A REFUTATION OF PURE CONJECTURE

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SUMMARY. The present paper explores three interrelated topics in Popper's theory of science: (1) his view of conjecture, (2) the aim of science, and (3) his (never fully articulated) theory of meaning. Central to Popper's theory of science is the notion of conjecture. Popper writes as if scientists faced with a problem proceed to tackle it by conjecture, that is, by guesses uninformed by inferential considerations. This paper develops a contrast between guesses and educated guesses in an attempt to show that there is more to scientific conjecture than conjecture. The suggestion is made that some inductive considerations enter into the process of educated guessing or scientific conjecture in such a way that the 'context of discovery' cannot be sharply separated from the 'context of justification'. This discussion leads to a tension between Popper's negative method of conjecture and his realism. Given Popper's (implicit) theory of meaning it seems Popper's epistemology (the conjecture and refutation method) is incompatible with his metaphysical realism.

Key words: Popper, induction, refutation, grue, truth.

Past experience sometimes leads us astray and a lucky guess can bring us to truth. Few among us however doubt that past experience can be a trustworthy guide into the future or believe that truth is always best arrived at by lucky guessing. Nonetheless, both of these extremes have been defended. David Hume argued that one is never rationally justified in one's use of induction, reasoning based on past experience, and Karl Popper, accepting Hume's arguments, claimed that the rational growth of knowledge involves pure conjectures which are reached independently of any inference. Both of these views have had tremendous influence on our thinking concerning the nature of science. What I will argue is that, whatever one thinks about Hume's problem, Popper's idea that rational, objective knowledge involves pure conjecture is at the least misleading and at the most implausible. I will also argue that there is a more serious problem of induction which Popper should take more seriously. The only serious problem of induction comes back to haunt the Popperian deductivist. My positive proposal is to distinguish between 'pure' conjecture and 'educated' guessing or 'cal-

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culated' conjecture. I will maintain that any plausible normative construal of the methodology of science must recognize and explain the fact that the scientific discovery of an hypothesis usually involves educated guesses which involve reasoning amenable to rational standards. In this view the considerations involved in the educated guessing of an hypothesis are not entirely independent of the considerations of what will count as justification or evaluation. The logic of discovery should not be sharply separated from the logic of justification or evaluation. Moreover, I will show how acceptance of Popper's strict demands on the kind of justification involved in scientific method helps lead in a backhanded way to the current irrationalism in philosophy of science which Popper would certainly abhor. I will make the point by posing a dilemma for Popper's methodology which is reminiscent of one posed in the philosophy of mathematics by Paul Benacerraf. I will argue that Popper's strict deductivist methodology is in an important sense incompatible with his scientific realism. One way out is to give up scientific realism, another is to give up the conjecture and refutation methodology. In the end, I suggest that a plausible alternative to the method of pure conjecture and refutation can be found between the extremes of faithfully following past experience and of blindly guessing at the truth.

1. THE NEW RIDDLE OF INDUCTION AND THE LIMITS OF POPPER'S DEDUCTIVISM

Hume's problem of induction is succinctly stated as follows: an inductive rule of inference cannot be justified inductively because that method begs the question or leads to an infinite regress. An inductive rule cannot be justified deductively because deduction is not ampliative. Deduction does not allow an inference from past successes of induction to the future success of induction. Hume concludes induction is not rational; it is simply an acquired habit.

Since Hume's *Enquiry*, perhaps the boldest philosophical attempt to address this problem is made by Popper. He accepts the Humean dilemma; induction is not rationally defensible. Popper claims that every justification of induction 'leads either to an infinite regress, ot to the doctrine of **apriorism**'.[1] Nevertheless, he rejects the Humean conclusion. Induction is not an inevitable human habit. Philosophers do not need to justify this method, because science progresses adequately without it. Induction is simply a bad, nonrational habit that one would do well to break. Insofar as one is rational, one will abandon induction. Popper does not try to vindicate scientific method by justifying induction. On the contrary, he elucidates a scientific method which lacks induction altogether.

