at the end of I.1, in Chapter I, above.

¹⁰⁶ Cf., *ibidem*.

¹⁰⁷ Cf., Laudan, 1989, pp. 370-1

Laudan tries to provide a rational account of the development of science through a reticulated model in which justification is multidirectional¹⁰⁸, and in which scientific theories, methods and aims can change during the history of science.¹⁰⁹ Temporarily accepted methods justify the theories of the day, and are justified by temporarily accepted aims. These methods, in their turn, can also be changed by factual theories, while empirical theories and methodological rules also constrain the set of rationally possible cognitive aims. Hence, there is a mutual and typically non-simultaneous adjustment and justification among factual theories, methods and ends. Moreover, none of these three levels is an ultimate, or more solid, ground.

Rationality is for Laudan about searching for warrants to believe that one is following the most effective means, of those available, for the attainment of certain ends that one takes to be desirable. Furthermore, Laudan holds that the methodological rules of science are elliptical means-ends injunctions, 'hypothetical imperatives', of the form: *if you value, or desire 'A', then you should do 'X'*. And since experience informs us which are the best means for our chosen ends, then methodological rules are thought to be fallible, amendable and improvable via past or present experience.

Now, if Laudan's reticulated model is to avoid relativism, then he must tell us how to rationally select the desiderata in the conditionals' antecedents, the cognitive aims 'A'. If on the contrary the A's, the aims of science, were not themselves rationally selected, if any cognitive aim were as legitimate as any other, then these arbitrary aims could endorse any conceivable methodological rule. Arbitrary rules which could in turn legitimate any substantial theory, thus opening the gates to a radical cognitive relativism.

¹⁰⁸ Laudan believes that previous meta-methodologies of science had in contrast a hierarchical view of justification, where justification flew unidirectional from goals to factual theories. In this alleged older hierarchical view, scientific goals sat at the top of the hierarchy and they were used to justify scientific methods, while scientific methods justified factual theories. (Cf., Laudan 1984, chapter II.)

¹⁰⁹ Cf., Laudan, 1996, p. 143

A 'scientific' creationist, for example, could propose as the central aim of science that of finding explanatory theories consistent with a literal reading of the Old Testament. If this cognitive aim were to be scientifically legitimate, then scientists would have as central endeavours the search for, and elimination of, inconsistencies between scientific theories and Biblical texts. Scientists would then have as an important scientific goal the search for an accurate translation and reading of the Old Testament. Creationism's central aims and methods, however, would disqualify contemporary geology, palaeontology and evolution theory while endorsing the Genesis account.

Laudan admits that his reticulated view needs to be supplemented by a theory of legitimate aims –an “axiology” as he himself calls it.

... radical relativism about science seems to be an inevitable corollary of accepting (a) that different scientists have different goals, (b) that there is no rational deliberation possible about the suitability of different goals, and (c) that goals, methods, and factual claims invariably come in covariant clusters.

But here a crucial flaw appears, for what is being assumed is that a rational choice between alternative sets of internally consistent sets of cognitive goals is always impossible. This assumption, I believe, is false, not always, but in a sufficiently large range of cases.

(Laudan, 1984, p. 50.)

And

I suspect that we all believe that some cognitive ends are preferable to others. Methodology, narrowly conceived, is in no position to make those judgements, since it is restricted to the study of means and ends. We thus need to supplement methodology with an investigation into the legitimate or permissible ends of inquiry. That is, a theory of scientific progress needs an axiology of inquiry, whose function is to certify or de-certify certain proposed aims as legitimate... Methodology gets nowhere without axiology.

(Laudan, 1987, p. 29.)

Laudan has given in his *Science and Values* some indications on how to develop such an axiology. I will explore Laudan's suggestions on how to decide rationally between competing scientific aims, and I will comment on whether Laudan's proposal can avoid relativism.

I LAUDAN'S THEORY OF AIMS

Laudan hinted in *Science and Values* -and in other more recent works¹¹⁰- that our scientific aims could sometimes be rationally appraised¹¹¹ by asking that they satisfy the following constraints:

I.1 Laudan *requires that scientific goals be jointly consistent.*

It is true that I stress that inconsistent or incoherent aims ought to be rejected, but so should similarly afflicted rules and theories.

(Laudan, 1990b, p. 51.)

This way deductive logic works as an absolute constraint in Laudan's meta-methodological proposal.

I.2 Laudan *requires scientific goals to be non-utopian, a requirement that allegedly follows from a means-ends perspective of rationality,*

To adopt a goal with the feature that we can conceive of no actions that

¹¹⁰ For example, Chapter 8 of Laudan's 1996.

¹¹¹ Thus, Laudan distances himself from a philosopher such as Hume, who famously thought that reason could say very little (if anything at all) about the selection of our aims:

Reason is and ought only to be, the slave of passions, and can never pretend to any other office than to serve and obey them.

(A Treatise of Human Nature, p. 415.)

And,

It is **not contrary** to reason to prefer the destruction of the whole world to the scratching of my finger.

(Ibidem p. 416; emphasis added.)

would be **apt to promote it**, or a goal whose realization we could not recognize even if we had achieved it, is surely a mark of **unreasonableness and irrationality**.

(Laudan, 1984, p. 51)(Emphasis added.)

Laudan believes that if one is means/ends rational then one cannot have ‘utopian’ aims, because utopian aims would be of no help in selecting means. He believes rational aims should help us in selecting the best means to attain these rational aims, something that cannot be done with impossible, obscure, or unrecognisable goals. Laudan is allegedly only making a conditional recommendation against utopian aims (*if you want to be rational, then avoid utopian aims.*)¹¹² A scientific goal can in turn be ‘utopian’ in three ways:

I.2.1 A goal might be *semantically utopian*:

Many scientists espouse values or goals that, under critical challenge, they cannot characterize in a succinct and cogent way. They may be **imprecise, ambiguous, or both**. Such familiarly cited cognitive goals as simplicity and elegance often have this weakness, because most advocates of these goals can offer no coherent definition or characterization of them.

(Laudan, 1984, p. 52.)(Emphasis added.)

The concept of science itself may provide for Laudan another example of a semantic utopian concept.

I.2.2 A goal might be *epistemically utopian*:

¹¹² Laudan, however, is possibly really making an implicit categorical recommendation against utopian aims. This because in the previous quote there is an implicit categorical recommendation to be means/ends rational (since Laudan values rationality, cf., footnote #104, above) and therefore there is an implicit categorical recommendation to avoid utopian goals.

It sometimes happens that an agent can give a perfectly clear definition of his goal state and that the goal is not demonstrably utopian, but that nonetheless its advocates cannot specify (and seem to be working with no implicit form of) a **criterion** for determining **when the value is present or satisfied** and when it is not.

(Laudan, 1984, p. 53.)(Emphasis added.)

Notice that there is an ambiguity in this last quote, since it is not clear what to understand by a 'criterion'. Is a criterion something everyone in a scientific community is to agree on? Must the criterion be infallible? Can the desired criterion be intuitive? If a criterion for the *attainment* of a goal were lacking, would it instead be enough to have a criterion for deciding when one approximates the goal? For example, a criterion could both be fallible and intuitive.

I.2.3 In addition, a goal is *demonstrably utopian* when,

it cannot possibly be achieved, given our understanding of logic or the laws of nature...

(Laudan, 1984, p. 52.) (Emphasis added.)

It would be utopian, for example, to aim in an infinite or immense cosmos, for certainty about empirical universal statements or to aim to construct a perpetual motion machine. And one way to find out whether some goals are non-demonstrably utopian (that is, achievable) is to search the historical record to see if our goals have been, and therefore can be, achieved, this irrespective of whether they were consciously sought or were merely unintended consequences of some actions. On the other hand, if the historical record shows that a sought after goal has not ever been achieved, then this goal could be unachievable or merely very difficult to attain.

I.3 Finally, *Laudan proposes as another constraint on scientific goals that these goals should be consistent with the canonical achievements of a successful scientific discipline.*

Laudan's constraints of non utopianism and mutual consistency for scientific aims let in too much, that is, even if these constraints were to be sought and satisfied, one could still end with "scientific" aims that are surely ridiculous such as:

Look for theories in agreement with a literal reading of the Old Testament! Or, gather data at random! Or, seek false theories!

Laudan therefore further narrows the spectrum of possible cognitive aims by requiring that any proposal for new scientific aims must also be able to capture, to re-describe, most of the canonical achievements of any successful scientific discipline,

... any proposals about the aims of science must allow for the retention *as scientific* of **much** of the **exemplary work** currently and **properly** regarded as such.

(Laudan 1996, p. 158.) (Emphasis added.)

And the proper achievements of any scientific discipline are judged by some implicit "pre-philosophical" pragmatic canons of scientific success.