Popper's now familiar deductivism can be summarized as follows. *Science begins with problems. Problems give rise to conjectures.* The scientist invents some hypothesis, set of hypotheses, or theory. This conjecture does not involve an inference; it is guess. The outcome (an hypothesis or theory) may be the end of a complex causal or psychological process, but it does not involve an inference. He writes, 'there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process' (L.S.D., p. 32). The failure of previous philosophers to recognize the distinction between the psychological and the logical elements of science aggravated the problem of induction. In the logic of science, the process begins with a guess. Hypotheses are not inferred from observations. Once a conjecture is made, the scientist must test the theory empirically. What the scientist does is deduce observational consequences from his theory. These will be the predictions. If the observational consequences do not occur as the theory predicts, then the theory has been *falsified*, and it must be rejected. If the facts occur as predicted, then the scientist simply says that the theory has passed a test; it has been corroborated. This corroboration does not mean that the theory is probable, reasonable, or true, but it has simply passed a test and should be subjected to other tests. Corroboration also does not provide reasons for accepting the theory.[2] If the scientist corroborated a theory and from that inferred that it was true or reasonable, then he would be making an inductive inference. Popper claims that practicing scientists are concerned simply with falsifying theories by severe testing, and that they do not attempt to infer that a theory is reasonable or not. Scientific method from beginning to end involves only deductive inferences, and the logic of science is completely justified.

One cannot overemphasize the fact that Popper's confidence in his conception of scientific method rests solely on his trust in deduction. Something which should have worried Popper more are questions concerning the justification of deduction. Anyone who accepts Humean arguments against induction should fear that similar threats are posed for deduction.[3] Can deduction be justified deductively? Obviously not, since such a justification leads to an infinite regress as was so aptly demonstrated by Lewis Carroll's tortoise. But then, deductive inference can never be justified inductively because past experience can never establish the necessity required for deduction. So, accepting Humes's strategy, can't one conclude, using the same words Popper does concerning induction, that any justification of deduction 'leads either to an infinite regress, or the the doctrine of **apriorism**'? If so, then Popper's view of science is ultimately on no better epistemological footing than the view he is replacing. If not, then he at least owes us an explanation of why, in light of this Humean reasons against induction, deduction is safe and sound. Popper has devoted some attention to this problem. He tried to show that deductive logic is ultimately trival. Based upon such an explication of deduction, Popper or a Popperian could then simply admit that deductive rules are apriori justified without creating problems for the justification of deduction analogous to the problems of an apriori defense of induction. One way to show that the rules of deduction are ultimately trivial analytic truths – and as such acceptable candidates for apriori knowledge – is to explain how the truths of deductive logic depend solely on the meanings or a conceptual analysis of the logical constants, 'not', 'if...then', 'either...or'. This task turns out to be more difficult than it sounds, however. Tarski showed that if one presuppposes an account of some logical constants, then one can define the notion of logical consequence. But one's definition of logical consequence was relative to one's presuppositions about which terms are logical constants. Popper's idea was that this problem could be solved without any nontrivial assumptions, and so in this way deduction might be justified apriori. In retrospect Popper explains:

> My papers were...inspired by the hope of solving a problem which Tarski...indicates as *insoluble; rightly, I now suspect.* This was the problem of distinguishing between logical (or as I prefer to call them, 'formative') signs and descriptive signs...Tarski showed that the concept of logical consequence can easily be elucidated (with the help of the concept of truth of a 'model') once we have decided upon a list of logical or formative signs. My idea was very simple: I have suggested we take the concept 'logical consequence' as primitive and try to show that those signs are logical or formative which can be defined with the help of this primitive concept. It is only fair to say that *my papers did not succeed in this* (as emerges from Lejewski's analysis).[4] (emphasis added)

Popper's primitive was actually the relation of 'being deducible from', which supposedly he takes to be the same as the technical notion of logical consequence. Taking the meaning of the deducibility relation to be clear and trivial, he then attempts to define the logical constants without any presuppositions. What Lejewski shows is that Popper must presuppose exactly those notions he does not want to presuppose. Lejewski concludes, 'The upshot seems to be that Popper's claim to have constructed a logic without assumptions or a logic without axioms; i.e. a logic based on definitions alone, can hardly be upheld'.[5] Given this failure, one might think that Popper could still fall back on Tarski's account of logical consequence as an analysis which justifies the apriori defense of deduction. Tarski rigorously shows that the logical truths are analytic and thus apriori justified. That Tarski's 'definition' does this or is capable of doing this is far from obvious. As John Etchemendy argues

> ...his [Tarski's] account of logical truth and logical consequence does not capture, or even come close to capturing, any pretheoretic conception of the logical properties...Applying the model-theoretic account of consequence, I claim, is no more reliable a technique for ferreting out the genuinely valid arguments of a language than is applying a purely syntactic definition. Neither technique is guaranteed to yield an extensionally correct specification of a language's consequence relation.[6]

Popper himself seems to admit that such a project for justifying deduction is impossible. He writes, in an uncharacteristically Quinean moment,

> I should now make plain that I now think that Tarki's skepticism concerning a clear demarcation between logical and descriptive signs is well founded. Demarcations are needed; but they are usually not sharp. This seems always to be so, interestingly enough; and perhaps it is not to be regretted.[7]

Certainly it is not to be regretted, unless one wants an account of deductive logic which justifies deductive rules apriori via appeal to analytic truths concerning the logical constants. But if one cannot explain why deductive rules are apriori justified in some way that distinguishes this move from an apriori defense of inductive rules, such as Carnap's, then it seems Popper's worry about the justification of induction arises with equal force against the justification of deduction – it either leads to an infinite regress or apriorism.

Of course, there are *real* problems concerning induction, like that posed by Goodman's riddle, but Popper's worries are the Humean concerns over the justification of an inductive rule of inference. The 'old riddle of induction' concerns the rational justification of a rule of inference such as:

From: All A's examined under a variety of conditions have been B's.

Infer: The next A examined will be a B.

The 'new riddle of induction' concerns what will count as the relevant A's and B's in our use of the rule. The world seems to contain an overabundance of uniformities which can be labeled and so used as substitution instances for the A's or B's in the above inductive rule. Is green the real property of emeralds or is it grue? Goodman's problem is not with any inductive rule, but with ability to discover the uniformities in the world which make for suitable substitution instances of our ordinary inductive rules, which, for Goodman, are no less justified than our ordinary deductive rules. It seems that Goodman's skepticism concerning the determination of the real 'uniformities' or properties which are 'out there' in the world should bother a deductivist as much as an inductivist. The deductivist will want the hypothesis 'All emeralds are green' to make predictions and provide explanations in terms of real uniformities no less than any inductivist.

In defense of Popper, David Miller claims that 'The answer to this unrealistic and quite silly question is given by a simple refinement to Popper's criterion of demarcation'.[8] A statement is scientific only if there are tests which could in principle eliminate it. By extension, two conflicting statements will not 'be admitted' to science unless there are possible tests to eliminate at least one of them. Miller says, 'If we are not prepared to delay until 2001 a decision between 'All emeralds are green' and 'All emeralds are grue', therefore, we must not admit them both to science'.[9] Since the two statements are empirically indistinguishable which one shall be admitted as a scientific hypothesis? The Popperian cannot appeal to past experience for two reasons. First, both statements are equally well corroborated by past experience; and second, any justification of one statement as better in terms of 'truth' or 'reliability' than the other based on past experience will involve induction. Can a Popperian provide a nonarbitrary, noninductivist account distinguishing 'All emeralds are green' as scientific from 'All emeralds are grue' as unscientific? Miller believes the answer in simple:

> Although our background knowledge has evolved somewhat haphazardly, it is to a considerable extent the product of a long process of conjectures and refutations (many of them operating at a biological and preconscious level). And there is nothing arbitrary about a refutation. Things might have happened differently, to be sure. But as it happens, they did not. And had they happened differently, they would not have happened the way they did happen. There are many routes to the truth: we tread but one of them.[10]

This answer is not exactly clear, but Miller's position seems to be the following. Human beings are creatures that have evolved in such a way as to treat green instead of grue as a real property, natural kind, or uniformity out there in the world. Moreover, this evolutionary process was checked by the conjecture and refutation methodology (however unconsciously operating). Therefore, the acceptance of 'All emeralds are green' as scientific and the rejection of 'All emeralds are grue' as unscientific is not arbitrary at all.