Scientists' judgments as to the **success of a scientific** practice depend not on abstract epistemological and methodological matters but on palpably *pragmatic* ones (...) Thus, a medical practice is successful or not depending to the degree to which it gives its initiates the ability to **predict** and to **alter** the course of common diseases. An astronomical practice is successful to the extent that it enables one to **anticipate future positions** of planetary and celestial bodies. A theory of optics is successful if it can (say) **predict** the path of a light ray moving through various media and optical interfaces.

... If my suggestion that there must be a **prephilosophical notion** of empirical success -which is not itself beholden to controverted epistemic or methodological doctrines- seems controversial, we might ask how it could be otherwise.

(Laudan, 1996, pp. 148-9.) (Emphasis added.)

Laudan's "prephilosophical" pragmatic canons are cognitive goals such as empirical prediction and control¹¹³, and these canons judge what is scientifically *proper*, they judge what is scientifically successful. If Laudan, however, is to avoid relativism he should justify as valuable these standards of success, and he should also justify the high weight he gives to his pragmatic canons.

Furthermore, the following questions now arise: how "much of the exemplary work" is enough to retain? What of the 'exemplary work' must be retained, and what may be omitted? Moreover, is what Laudan and many of us think of as 'exemplary work' (say, the work of Newton, Maxwell, Einstein) really exemplary? And if so why, and according to which criteria? And how do these criteria get justified? ¹¹⁴

II ARE LAUDAN'S RECOMMENDED CONSTRAINTS FOR COGNITIVE AIMS ADEQUATE?

I will illustrate many of the following criticisms with examples from non-cognitive ends¹¹⁵, because we are often better acquainted with these other goals, and hence they provide a useful and clarifying analogy. There are thus analogies between cognitive aims such as the search for verisimilar scientific theories, or to aim at simple or elegant scientific theories, and non-cognitive aims such as the 'pursuit of happiness'¹¹⁶, the search of wisdom, or the craving for love. The analogy resides in that all of these goals both cognitive and non-cognitive ones would be for Laudan semantically and/or epistemically utopian, that is, these goals are imprecise (these goals are to a big extent intuitive) and we lack a litmus test or criterion of satisfaction for all of them.

On the other hand, there are also analogies between a non-cognitive aim such as perfect social justice and a cognitive aim such as complete truth, complete empirical adequacy, or full objectivity, concerning some scientific discipline. The analogy

¹¹³ Cf., Laudan 1996, pp. 148-9

¹¹⁴ If Laudan were to answer by proposing an analysis of a selection of past scientific exemplary work, the norms of exemplariness would be there already in his selection. In other words, 'exemplary' is a normative term, and if one were to try to infer the standards of exemplariness from a selection of past scientific work, one would only obtain the standards that one started with when selecting some scientific achievements, since to select the exemplary we must first assume some standards of exemplariness. In short, the exemplary cases don't select themselves.

resides in this case in that all of these aims cannot be achieved ('given our understanding of logic or the laws of nature') and so these goals would be, for Laudan, demonstrably utopian.

If it were to be argued that examples involving non-cognitive aims are misconceived because Laudan's theory is intended only for cognitive aims, then one would expect these critics to argue why analogies can't be drawn between these two types of aims. Laudan does not offer any argument of why his requirements for cognitive aims cannot be asked of non-cognitive aims, Laudan doesn't explain why what is asked of rational cognitive goals cannot be asked of the non-cognitive rational ends. In other words, why would it be rational for a Laudanite to have utopian non-cognitive aims? The ball is in these putative critics court. It is hoped that examples involving non-cognitive aims will have Laudan admit what he denies as rational in the case of cognitive aims.

I will first comment *ambulando* on Laudan's recommendations against 'semantic' and 'epistemic' utopianism, then I will dwell upon Laudan's injunctions against 'demonstrable utopianism' and I will conclude by criticising Laudan's injunction in favour of his pragmatic canons.

CRITICISMS OF LAUDAN'S THESES ABOUT SEMANTIC AND EPISTEMIC UTOPIANISM

II.1.1 *Laudan overvalues precision when excluding as rational (because of their being semantically utopian) imprecise and/or ambiguous goals*

Aristotle's maxim that one should not attempt to be more precise than the subject matter demands is a common place; one is as precise as the problem before us requires precision. Thus, it is not reasonable to look for conceptual precision for its own sake. For example, when dealing with everyday life problems we do not go after pedantic precision in concepts such as explanation, institution, state or person. Furthermore, the search for precision can conflict with the search for epistemic

¹¹⁵ Many of the following arguments were inspired by various helpful conversations I held on these topics with John Worrall.

¹¹⁶ This aim is in the American Declaration of Independence.

virtues such as clarity and simplicity, because the search for precision will require extra terms and concepts. We should then be only as precise as the problem in hand requires, otherwise, we may end burdened with obscure and complex conceptual schemes, a situation that can be an obstacle to further theoretical developments.¹¹⁷ If the solution of a problem were to require, however, of more precision then the search for precision would be legitimate. This was the case with the Tarskian formalisation of the concept of truth, a formalisation that was needed, at least partly, because the intuitive notion of truth led to logical problems such as the ‘Liar paradox.’¹¹⁸

On the other hand, in the case of many important philosophical and meta-scientific terms and goals it appears unlikely that we will ever attain an absolute discursive precision of these terms, that is, a discursive precision that fully grasps our intuitive understanding of these terms and aims. This conclusion follows, because all definitions in order to avoid circularity or an infinite regress ultimately depend on *intuitive* primitive concepts.

As Socrates showed, we often know intuitively how to apply a concept without being able to provide an adequate explicit definition of the intuitive concept. That is, without being able to provide a definition that will apply to all the cases –an only to those cases- where the intuitive notion applies correctly, and without a definition that will let us discern the defined notion from other close notions.

¹¹⁷ Popper for example argues (‘Realism and the Aim of Science’, pp. 270-1) that if Newton and other mathematicians had listened to Berkeley’s criticisms of their intuitive concepts of the derivative and the integral, and if these mathematicians had not neglected conceptual precision at this early stage of the development of the calculus, the growth of the differential calculus could have been obstructed: “It was the neglect of precision (...) which made the wonderful development of the calculus possible.” (Ibidem, p. 271.) Popper’s reading of the History of the Calculus is however controversial, here I am just echoing Popper’s view. Faraday’s electric field concept provides another example, the electric field started as a vague and imprecise concept, which won in precision as its relations with other electromagnetic concepts became clearer through Maxwell’s equations. It is then not reasonable to condemn as irrational imprecise or vague concepts, because that would disqualify the embryonic theories where these concepts appear.

¹¹⁸ By the way, verisimilitude is a concept where formalisation is not a requisite since its wide intuitive use hasn’t led into any logical paradoxes, even so, there have been various serious efforts at clarifying the concept of verisimilitude such as that of Niniluto (cf., *Truthlikeness*, Dordrecht:Reidel, 1987.)

If it were to be argued that, the meaning of many philosophical and scientific terms is not obtained via definitions, but holistically via their place in a web of concepts or sentences in a theory or group of theories, then the meaning of these terms would be the more precise the more complex is the web of linguistic relations in which they are embedded. Now, since this relational complexity will always be finite, so will be the attainable precision of our philosophical concepts.

We may then have to resign ourselves never to grasp fully and discursively important philosophical or meta-scientific concepts and goals,¹¹⁹ concepts such as:

Verisimilitude,¹²⁰ Kuhn's scientific paradigm, the concepts of theoretical fertility,¹²¹ and those of theoretical simplicity and elegance, the concept of science itself, the concept of substance, and the goals and concepts of beauty and the good.

Still, we go on using these concepts even when we lack a full explicit definition of them, in conclusion, if to use a concept we first had to fully define it not much philosophy would be done, thus,

Philosophy is full of faulty definitions, especially of definitions which, while indeed containing some of the elements required, are yet not

¹¹⁹ Thus, Sosa claims that,

... we can **legitimately** attribute causes to an event, knowledge to a subject, and explanatory power to an account even **in the absence of any precise** and general philosophical definition or account of causation, knowledge, or explanation.
(E. Sosa, p. 99.) (Emphasis added.)

Sosa believes humans have an intrinsic and generally reliable faculty of reason and he arrives to this conclusion via an analogy with our faculty of sight. Now, given Sosa's belief in our in general reliable faculty of reason it follows that our rational intuitions, in general and in standard situations, provide us with true beliefs and correct concepts. Thus,

If our defense of the analogy between sight and reason is adequate, it sustains invoking a faculty of reason to meet the challenge of how we know principles of logic constitutive of consistency –like the impossibility of self diversity- or principles of epistemology constitutive of coherence, like the need of high consistency for coherence. **The answer in each case would be that such truths are manifest to reason.**

(E. Sosa, p. 98.) (Emphasis added.)