This evolutionary, Popperian answer is, however, no answer at all. First, Miller's insensitivity to the difficulty of the grue paradox is obvious when he says, 'There are many routes to the truth: we tread but one of them'. The force of the grue paradox is not that 'All emeralds are green' and 'All emeralds are grue' are empirically indistinguishable, so we can choose one of them and tread our way to the truth. On the contrary, both statements are empirically indistinguishable at the present time. Given the reasonable assumption that there will be unobserved emeralds after 2000 A.D., they are contraries; so at most one can be true. How do we 'admit' the true one while eliminating the false one? There seems to be no empirical way of justifying any such 'admittance'. Second, the appeal to evolution and refutation seems as useless here as an appeal to induction. Suppose humans had evolved differently projecting grue as a natural kind or uniformity of the real world. The statement 'All emeralds are grue' would have passed the test of natural selection no less than 'All emeralds are green'. In regard to all past and current evidence the statements are indistinguishable; therefore, they will be indistinguishable from the view of natural selection. Natural selection favors both statements equally up till now, but only one can be true. Many routes may lead to the truth, but only one of these roads will, the other is a dead end. The conjecture of evolution and the refutation of natural selection provides no road map here. It leads us up to a fork in the road and there the grue paradox rears its ugly, 'silly' head.[11]

One comment Miller makes about the grue problem is especially bothersome. He says of the statement 'All emeralds are grue:'

> ...it solves no problem that is not equally well solved by 'All emeralds are green'. Thus one at least of the two hypotheses is redundant, and may be excluded from consideration. It is perfectly clear which one that is.[12]

He appears completely insensitive to the problem. One could as easily say that 'All emeralds are green' solves no problem that is not equally solved by 'All emeralds are grue'. An alien who speaks grue-bleen talk might well agree with Miller that it is perfectly clear which one is to be excluded. The trouble is that, although they can both be wrong, Miller and the alien cannot both be right. To treat such a problem as no real problem at all should be philosophically unacceptable to a Popperian. Why? Consider an earlier comment by Miller (attributed to Bartley) that gemologists have never considered the statement 'All emeralds are grue' because it solves no problems. The suggestion seems to be that the grue paradox is not to be taken seriously because gemologists would never think of projecting 'grue'. For a pragmatist, like Quine or Wittgenstein, this move seems reasonable. But how can a Poperian suggest this as a good reason for ignoring the grue paradox. Could one not turn this very stategy against Popper and Miller? Most practicing physicists, biogists, geologists, and even gemologists do not seriously consider that induction is in principle problematic. Should one then conclude that the problem of induction is just a 'silly' philosophers problem that does not in any way engage the scientists and their practice? Although Wittgenstein or Quine will answer with an unqualified 'yes', the Popperian should be appalled by such a response.

A Popperian who takes the grue paradox more seriously is John Watkins. In his *Science and Scepticism*, he elaborates an account of theories which is such that 'grue variant' of real theories are not themselves real theories and so are not as well corroborated as their real counterparts. So, Watkins' account represents a straightforward Popperian *solution* to the grue paradox. According to Watkins, any genuine scientific theory must meet what he calls the 'organic fertility requirement'.[13] This notion is defined in terms of the notion of the testable content of a theory T, or CT(T), which is the set of T's singular predictive implications. Consider any theory T to be a set of axioms greater than one, and consider T' and T'' to be subsets of any mutually exclusive partition of T such that $T' \& T'' = T$ while $T' \neq T \neq T''$. The organic fertility requirement says that T is a genuine theory if the testable content of T is greater than the union of the testable content of T' with the testable content of T'',

$$CT(T) > CT(T') \cup CT(T'') \text{ and never } CT(T) = CT(T') \cup CT(T'').$$

With these definitions Watkins then proceeds to demonstrate straightforwardly that 'grue variants' of genuine theories are not themselves genuine theories.[14] Let the theory T be presented by