¹²⁰ For an argument in favour of an intuitive notion of verisimilitude, see Psillos, pp. 276-9.

¹²¹ A qualitative concept used by A. F. Chalmers and necessary for his objective theory of scientific change, cf., A. F. Chalmers: 'Towards an objectivist account of theory change', BJPS, 30, 1979, pp. 227-33.

complete. If we could make no use of a concept till we had defined it, all philosophy would be in a pitiable plight.

(Kant, A731, B759.)

If so, in the case of our most theoretical or basic concepts to aim for full discursive precision is a goal suspicious of being demonstrably utopian.

II.1.2 Truth is for Laudan an epistemically utopian aim only because Laudan's criterion of satisfaction is too exacting

Laudan thinks that truth, understood as correspondence, is an example of epistemic utopianism¹²², therefore, Laudan believes that truth is an irrational scientific goal this notwithstanding that Laudan considers inconsistency –as most of us do- as an epistemic vice (for example, he recommended jointly consistent scientific goals in section I.1 of this chapter¹²³.) If so, Laudan is saying that inconsistency is to be avoided, but surely if inconsistency is going to be avoided it is only because inconsistent theories cannot be *true*, in other words, Laudan, in spite of himself, aims at true theories.

Furthermore, Laudan's denunciation of truth as irrational is implausible given that the search for truth has been the explicit aim of many scientists¹²⁴. Moreover, we do have fallible but strong criteria for determining when truth is absent, and we also have some strictly fallible but still reasonable criteria for the presence of truth.

¹²² Cf., Laudan 1996, p. 78

¹²³ And Laudan claimed that inter-theoretical inconsistency is an internal conceptual problem in his older book *Progress and its Problems*,

The most vivid, (...) type of internal conceptual problem arises with the discovery that a theory is logically inconsistent, and thus self contradictory (...) Unless the proponents of such theories are prepared to abandon the rules of logical inference (...), or can somehow "localize" the inconsistency, the only conceivable response to a conceptual problem of this kind is to refuse to accept the offending theory until the inconsistency is removed.

(Laudan, 1977, p. 49.)

¹²⁴ Numerous scientists have highly valued and searched, at least *prima facie*, for explicative truth. Garré of Basel, a disciple of R. Koch, for example, risked his health and life by inoculating himself with staphylococci; he did this to find out whether the hypothesis of a bacterial cause for anthrax was true. Therefore, if truth as Laudan sustains is not a rational cognitive goal, then many scientist have been either grossly irrational qua scientists, or the

For the absence of truth, we have criteria such as inconsistency and empirical refutation, and for the presence of truth, we have plausible and fallible criteria such as empirical success. If not, we have at least fallible criteria for the presence or absence of rational belief, where rational belief is belief that has been justified as true. We cannot, though, maintain that “if you form a rational belief then it will be true”, the most we can assert is that:

According to all the relevant evidence, there are good reasons to think that a rational belief is true. (This is precisely what ‘rational belief’ means.)

Thus in everyday life if we have good reasons to believe a statement to be true then we consider this statement as putatively true except if there were some good reasons to the contrary. For example, if I see somebody approaching at a distance that looks like a friend of mine, then I assume it is true that my friend is approaching¹²⁵ –unless I had some good specific reason to doubt it such as my previous knowledge that she

victims of self deception, but both these possibilities are implausible. The last one, because it would demand a colossal amount of false consciousness on the part of scientists, ironically, Laudan has criticized both Lakatos’ and Worrall’s alleged view that,

... scientists, including great scientists, chronically suffer from methodological ‘false consciousness’ about what they do and why they do it... [Lakatos and Worrall] are forced to suppose that scientists are Koestlerian sleepwalkers, stumbling from discovery to discovery, reduced to incoherence and self-delusion whenever they attempt to describe what they are doing. Apart from the monumental psychological implausibility of supposing that great scientists never really understand what they are doing (but that we philosophers do), I must confess to finding it rather uncharitable to suppose that scientists’ explicit pronouncements about their principles of inference and experimental design are uniformly wide-of-the-mark.

(Laudan, 1988, p. 372.)

¹²⁵ That is, I assume that my friend plus her not directly observable character structure is approaching; in other words, to have good reasons to support the hypothesis that my friend is approaching is ipso facto to have good reasons to believe that the unobservable entities postulated by this hypothesis (such as my friend’s character) exist. These unobservable entities are thought to exist because my previous experience has shown me that their putative existence provides *the best explanation* of my friend’s personality, though,

There is, of course, a problem about how one is to judge that one hypothesis is sufficiently better than another hypothesis. Presumably such a judgment will be based on considerations such as which hypothesis is simpler, which is more plausible, which explains more, which is less *ad hoc*, and so forth.

(Harman, p. 89.)

has gone abroad. Analogously it is rational to conjecture that the entities postulated by our best-corroborated (or best confirmed) scientific hypotheses exist (from which it doesn't follow that they in fact exist) except if there were good reasons to the contrary.

Laudan argues¹²⁶ that there are reasons to doubt that empirical success and truth are linked in the case of scientific theories. Laudan denies that one can explain the empirical success of scientific theories in terms of the truth-likeness of the ontological claims of scientific theories. He argues that the history of science shows that many empirically successful scientific theories of the past although they were successful for long periods are now thought to be wrong about their ontological claims. Therefore, by a simple meta-induction we conclude that our present successful scientific theories (hence our *prima facie* truth-like scientific theories) will be shown eventually to be ontologically false.

In this vein, the history of optics from Newton to our days allegedly provides a good example of such scientific change. Thus light was first thought to be beams of material corpuscles, later on, light was imagined as transversal waves in an elastic ether, after that, it was held to be electromagnetic waves in empty space, and now, light is thought to be a collection of photons, these last are entities with a schizophrenic particle-wave nature. The ontology assigned to light has changed dramatically in the last centuries, and yet all these various ontologies have been part of various past empirically successful scientific theories. It is then held that this historical example -and others like it- support a scepticism about the ontological claims of past and present scientific theories.

Laudan's argument, however, has been criticized,¹²⁷ the criticism, very briefly, goes as follows:

Laudan's concept of empirical success is too weak and therefore on Laudan's terms scientific success is too easy to achieve. If we define, however, empirical success

¹²⁶ Cf., Laudan, 1984, p. 121.

¹²⁷ Cf., Psillos, chapters 5 & 6.

properly¹²⁸, then genuine empirical success provides a fallible criterion of ontological truth, where genuine empirical success, however, provides a truth criterion only for the ontological claims mainly responsible for the success in question. In other words, the argument is that the genuine empirical success of past scientific theories did not depend on what we now believe to be their false ontological claims, but it depended on what was truth-like in their ontology. In addition, it is claimed that this truth-like portion in the ontology of past successful scientific theories has been retained in subsequent scientific theories. In other words,

... when a theory is abandoned, its theoretical constituents, i.e. the theoretical mechanisms and laws it posited, should not be rejected *en bloc*. Some of those theoretical constituents are inconsistent with what we now accept, and therefore they have to be rejected. But not all are. Some of them have been retained as essential constituents of subsequent theories (...) if it turns out that the theoretical constituents that were responsible for the empirical success of otherwise abandoned theories are those that have been retained in our current scientific image, then a substantive version of scientific realism can still be defended.

(...) the genuine empirical success of a theory does make it reasonable to believe that the theory has *truth-like constituent theoretical claims*.

...My claim is that it is precisely those theoretical constituents which scientists themselves believed to contribute to the successes of their theories (and hence to be supported by the evidence) that tend to get retained in theory change.

(Psillos, pp. 108-9.)

Along these lines a 'structural realist' such as John Worrall¹²⁹ will argue that there are some ontological assumptions of abandoned mature scientific theories which get retained in the later reigning scientific theories. Worrall argues that these conserved

¹²⁸ The notion of genuine empirical success includes the requirement of novel predictions which are in principle testable, where a prediction P, of a phenomenon E, is novel with respect of a theory T, if E is known before T is proposed, T is not ad hoc, and T predicts E. Cf., Psillos, pp. 106-7, also for a characterization of ad hoc ness.

¹²⁹ Cf., J. Worrall, 1989b.

parts relate to the mathematical structure of a theory. In this way, our previous example from the history of optics when read by Worrall, shows that the mathematical structure of light remained constant between Fresnel's optics and later dominant optical theories. Thus, light is held to obey the same differential wave equations in Fresnel's, Maxwell's and quantum optics' theories. This wave equation is then a putative description of a mathematical structural aspect of the world (the wave equation expresses a set of relations amongst unobservable real objects).