$$\forall x(Fx \rightarrow Gx).$$

Construct the 'grue variant' T^g of T as

$$\forall x \forall t((t \leq t_0) \rightarrow ((F(x,t) \rightarrow G(x,t))) \ \&$$

$$\forall x \forall t((t > t_0) \rightarrow ((F(x,t) \rightarrow G'(x,t))),$$

where t^0 is some point of time in the future, say the year 2000, and G and G' are incompatible. Consider the partition of T^g in which Tg' is the first conjunct in the above formula and $T^{g''}$ is the second conjunct. Because $T^{g'}$ is restricted to the parameter $t \leq t_0$ and $T^{g''}$ is restricted to the parameter $t > t_0$, the conjunction of $T^{g'}$ and Tg'' will yield no testable consequences not already implied by $T^{g'}$ or $T^{g''}$. So,

$$CT(T^g) = CT(T^{g'}) \cup CT(T^{g'}),$$

and the 'grue variant' T^g fails the organic fertility requirements and is not a genuine scientific theory. Watkins' intuition is that 'a genuine scientific theory makes possible predictions across a span of time: it will have singular predictive implications of the form $e_1 \rightarrow e_2$, where e_1 describes initial conditions obtaining at one time, and e_2 describes an occurrence at a later time'.[15] T^g does not have such singular predictive implications. So, 'grue variants' of real theories are never as well corroborated as their real theory counterparts. Therefore, the grue paradox is not a worry for the Popperian.

Although one might wonder why a genuine theory must meet the organic fertility requirement and also whether Watkins-style grue variants represent Goodman's version of grue predicates, one should consider a more fundamental worry. If certain predicates are taken as primitive in a particular theory, then one can, using Watkins' strategy, 'prove' that certain variants of this theory, which are commonsensically real theories, are not genuine theories. For example, consider again the aliens who speak grue-bleen talk. 'Grue' and 'bleen' are primitive predicates in their theory of the world. In this case, part of their theory can be represented, along the lines of Watkins, by T

$$\forall x(Fx \rightarrow Gx),$$

where 'Fx' is the open sentence 'x is an emerald' and 'Gx' is the open sentence 'x is grue'. From the point of view of the aliens who speak grue-bleen talk, 'green' and 'blue' will be deviant predicates defined as follows:

> x is *green* if and only if x is observed before t and is grue or x is unobserved before t and bleen;
>
> and
>
> x is *blue* if and only if x is observed before t and is bleen or x is unobserved before t and is grue.

From the point of view of their theory T, the aliens can now provide a Watkins' style argument to the conclusion that 'green variants' of T are not real theories. The 'green variant' of T is simply the conjunction of

$$T': \forall x \forall t((t \leq t_0) \rightarrow ((F(x,t) \rightarrow G(x,t))) \text{ and}$$

$$T'': \forall x \forall t((t > t_0) \rightarrow ((F(x,t) \rightarrow G'(x,t))),$$

where G and G' are the incompatible predicates 'x is observed before t and is grue' and 'x is unobserved before t and is bleen'. Now, the grue theory, T, will be a real theory, but the conjunction of T' and T'' is not because it does not meet the organic fertility requirement since T' is restricted to the parameter $t \leq t_0$ and T'' is restricted to the parameter $t > t_0$. So, the 'green variant' is not a genuine theory. Yet, paradoxically it should be as well corroborated as the grue theory T since all (T' & T'') says is that all emeralds are green! Thus, on Watkins' account is seems that what is to count as a 'genuine theory' is relative to what predicates one takes as primitive, or as Goodman would say 'entrenched'. But, in that case, Watkins has not provided a nonnaturalistic, nonpsychologistic Popperian solution to the grue paradox. In spite of all its technicality, Watkins' discussion simply provides a way of elucidating the force of the paradox. Watkins' discussion simply takes one back to the question which troubles Miller's Popperian account: how can a Popperian consistently provide a solution to the grue paradox or the 'real' problem of induction that does not depend on a naturalistic, psychologistic claim about what practicing scientists do – about what predicates they happen to take as primitive?