This way, optics is realist in the sense that it appears to have captured at least approximately a mathematical structure of light. This mathematical form or structure of light has been kept through successive optical theories while it has been associated to different images, or representations, or causal mechanisms: waves travelling in an all encompassing elastic solid ether, electromagnetic waves propagating in empty space, probability waves, etc. If so, it is therefore reasonable to conjecture that optics has found some true mathematical structures, and that at least in this modest sense, *pace* Laudan, truth is, and can be, a rational aim of optics, and if so, a rational aim of at least some of the sciences. Alternatively,

Roughly speaking, it seems right to say that Fresnel completely misidentified the *nature* of light; but none the less, it is no miracle that his theory enjoyed the empirical predictive success that it did; it is no miracle because Fresnel's theory, as science later saw it, attributed to light the right *structure*.

Thus if we restrict ourselves to the level of mathematical equations - *not*, notice, the phenomenal level - there is in fact complete continuity between Fresnel's and Maxwell's theories.

... This example of an important theory change in science certainly appears, then, to exhibit cumulative growth at the structural level combined with radical replacement of the previous ontological ideas. It speaks, then, in favour of a *structural* realism.

(Worrall, 1989b, pp. 157-160.)

Now, it is not always the case that the same mathematical structure is kept in a temporal sequence of dominant scientific theories, thus one finds that in some historical scientific sequences, the mathematical structure of the once dominant theory can be approximated as a special limiting case (an example of the ‘correspondence principle’ at work) of the later prevailing theory. For example, the Newtonian equations are a special limiting case of the later Einstein’s gravitation equations, and continuity or accumulativity between these theories occurs (beyond carrying over the successful empirical content of the older theory) only at the mathematical level and this is what is understood by structural or syntactic continuity. Because of this continuity only at the structural level, the structural realist,

... insists that it is a mistake to think that we can ever ‘understand’ the *nature* of the basic furniture of the universe.

(Ibid. p. 162.)

The structural realist sustains that we can only hope to grasp some of the true mathematical relations or structures holding amongst very basic, unobservable¹³⁰ and ineffable objects in nature.¹³¹

¹³⁰ The structural realist’s conclusions may apply only to those mathematical mature sciences that deal with very basic and unobservable constituents of nature. Since in the case of sciences that deal with less basic objects, such as molecular biology, we can reasonably claim to have already seen - and manipulated - some of its theoretical objects, such as DNA and RNA molecules, say, via X ray diffraction, and by inserting and cutting segments of these molecules. Though, this could also be claimed even of particle physics, where physicists often talk of seeing protons (say, in a cloud chamber) and of spraying electrons and positrons,

Now how does one alter the charge on the niobium ball? Well, at that stage, said my friend, ‘we spray it with positrons to increase the charge or with electrons to decrease the charge.’ From that day forth I ‘ve been a scientific realist. *So far as I’m concerned, if you can spray them then they are real.*

(I. Hacking, p. 23.)

The problem here is that terms such as ‘seeing’ and ‘manipulating’ are ambiguous.

¹³¹ The structural realist then believes that one can distinguish between the structure and nature of a process or object, for example he believes that light is more than the mathematical relations that light obeys, and that this more (not capturable by mathematics?) is unknowable. For the structural realist this ‘more’ or noumenal nature of objects remains mysterious or obscure, the possibility, however, of a sharp distinction between structure and nature has been put in doubt,

II.1.3 *Laudan's prescription against 'semantically' and 'epistemically' utopian aims is unjustified because it often happens that one doesn't know, at least consciously, what one is aiming at, and still one can approach obscure goals by the 'via negativa'.*

One can aim at a goal as a sleepwalker; thus, many have tried to reach fuzzy goals even if they had to strive for them half in the dark. For instance, when one longs for somebody, it often happens that one does not really know what it is that one desires. It is easy to confuse a longing for love, or beauty, or immortality, or transcendence, or self-knowledge, or companionship with sexual desire.¹³² Thus, a personal relationship could start because of the search for fulfilment of a supposed erotic desire, just to discover that this desire is only an aspect of what we are really looking for, one discovers that the original longing was for something more than sex. What precisely that *more* is, it is something we cannot clearly express, it is a *je ne sais quoi*. Arthur Rimbaud describes such a search in his dreamlike poem "Le bateau ivre" where he portrays the journey of a seer in a tipsy boat, this navigator seer is on a search for some unnamed goal that he only glimpses. Luis Buñuel has also portrayed such a situation in his *Cet obscur objet du désir*.

Such ends, due to their obscurity, are likely to be both semantically and epistemically utopian, that is, goals like these cannot be characterised in a 'succinct and cogent way', and/or we do not have a 'criterion' for determining when we have reached them. Hence, Laudan would disqualify aiming at them as irrational.

Still, one could hope to approach an intuitive goal, by struggling to eliminate what it is not, thus, we may try to approach the glimpsed aim by following a *via negativa à la Popper*, a *via* that is as fallible as any other strategy. One hopes, for example, to promote intuitive goals such as wisdom or verisimilitude¹³³ by striving -in the first

Structural realism, to the extent that it rests on a sharp dichotomy between structure and content, and insofar as it makes only structure knowable and attainable, cannot be adequately defended.

(Psillos, p. 161.)

¹³² Cf., Diotima's discourse in Plato's Symposium.

¹³³ Verisimilitude is an example of a cognitive goal that Laudan considers both semantic and epistemically utopian and therefore as irrational, even so, many scientists and philosophers claim, for example, that it is rational to believe that Einstein's special relativity is a better approximation to truth (more verisimilar) than Newton's mechanics.

case- against cases of foolishness, or by struggling -in the second case- to reduce error. And one follows the *via negativa* **only because** one values, only because one desires, the obscure positive goals.

CRITICISMS OF LAUDAN'S RECOMMENDATION AGAINST DEMONSTRABLY UTOPIAN AIMS.

II.2 *Laudan's prescription in favour of non-demonstrably utopian aims is ambiguous*

An ambiguity becomes apparent when the paragraph quoted at the beginning of this chapter's section I.2, is compared to that quoted in section I.2.3, also of this chapter. While in the first of these quotes a utopian goal was characterised as one that could not be promoted by any actions, in the second quote a utopian aim was characterised as one that is impossible to achieve. According to the first characterisation, a goal such as social justice or the whole truth about some discipline would not be utopian, since we rationally believe that we can come nearer to them, that we can "promote" them. While on Laudan's second characterisation, both these goals would be demonstrably utopian, that is, given our understanding of human frailty and finitude, these goals are *strictu sensu* unachievable.

This ambiguity about the nature of utopian goals may be the result of confusion in what Laudan understands by means/ends rationality. Laudan says in one place that a methodological rule is rational if it *promotes* some valued cognitive end(s), just to add in the next page that a rule is rational if following it is more likely than its alternatives to *produce* the valued end(s) (cf., Laudan 1987, pp. 24-5.) There seems then to be confusion between promoting and producing some valued result. It seems that Laudan has conflated two different types of goals as 'demonstrably utopian':

- a. Type one: valuable goals known to be both impossible to attain and to approach.

- b. Type two: valuable goals known to be impossible to attain, yet known to be *continuously* approachable or promotable¹³⁴, I will call these last type of ends, ideal goals.

I will concede to Laudan that it may be irrational to accept valuable goals that are both impossible to attain and to approach (a rather uncommon kind of goal),¹³⁵ but I will argue *-contra* Laudan- that ideal goals can be adopted rationally.

To call ideal goals irrational is to assert that if it is impossible to fully attain some valuable goal, then we should forsake this goal. This would be akin to a tantrum: 'either I know I can fully get what I desire, or I don't care about it.' On the other hand, Laudan's advice against impossible goals may be cogent in a situation in which one has only one possibility: complete failure, without the possibility of partial failures, without intermediate options. In such a hypothetical situation, if the valuable goal sought is known to be unreachable, and if this goal is also known to be unapproachable, it might then be rational to resign ourselves and look for another

¹³⁴ According to both of Laudan's characterizations of utopian goals, goals that are attainable but extremely difficult to obtain can be rational. It may be, however, problematic in practice to distinguish between extremely difficult goals and impossible ones. For example, it would have been problematic for Spartacus and his followers to decide on light of the evidence available to them whether their aim (say, the abolition of slavery) was an impossible aim or just a very difficult goal.

Alternatively, consider the case of Soviet dissidents who struggled for political freedom in the 50 's, were these dissidents irrational, as a Soviet psychiatrist would say? According to Laudan, whether these dissidents were irrational will depend on whether their goal was an impossible aim, or instead merely a very difficult end. It was extremely difficult to rationally decide one way or the other in the 50's, since at the time there was not a single case of a 'communist' country that had turned into a liberal democracy.