So, there is a real problem with induction – the grue paradox – but it is a problem which infects the serious deductivist method no less than inductivism. The only philosophy that can legitimately ignore it is some version of naturalism or one that abandons altogether the search for a methodology of science. Such a possibility does not appear to be an alternative for Popper, who has tried, perhaps harder than anyone, to elucidate and defend a methodology of all empirical sciences.

2. PURE CONJECTURE VS. CALCULATED GUESSING

Has Popper adequately described the logic of scientific method? Is science void of induction? Is the scientist not interested in anything but falsification and corroboration? Such questions arise at every step in Popper's deductivism. One should ask whether Popper's account of hypothesis formation is totally correct. Surely, there is an element of guess involved in the formation of theories, but does it follow that no inferences occur? Popper claims that when one observes science one will see that the step of conjecture is best understood as purely psychological. But one could grant what Popper claims about the 'psychology' of hypothesis formation and deny that no inference was involved. Peter Achinstein delineates four claims involved in the denial of inferences to laws: (a) there is no mechanical way to infer laws from data; (b) hypotheses are arrived at by imagination; (c) a law is not inferable from data alone; and (d) formulation of a hypothesis is part of a causal process which involves not only observations and theories but also the scientist's personality.[16] Perhaps these four things are true of hypothesis formulation, but none of these imply that no inference occurs in conjectural processes. Achinstein underscores this gap by responding with four corresponding counterclaims: (a′) Hypotheses may not be inferred from data by mechanical reasoning. Certainly, scientists are not conscious of step-by-step inferences which they make according to certain rules. Much less could they cite the rules and the steps they follow. Nevertheless, they could be making inferences in hypothesis formulation. (b′) Hypothesis formulation takes imagination, but, after all, many inferences require imagination and creativity. (c′) An hypothesis is not inferable from observation alone, but this does not hinder the possibility of inferences to hypotheses from the conjunction of observation statements and certain background information. (d′) A scientist's personality may affect hypothesis formulation; nevertheless, an inference could have been made. If, in fact, an inference was made, the scientist made it partly because of certain features of her personality. There is a causal-psychological explanation of how the hypothesis came about, but that need not be whole story. An inference could be involved. Inference and conjecture are not necessarily incompatible.

Popper might respond to all these points in the following manner: perhaps, inference and conjecture are not incompatible. I (Popper) believe that they are, but I will grant the point for the sake of argument. The deductivist's *normative* claim is that when one is concerned with evaluating the rationality of a scientific theory or scientific progress one *should* understand the history of science as one in which the best scientists guess bold hypotheses and try to refute them with crucial test. The guess is best understood as a causal process that need not involve an inference. Popper is not making a straightforwardly empirical claim about actual scientific practice but a claim about how science should be viewed as a rational endeavor.[17] Whether or not actual practicing scientists could ever vindicate such a general claim as Popper makes there is clear evidence that some eminent scientists agree with Popper. Consider the following passage from Richard Feynman's *The Character of Physical Law*:

> In general we look for a new law by the following process. First we guess it. Then we compute the consequences of the guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation with nature, with experiment or experience, compare it directly with observation, to see if it works. If it disagrees with experiment it is wrong. In that simple statement is the key to science. It does not make any difference how beautiful your guess is. It does not make any difference how smart you are, who made the guess, or what his name is – if it disagrees with experiment it is wrong. That is all there is to it.[18]

Feynman's statements on methodology are straightforwardly Popperian. Laws are the outcome of guesses. The process of guessing is a psychological one which can be described in various ways – beautiful, smart, or made by a 'smart' person – but none of these factors is relevant or necessary for the outcome to be 'scientific'. All that matters is falsifiability or testability.

Whether Popper's claims about the methodology of the practicing scientist are accurate and adequate for describing the rationality of science will be considered in more detail later after a simple point concerning the relationship between guessing and inference has been made. Here, an example will be used in an attempt to elucidate certain intuitions about the use of the word 'guess'. Perhaps a distinction can be pointed out between the ordinary use of 'guess' and the use when one says that a scientist guessed. Suppose a boy has a bad case of ammesia. He knows only very basic facts. In his hospital room, a doctor asks him to guess how many people are in the next room. At first the boy is hesitant, but then the doctor offers him a surprise if he can guess correctly. The boy then guesses by simply picking a random number. Now consider an average man on the street. He is asked to compete with Feynman in a guessing contest about quantum physics. Whoever guesses the best theory wins. When the man is asked to guess, he will reply, 'I can't. I don't know the first thing about physics. I just can't'. This example shows that a scientific guess will be an educated guess.