¹³⁵ Even in this case, doubts arise when we recall that Calvinists -as declared in the Synod of Dort- aspire after salvation, although salvation might be impossible for some of them both to attain it and to promote it. This because they could be one of the unfortunates unknowingly predestined for damnation and this regardless of their faith, love of God and moral merit, or lack thereof. On the other hand, while Calvinists didn't have a criterion for election, they thought it was reasonable to suppose that most of the elect would be marked by their character and behaviour, so Calvinists had a fallible criterion of election.

Calvinists live then in a permanent state of doubt and apprehension just hoping to be one of the elect. A similar situation may arise, when one wants to be fully rational about our epistemic methods, and therefore one wants to justify induction and deduction (say, without vicious circularity, regress or dogmatism.) Such a justification is likely to be unavailable and unapproachable, but still one aims at it, although lacking the desired justification, one keeps on using induction and deduction, though, without being committed to them. One proceeds just hoping for the best, fully aware of one's precarious epistemic situation.

goal. In the case of ideal goals, however, we don't have such a radical situation, although ideals are strictly unachievable, they can still be continuously approximated, that is, there is an indefinitely long sequence of intermediate states between not achieving the utopian goals at all, and the impossible aim of fully achieving these goals.

Laudan could object that it would clearly be irrational to aim to construct a perpetual motion machine, an impossible goal given our present knowledge of the laws of thermodynamics, furthermore a perpetual motion machine is a promotable end, since we can approximate to this impossible goal by constructing more efficient machines. Laudan then could argue that this example creates a difficulty for the proposal that ideal goals can be rational. There is, however, a crucial difference between ideals and an impossible goal such as a perpetual motion machine. The difference is that while ideals (as far as we know) can be approximated indefinitely by a continuous sequence of small steps, in the case of the perpetual motion machine there are theoretical limits to how much the impossible goal can be approximated. In other words, in the case of the impossible machine the sequence of approximating steps has to end, in principle, long before the impossible goal is attained. Therefore, a perpetual motion machine is an impossible goal that after some stage is unapproximable and that is why it is irrational to aim to construct such a machine, though it is not irrational to aim to construct the theoretically most efficient machine possible. This last goal and ideals share the property of being approximable by an indefinitely long sequence of steps.

Ideals can be rational objectives if we understand means/ends rationality as the attitude of someone that searches for the warranted optimum means –amongst those available- for the attainment of, or for the *continuous approximation, to his valued aims*. Means/ends rationality then only requires that our means be at least conducive to our aims, it does not require that the rational means actually deliver the aims. Means/ends rationality excludes impossible, but continuously advance-able aims as rational only if it is understood narrowly, as Laudan sometimes seems to do. This exclusion happens only if means/ends rationality is understood as requiring that if rational we should look for strategies that *take us to our goals*.

Laudan's lack of discrimination between the previous two types of demonstrably utopian goals turns his injunction against demonstrably utopian aims into an 'imprecise' and 'vague' recommendation. Laudan's injunction against demonstrably utopian goals is then itself 'semantically utopian', and Laudan's theory of scientific aims is self-referentially inconsistent.

II.3 Idealists *aiming for valuable and strictly impossible goals (though continuously promotable ones) have been praised by legions, and these idealists have been admired precisely because of their idealism. Laudan's disqualification of ideal aims is counter-intuitive, since it contradicts these widespread historical value intuitions.* Laudan says,

We **customarily** regard as bizarre, if not pathological, those who earnestly set out to do what we have very strong reasons for believing to be **impossible**.

(Laudan, 1984, p. 51)(Emphasis added.)

Perhaps we customarily judge thus, when considering common goals, but one is not governed by customary judgements, when assessing extraordinary cases. Thus, the epithets of bizarre, pathological, or unreasonable, are frequently withheld if the impossible but promotable aim sought is considered to be extremely valuable. In such a case the subject, (or generations of subjects) who struggles, or who is thought to struggle, after ideal aims will not be called mad or bizarre. He (they) will instead be considered an idealist, a hero, a martyr, a courageous man, or a saint.

The revered individual has often been the tragic idealist who aims at impossible, but continuously or indefinitely promotable goals, even if this idealist has to take arms against a sea of troubles, and even if during his lifetime he cannot prevail. The standard reading of Socrates' conduct after his trial provides a well-known example of idealist conduct. Socrates chose to stay in Athens even after the death penalty had been pronounced against him, Socrates didn't flee (which he could have done) because he allegedly thought that the correct thing to do, was to be self-coherent, to

be true to himself, to be true to his sense of justice, and to obey his city's laws¹³⁶. Now, full personal and intellectual integrity is an impossible aim because of human frailty, and because its full attainment would require of full self-knowledge, it would require of no self-deception, of no inner hypocrisy. Still Socrates had it as an aim, and he was ready to sacrifice his life for this aim, would we call Socrates irrational by aiming at this end? ¹³⁷

Laudan may likewise say that all those Christians that have aspired to be like Christ, and have aimed at a *perfect* Christian life are irrational, qua religious persona. An example of such a Christian would be St. Francis; Laudan may disqualify Francis as irrational¹³⁸ because to strive towards perfection is irrational. It is irrational because we cannot expect human perfection, still Jesus asks his followers to seek perfection,

Therefore you shall be perfect, just as your Father in heaven is perfect.

(Mathew 5: 48.)

The various Christian Churches, for example, advice their faithful to struggle for the ideal of Christian marriage and also these Churches recommend the paradoxical looking norm: 'love thy enemies', even though these Churches must be fully aware that perfection is humanly impossible to attain.

Laudan could retort, that while he excludes impossible goals as rational, he is not excluding as rational some achievable goal close to, or similar, to the unattainable one. He may argue that many admired idealists supposedly striving after an impossible aim were really striving for less ambitious achievable goals.

¹³⁶Cf., Plato's 'Crito'.

¹³⁷ Oscar Wilde, at the Cadogan Hotel in 1895, after his failed action against Lord Queensberry, rejected -as Socrates had done before- the achievable option of flight to await instead inevitable arrest. Wilde may appear in this act, to be self-destructive and irrational, however, in another reading of this event, Wilde's act shows him to have been determined not to yield to the pressures of a hypocritical society. Wilde stayed in England, and did forced labour, because at the Cadogan Hotel, Wilde decided to search an ideal, the ideal of self and social consistency, the ideal of self and social authenticity. Wilde stayed because he wanted to fight hypocrisy, and he was ready to suffer forced labour for the sake of this goal. A goal -that given what we know of human nature- is an impossible goal, and it is a goal that can only be approached.

¹³⁸ Francis' goal may also be irrational for Laudan, because it is possibly also both semantically vague, and epistemically utopian.

However, this let out doesn't work, we try to reach ideals, because any specifiable attainable aim would be palpably deficient as a substitute of the ideal goal. For instance, if we substitute the ideal of a perfect Christian life for an attainable goal such as a less than perfect Christian life, the substitute goal loses much of its appeal or value. Thus, we aim at the ideal even if we know we are condemned -as Sisyphus- to always fall short of the ideal aim.

Besides, how much personal integrity, or virtue, or justice, or truth, or Christian perfection, would be rationally sufficient or appropriate? Since there is no cogent way of specifying in advance how close, or how far from, or how similar to, the ideal is good enough, we aim for the ideal itself.

A Laudanite might still argue that all the previous examples of 'idealists' are wrong, because all the individuals mentioned were not genuine idealists. He could argue that all of these individuals were not really striving after demonstrably utopian aims, but were rather trying to satisfy their vanity, or were looking for power, or for some other non-utopian goal. Even if this were the case, these individuals have been admired, because they have been believed to be genuine idealists; in other words, the argument here only needs to assume that idealist behaviour has been widely held to be admirable¹³⁹. This common esteem for idealist behaviour appears to contradict Laudan's epithet of "irrational" or "pathological" for idealist conduct, and this even, if we were to grant that idealist conduct has never been genuinely exemplified by anyone.

Laudan's proscription of ideals as irrational contradicts what we know about common human valuations and behaviour. It contradicts what we know about the behaviour of admired idealists, as well as what we know about the behaviour of the admirers of idealists. Laudan's advice contradicts our understanding of persistent regularities, in this case, those regularities relating to the behaviour and valuations of at least a significant portion of humanity: idealists and all those that admire idealism.

¹³⁹ It could still be argued that the numerous people that have admired idealist conduct have been the victims of self-deception, that they have really admired something else, but what could this something else be? Besides, the hypothesis of self-deception would require of a colossal amount of self-deception, or false consciousness, which appears as an implausible thesis.

These transcultural psychological regularities could well be the result of one or more subjacent human psychological dispositional properties, or psychological laws of nature. If so, Laudan's recommendation against demonstrably utopian aims is itself under suspicion of being 'demonstrably' utopian. Laudan's recommendation is suspect of being precisely what it condemns, and then Laudan's anti-utopianism is suspect of being self referentially contradictory. If so, Laudan's meta-methodology has too exacting standards. If not, consider the following set of three theses:

- i. With Laudan, sustain that idealist behaviour is irrational.
- ii. Notice that in our culture 'irrational' is a term with derogatory implications of foolishness or madness. And
- iii. Consider the empirical fact that through history there have been idealists¹⁴⁰ aiming at valuable ideals, and consider the empirical fact that many of these idealists have been widely admired *qua* idealists.

This set appears to be incoherent, since from (i) and (ii) one concludes that idealists are foolish, or crazy, and this conclusion clearly clashes with (iii). One could try to escape this incoherence through one of the following options:

- a. Conclude that the term 'irrational' whatever our *de facto* social use says is not a term of disapproval or abuse. However, to conclude this one would have to ignore an empirical fact.
- b. Assert that idealists aiming for valuable ideals -whatever their numerous admirers have said- are not admirable *qua* idealists, and are even despicable. It is counterintuitive, however, to say, for example, that Socrates search for intellectual and personal integrity was "bizarre" or "pathological" (cf., Laudan's quote in this section.)
- c. Conclude that the search for very valuable, strictly unattainable, but promotable goals is rational, because a world without such utopian goals would be for many an impoverished world, and if ideals were irrational, then full rationality wouldn't be desirable for these many.

¹⁴⁰ Or at least, there have been people widely believed to have been idealists.

Still this argument is somewhat weak, we only know that the set of theses (i)-(iii) is incoherent, but logic does not tell us which of these theses to give up, and which to adopt. I explore below, some further arguments for adopting option (c), none of these arguments taken in isolation will be conclusive, but their sum may carry considerable weight.

II.3.1 *Laudan's recommendation against ideal aims is in fact a prescription for intellectual and moral complacency, for mediocrity or opportunism.*

Laudan's recommendation against ideal aims discourages us from aspiring after excellence, cognitive or otherwise. Laudan's recommendation is contrary to a traditional virtue: courage, a virtue necessary to lead a good life, Laudan's advice substitutes courage with conformism and stoic resignation. For Laudan a conformist or resigned slave would be rational, but a frustrated idealist –such as Spartacus- who would not conform, say, because of his aspiration after an impossible, but promotable, aim such as perfect social justice would not be rational.

Laudan seems to have confused success, expediency, with the struggle to do the right or correct thing, for Laudan, success understood as the attainment of attainable goals is the ultimate goal, success is Laudan's idol, and then Laudan is prescribing opportunism.

Success, however, cannot be the ultimate standard, it cannot be the ultimate value, because we can always ask: is the success sought (i.e., the attainment of the attainable goal) right? Is the success sought just? Is the success sought desirable or valuable? For example, if the cognitive aim sought is reliable predictions or control of nature, we often think of it as undesirable, if to achieve it, human or animal suffering is required. This is shown by the restrictions on human medical experimentation and by the ongoing debate on animal experimentation.

In addition, pyrrhic victories, and unjust victories (in the case of these last, as shown by the ongoing debate on just war) are often thought undesirable. And we may even value a defeat, an example is provided by the battle of Kosovo that Serbs -and their Hungarian and Albanian allies- lost in 1389. And yet this defeat has been hallowed

by Serbs for centuries “in several great heroic ballads”¹⁴¹ possibly because it is believed that in that lost battle some ideal value was sought or defended (say, liberty, or honour.) Analogously we sometimes also value failed past theories (failures as judged by Laudan’s pragmatic canons of scientific success) because these theories suggested new perspectives or problems, possible examples of such theories are those of Aristarchus and of ancient Atomism.

II.3.2 The fact that ideals are humanly impossible to attain, and that one can only approach ideals, provides paradoxically a powerful psychological reason for striving after valuable ideals, because striving after valuable ideals can create an enduring emotion of self-respect.

Open-ended valuable goals can be more fulfilling because they permit us to move forward, because the journey is often more rewarding than reaching the destination. The idealist aims for ideals because he wants to keep on improving his accomplishments, because he believes in the perfectibility of life on earth, ideals help him in avoiding self-complacency. Ideals provide aspirational goals, regulative principles, which guide the idealist’s imagination, which guide his hopes and energies, even if the idealist cannot ever fully expect to achieve his ideals. Thus,

human reason contains not only ideas, but ideals also,... [ideals which] have *practical* power (as regulative principles), and form the basis of the possible perfection of certain *actions*... The wise man (of the Stoics) is... an ideal, that is, a man existing in thought only, but in complete conformity with the idea of wisdom.

(Kant, A569, B597.)

¹⁴¹ “Serbs and their allies suffered a defeat that has become hallowed in several great heroic ballads. (...) They have become lenses through which subsequent creators of national mythology have come to see their past, endow it with deep metaphysical import, and imagine the attributes of the nation in essentially spiritual terms. Kosovo was turned (especially during the 19th century) into the Jerusalem of the Serbs.” History of Serbia, Encyclopaedia Britannica CD 99 Multimedia.

Contrariwise, in the case of non-utopian goals, one often suffers a letdown, after the transitory pleasure one experiences, once these goals are reached; then one wonders: what else is there? ¹⁴² It is continued hoping and continued striving that propels a person through life, this psychological fact, supplies one reason for aiming at ideals. Thus,

It is better to travel hopefully than to arrive.

(Old English saying.)

Furthermore, a life's struggle after ideals can cause -at least in certain temperaments- lasting emotions of self-respect or self-esteem and these emotions are necessary for a good life¹⁴³. Therefore, it may be rational -at least for some temperaments- to strive for ideals and their *concurrent* emotions of self-esteem.

Consider, for instance, the case of an idealist such as Sir Thomas More who sacrificed the Chancellorship and his life to be true to himself¹⁴⁴. Thus, before being executed, he said to the onlookers that he was dying,

... the king's good servant and God's first.

(Emphasis added.)

Sir Thomas' aim unfortunately clashed with the Royal absolutism then in vogue, but St. Thomas prioritised personal integrity whatever the cost. Now, if one takes into account More's situation (i.e., the background beliefs and valuations of St. Thomas and those of an important sector of his European contemporaries) one then discovers that he and many of his contemporaries considered his conduct as praiseworthy.¹⁴⁵

¹⁴² This psychological fact may be exemplified by a phenomenon such as that of the idealised and valued Medieval 'courtly love'. This love was a longing that lasted as long as it was not physically satisfied, and therefore the lovers avoided physically consummating their love, this was a love, which on the other hand, could not be consummated without violating the partners' previous marriage or fealty vows.

¹⁴³ For Rawls (cf., section 67) self-respect is one of the primary goods, that is, one of the goods necessary for the framing and successful execution of a rational plan of life.

¹⁴⁴ This is a strictly impossible aim, as I argued when discussing the case of Socrates, therefore, from Laudan's perspective More's goal is an irrational aim.

¹⁴⁵ Thus,

Hence, More's conduct very likely provided him with self, and social esteem; in other words, there is a link between the search for ideals and positive emotions, such as self-esteem. These positive emotions could arise only if both Saint Thomas and his contemporaries (or at least, those that Sir Thomas esteemed) believed that More was really aiming at some valuable ideals, and not just, for example, at fame or prestige. What his contemporaries probably admired in Sir Thomas was his heroic effort to be true to his own values and principles, that is, they probably admired his *enkrateia*.

The search of valuable ideals can likewise provide whole communities with generalised emotions of self-respect¹⁴⁶. This fact has been known and exploited, for example, by army leaders. These leaders take care to motivate future combatants by convincing them that the war they are to engage in is a just war, a war that aims at ideals, such as democracy, justice, freedom, honour, glory, etc. An army that believes that it is fighting for valuable ideals is a motivated army, and such a collective belief increases the likelihood of this army's heroic behaviour. In the case of scientific communities, one may speculate that those scientific communities that aim (or believe to aim) at utopian goals such as truth gain in self-respect, and therefore such communities also gain in motivation.

In Laudan's tripartite reticulated model of substantial theories, methodological rules and goals, emotions have been left out, possibly because we ignore so much about the nature of emotions and about their possible rationality. But as the previous example suggests, a complete theory of rational human action, in particular a complete theory of rational scientific action, may need to consider emotions.

The news of More's death shocked Europe. Erasmus mourned the man he had so often praised, "whose soul was more pure than any snow, whose genius was such that England never had and never again will have its like."

The official image of More as a traitor did not gain credence even in Protestant lands.

(‘Sir Thomas More’ in Encyclopaedia Britannica, CD 99.)

¹⁴⁶ The ideals of one community clearly don't have to be the ideals of any other community, hence, one could imagine a community of psychopaths or Nazis -who having a coherent

RELATIVISTIC IMPLICATIONS OF LAUDAN'S THEORY OF SCIENTIFIC GOALS

II.4 *Laudan does not justify as valuable his pragmatic canons of scientific success, and therefore relativism as characterised by Laudan threatens*

Laudan told us that scientific aims ought to be consistent with his pre-philosophical pragmatic canons of scientific success (such as, empirical prediction, and control, cf., this chapter's section I.3 above.) These pragmatic canons allegedly distinguish the success of science -the scientific Tradition¹⁴⁷- from the success of other disciplines, also with a tradition, such as for example philosophy or theology.

Laudan's pragmatic canons provide then a *de facto* demarcation criterion between successful science and other cognitive endeavours, and this demarcation criterion has the character of an intuition, since Laudan told us that his pragmatic canons are "pre-philosophical" notions. This notwithstanding Laudan's rejection of intuitionism,

... we will have no need for our '**pre-analytic intuitions**' about concrete cases, or for value profiles of the 'scientific elite', or for any other form of **intuitionism** about concrete cases. (...) The naturalist metamethodologist, as I have described him, needs... **no prior assumptions about which disciplines are 'scientific' and which are not.**

(Laudan, 1996, pp. 137-8.)(Emphasis added.)

Laudan seems to be saying (again, cf., this chapter's section I.3, above),

If you are to be rational, and if you want to do successful science, then you should not ignore my pre-theoretical pragmatic canons of empirical success.

The question now arises of how to justify this conditional norm,¹⁴⁸ if one rejects, as Laudan has done, justification in terms of intuition, convention or stipulation¹⁴⁹, then

alternative morality to ours- would get emotions of self-respect by aiming at what we would consider criminal goals.

¹⁴⁷ Cf., Laudan, 1996, p.146.

¹⁴⁸ There is in this conditional an implicit categorical prescription in favour of the pragmatic canons, since Laudan would not call someone who would ignore his pragmatic canons, while wanting to do successful science, fully rational, moreover, 'rational' is for Laudan a term of praise (cf., footnote # 104.)

we may look for an empirical justification of the conditional. Laudan believes that as a matter of historical description the successful sciences have satisfied his pragmatic canons, and that therefore the conditional in question follows from the historical evidence. Nevertheless, why aren't the canons of other activities prescribed to whoever wants to do successful science? For example, why aren't theology, philosophy, scientology, creation science, or even demonology, taken as examples of *bona fide* scientific disciplines, as examples of successful sciences?

Laudan has selected some disciplines as examples of successful science, because *they fulfil his preconceptions or intuitions* (intuitions which are also ours, but not those of 'creation scientists') of successful science, and of course, the disciplines so chosen then exemplify his pre-philosophical canons of successful science. It couldn't be otherwise. We are then left with pre-analytic canons, which are merely declared as idiosyncratic of successful science. We are then left with some intuitive canons that are dogmatically asserted as those of scientific success,¹⁵⁰ this claim then has an a priori character, an a prioriness which contradicts Laudan's intended naturalism.

The situation is analogous to that of someone who would say: *if you want to be just, do as St Francis!* If we ask why do as St Francis? Then we would be answered, *because the just, in fact, behave as St Francis.* Since the just, however, don't select themselves, the following questions now arise: which standards were used to select the just? And why weren't Henry VIII, Richard II, or Francisco Pizarro selected as archetypes of the just?

The answer may be that some individuals were selected as just, because their conduct is consistent with widely held "pre-philosophical" preconceptions or intuitions of justice (these 'pre-philosophical' preconceptions of justice are not shared by all, for example, not by Hitler.) Then, of course, the individuals so selected, in fact exemplify our pre-philosophical canons of justice. The problem is now to justify as correct the preconceptions or intuitions that helped to select the allegedly just

¹⁴⁹ Laudan has criticized Popper for his conventionalism about scientific aims and methods, and he has criticized Lakatos for his intuitionism. Cf., Laudan (1996), pp. 15- 16.

¹⁵⁰ Otherwise, to study the sciences we must have some pre-theoretical notion of what science is, in order to recognise and select the sciences, and with this pre-theoretical

individuals. If this petition of justification is not satisfied, then we could rightly conclude that it has merely been dogmatically asserted that St Francis conduct was just¹⁵¹.

Then relativism threatens because if Laudan's pragmatic standards have to be taken for granted, if they have the logical character of dogma, then the logical possibility arises of a Babel of different dogmatic canons. The creation scientist, for example, could reject Laudan's canons and invoke other standards; standards, which the creationist could rightly argue, are as dogmatic, as irrational, as Laudan's.

Laudan may argue that to ask for justification all the way down to the 'bedrock' is unreasonable, that it is unreasonable because bedrock justifications cannot be provided.

Laudan may argue that to aim at such ultimate justification is a 'demonstrably utopian' cognitive aim,¹⁵² and that therefore it is an irrational aim. Still, Laudan himself has told us that what gives comfort to relativism is the lack of justification of methodological rules *or standards* (cf., footnote # 86 in Chapter IV, above.)

Furthermore, Laudan's pragmatic canons are *de facto* scientific aims or standards, though of a very general character, since they apply to all scientific disciplines. For example, to abide by the canon that we should prefer theories that make successful predictions is the same as to set prediction as a goal that must be fulfilled by all scientific theories. This becomes especially clear when one notices that these canons "*serve as certifier or de-certifier for new proposals about the aims of science*"¹⁵³, so these canons are the supreme scientific aims, the aims that judge any other scientific aims. And if we are to accept Laudan's directive on how to beat relativism, we must then try to justify these canons, moreover, since a non viciously circular justification is, and apparently it will be always unavailable, then one must conclude that

presupposition as a background then we can go on to demand that any philosophy or methodology of science be compatible with it.

¹⁵¹ This argument is inspired in a similar argument of J. Worrall, cf., Worrall, 1999, p. 348.

¹⁵² It is a 'demonstrably utopian' aim, because if we understand proper justification (as Laudan does) as an argument in favour of a statement, method, or goal, then logic tells us that since every argument has premisses, the search of justification must lead to an infinite regress, circularity or dogmatism.

¹⁵³ Laudan, 1990b, p. 53.

relativism -as characterised by Laudan- is likely to be unbeatable¹⁵⁴. To beat this relativist threat Laudan would require a criterion of rationality by which to judge his pre-philosophical canons, and Laudan should then proceed to justify – or if not, at least try to explicate- this prior criterion of rationality. But both justification and explication are missing. In sum, Laudan advises against demonstrably utopian aims, but at the same time he appears to have an impossible or demonstrably utopian aim: the goal of avoiding the relativism that results from a lack of justification.

Notice also, that Laudan’s pragmatic canons are *de facto* ahistorical and universal basic scientific aims, this because these canons judge the success of any past or present mature scientific discipline.¹⁵⁵ Thus, these canons judge the success of disciplines as dissimilar as medicine, optics and astronomy (cf., second quote in this chapter’s section I.3, above) The fixed and universal character of these canons contradicts, however, Laudan’s thesis that the aims of science can change.

The view of science now emerging in some quarters (including my own) is **Heraclitean through and through**, insisting that science -diachronically viewed- changes its content, its methods, and its aims from time to time.

(Laudan, 1996, p. 143)(Emphasis added.)

II.5 Even if we grant to Laudan -without justification- that his pragmatic canons of scientific success are valuable scientific aims, he also needs to assume -again without justification- that his canons are dominant amongst valuable scientific goals

Laudan requires that scientific goals -amongst these one would expect to find his pragmatic canons of scientific success- should be jointly consistent. Mutual goal consistency, however, is not a trivial matter, because our aims are not always completely independent, hence, acting to fulfil some aims may make it difficult or impossible to achieve others. This difficulty arises because a rational life does not consist of a series of successive actions, each one directed at satisfying one or

¹⁵⁴ Or as John Worrall has argued: “relativism as Laudan defines it, is inevitable.” (cf., Worrall, 1989, p. 381.)

¹⁵⁵ In the case of physics, say, from Newton onwards.

another of our goals. Our different valuable aims then have to be somehow negotiated or sacrificed to be made complementary, to be accommodated into a coherent whole. Hence, goal debates often merely have to do with diverse ways of weighting ends or values, and not with the selection of the set of valuable aims itself.

For example, a British Labourite allegedly gives more weight to social justice than a Tory does, though both might share the same list of liberal values. As another example, assume that two XVIth century astronomers share the same cognitive values, and assume that both astronomers share the same value ranking, except that the first astronomer gives more weight to conceptual simplicity, while the second one gives a higher rank to inter-theoretical coherence. If so, our first astronomer may end preferring the Copernican system, because of its higher conceptual simplicity, while the second scientist may side with the geocentric system, because of its higher coherence with Aristotelian physics and cosmology.¹⁵⁶

Another tension between cognitive aims¹⁵⁷ is exemplified by the incompatibility between clarity, precision and brevity (cf., this chapter's section II.1.1, above), there are, moreover, incompatibilities between many cognitive aims with other type of goals, such as social usefulness, psychological well being, and moral ends. This last case has been exploited by fiction writers with the character of the 'mad scientist' or technologist such as Dr. Frankenstein. Examples of every day life contradictory aims, or of aims that are at least partially incompatible, are:

- The tensions between social egalitarianism and individual freedom.¹⁵⁸
- The incompatibilities between preservation of life and quality of life, as illustrated by the axiological debates around abortion and euthanasia.
- The inconsistencies between economic growth, standard of life, and a healthy ecosystem.
- The inconsistencies between full employment and no-inflation in a market economy.

¹⁵⁶ Cf., chap. XIII of Kuhn's *The Essential Tension*.

¹⁵⁷ Cf., chap. XIII of Kuhn's *The Essential Tension*, where Kuhn discusses some other examples of incompatibilities between cognitive aims.

¹⁵⁸ This example of incompatible aims was argued at length by I. Berlin, cf., p. 12.

- The tensions between individual freedoms and community values, for example, the case of individual private property vs. communal property.
- The tensions between freedom of speech, the preservation of life, and physical and moral integrity, as exemplified by the axiological debates about child and sado-masochistic pornography.

Incompatibilities between goals can lead, when unsolved, to a Buridan's ass's situation, hence, it is necessary to know how to prioritise weight or reinterpret aims to combine them in a new consistent synthesis¹⁵⁹. However, there are many possible value hierarchies allowed by reason, and this situation holds even after full deliberation of these value hierarchies' foreseeable consequences. This because to evaluate the consequences of hierarchies, to arrive at judgements in pro or in con, requires in turn of some other values, or meta-values.

Thus, one could arrive at the rational evaluation of some value ranking by considering whether this ranking's foreseeable consequences are conducive to the attainment of some ulterior goal(s), or rather meta-values. But if one tries to go on to justify as valuable some of these meta-values, while excluding -as Laudan would like to do- justification by convention or intuition, then one will end once more with Sextus trilemma. In addition, if the regress and viciously circular justifications are to be avoided, then we are left only with dogmatism.

Therefore, axiological inconsistencies in the end will have to be dealt with by ranking values by appealing to dogmatic prejudices about what is important or relevant. The harmonisation of aims is then a question decided by biographical or historical accident, not by reason, in other words, which value ranking you end preferring is a matter of conviction, not logic. This means that even if different rational communities were to share the same values, they still could have different

¹⁵⁹ The weighing of ends is also needed to fine-tune the means chosen to approach or attain some aims, since the means are often underdetermined by the desired end states. If the only aim of a community were, for example, egalitarianism the way it was approached (say, through revolutionary terror, or through gradualist reform) would be irrelevant. Hence, to help narrow the under determination of chosen means, other weighted aims are needed, aims such as human rights and democratic values. If not, one could end with results as dissimilar as Maoist China and the Japan of the 60's, two very different societies that were allegedly quite egalitarian.

value rankings, moreover, none of these various value hierarchies could be shown to be rationally better than any other, except, from their own meta-perspective.¹⁶⁰

If a pluralism of value hierarchies is to be innocuous, if it is not going to become an anarchic relativism where anything goes, then it must give priority to some aims, to confine the universe of value hierarchies to those acceptable. For example, in the case of contemporary liberal democracies, the pluralism of life styles allowed by these societies is far from being full relativism; contemporary democratic liberalism is restricted by the priority given to values such as human rights, and tolerance.

Similarly, if a pluralism of scientific value hierarchies were to be innocuous, it would have to be restricted by postulating that some scientific goals should have priority in all acceptable scientific value rankings.

For Laudan the dominant goals are likely to be his pragmatic canons, Laudan needs scientists *qua* scientists to value his canons, but Laudan also needs scientists to give his canons priority over other cognitive desiderata. Consequently, if these canons were to be valued but given a low weight, if one were to emphasise in their instead, say, audacious speculation plus theoretical beauty, then one may end doing something closer to contemporary French philosophy than to empirical science.

Laudan has not justified his prioritisation of his canons, therefore Laudan's prioritisation of his canons has to be taken for granted and it has a dogmatic character, a dogmatic character that leads into relativism as understood by Laudan.

He says,

... when values are shared but not weighted equally, and when values are not fully shared, we seem to be confronted by an irresolvable disagreement - irresolvable, that is, if we stick to the limited resources of the classical hierarchical model.

(Laudan, 1984, p. 41.)

¹⁶⁰ The resulting pluralism of value hierarchies implies that there are many possible rational plans of life, or many possible rational scientific conducts. The awareness of this axiological fact may be an antidote against the danger of fanaticism, a danger to which the search for ideals can lead. The fanatic is the narrow-minded idealist who considers his ideals –and his high ranking of his ideals- as the only legitimate ones. The fanatic does not recognise – or even if he does, he chooses to ignore it - that reason permits many alternative ideals and value hierarchies.

But from what we have seen, these goal disagreements are irresolvable even with the resources of Laudan's reticulated model, in conclusion,

Without restrictions on appropriate aims for science, Laudan's account surrenders proper normative force; by acknowledging such restrictions it surrenders naturalism.

(Worrall, 1999, p. 354.)

Laudan's account surrenders a full bloodied naturalism because Laudan's pragmatic canons have an a priori character, and because Laudan is taking a non-descriptivist position when requiring his canons, thus for Laudan someone not following his canons would not, as a matter of evaluation, be acting in an authentic scientific way.

CONCLUSION

The counter-examples to Laudan's theory of scientific aims discussed in this chapter tried to show that Laudan's theory is incoherent –i.e., jointly incompatible- with value beliefs or goals that are intuitively right, and which form part of our background knowledge. Now, if it were asked that why should we take seriously these intuitive value beliefs, the answer would be that these intuitive beliefs are manifest to reason and that reason, like sight, is an intrinsic, fallible, but in general reliable – at least when dealing with our strongest shared intuitions - human faculty. Now, if the sceptic were to press and ask for a justification for the belief in the reliability of reason, we will end justifying this last belief by using directly or indirectly the same faculty in question, and the argument would be viciously circular. Alternatively, belief in the overall reliability of reason has, in the end, a dogmatic character.

On the other hand, Laudan's injunctions against 'semantic' and 'epistemic utopian' scientific aims would be unobjectionable if these injunctions were taken as weak desiderata, but not as strict requirements for rational scientific ends. On the other hand, Laudan's recommendation against 'demonstrably utopian' aims is just wrong,

because means-ends rationality at most proscribes impossible and not asymptotically approximable goals, that is, it does not proscribe as rational ideal goals.

In addition, Laudan could avoid relativism by either re-defining what to understand by relativism, or by openly welcoming some viciously circular justifications.

Alternatively, he could just resign himself and accept that his meta-methodology cannot avoid relativism, as he understands relativism.

Furthermore, Laudan needs a theory of scientific aims because he considers methodological rules as conditionals, with scientific aims as the antecedents of these conditionals. But if Laudan were to have another model of scientific methodological rules, then the need for a theory of scientific aims could be obviated; this would be welcomed given the complexity and difficulty of adjudicating scientific aims.

There is a further reason to abandon the conception of methodological rules as hypothetical imperatives, it is that at least some scientific methodological rules, such as ‘avoid ad hoc hypothesis,’ cannot be properly construed as empirical elliptical conditionals, thus,

[Laudan] suggests that this rule is really a conditional: ‘if you want to have risky theories, then avoid ad hoc hypothesis’. But the connection between the antecedent and consequent of this conditional is not of course empirical (or even partly empirical) it is purely conceptual – nothing in the way the world is could make an ad hoc theory risky, ‘ad hoc’ means tailored to already known evidence (and if the evidence is already known and the theory tailored to it, it follows of course that the theory is at no risk from that evidence).

Other plausible ‘cognitive virtues’ that spring to mind are empirical adequacy, simplicity and truth (though Laudan would dismiss the latter two as reasonable aims on the grounds that they are utopian). But what values of the variable X in ‘if you want a theory that is simple/true/empirically adequate prefer theories with X’ might make such a conditional plausible and at the same time dependent for its acceptability on which theories we accept about some substantive feature of the world?

(Worrall, 1999, p. 354.)

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