Popper is perfectly content with this fact. But there seems to be quite a difference between saying that the amnesia victim guessed and that Feynman guessed. To guess like the boy takes little talent and no inference – it is just a mental leap, a random pick of a number. This sense of 'guess' is ordinary. To guess like Feynman takes much talent and much education. The educated guess seems different from the boy's guess, because the educated guess is *calculated* in a sense in which the boy's guess is not. In fact, an educated guess is often called a 'calculated guess'. Calculations usually involve inferences. Perhaps intuitions like these seem to make the educated guesses of the scientist more than 'mere guesses' or 'pure conjectures'. Popper, of course, would say that educated guesses do involve more than mere guesses, but that is to be explained psychologically or sociologically, not logically. Feynman's education *caused* him to make certain guesses, but this does not mean he inferred his conjectures – nevertheless, ordinary intuitions balk here. Of course, certain information will causally affect certain guesses; but, in some cases, that will only be because that information promotes the inference involved in the guess. None of this settles the issue, but perhaps it has been pointed out how Popper's notion of guessing theories without inferences is somewhat counterintuitive. This issue is important because if educated guesses sometimes do involve inferences, then science may again be faced with Hume's problem. The detailed discussion of 'educated' guessing will be postponed until section 3. For now it is sufficient to point out that there seems to be a distinction in ordinary language between random guessing and the kinds of guessing scientists make.

Popper would probably be unmoved by such a point. First, he might suggest that he can handle the distinction between mere guesses and calculated guesses within his methodology.[19] Emphasized in the section 1 sketch of Popper's methodology was the fact that science begins with problems and proceeds to search for solutions to those problems. A conjecture in science is not a blind guess but one made in the context of a certain problem situation. A conjecture is thus not a blind guess but guess calculated to solve a specific problem. So, one might suggest that Popper's conjectures are 'educated' or 'calculated'. While this response seems correct as far as it goes, it is not obvious that it goes far enough. Although Popperian conjectures might be in a sense 'calculated' or 'educated' because a scientist proposes them in light of a problem, there is still a strong sense in which they are still mere guesses like the boy's in the above example. Popper insists that they are not guided by nor are they the outcome of inference, and so in this sense they are not 'calculated' or 'educated'. The boy in the above example does after all have a specific problem to solve, a problem with practical consequences. The fact that he is to make a conjecture in light of the problem does nothing to help him make anything but an arbitrary guess. Popperian conjectures seem to be in no better position even though they are posed as solutions to problems. Of course, this is not intended to solve the issue; it simply gives a new way to pursue the question about Popper's methodology: is this weak Popperian sense of 'educated' or 'calculated' conjecture as one posed in light of a specific problem adequate for an account of scientific rationality?

Second, Popper might (*very* reluctantly) agree that inductive or nondeductive inferences take place when scientists guess hypotheses, but point out that all this talk of guessing and inference involves issues in the 'context of discovery'. What is important to Popper's deductivism is that no inductive or nondeductive inferences take place in the context of justification or evaluation. That is, although a practicing scientist might use an inductive methodology to arrive at his hypothesis or laws, none of this can play any role in justifying or evaluating a scientist's claims or hypotheses. All that is important for justification or evaluation is the *testing* of such claims or hypotheses and only *deduction* will be necessary for that. Hume's problem does not arise in that context. I want to suggest, however, that the context of discovery is not clearly separable from the context of justification. Recognizing their connection will put this issue into a new light.

3. HYPOTHESIS AND INFERENCE

For the present, suppose that Popper has pointed out a very important part of science, that is, severely testing theories in an attempt to refute them. The possibility of inductive inferences still remains. After a theory has passed many severe tests, doesn't the scientist then accept the theory? According to Popper, when a theory has been well tested and not falsified (corroborated) no inference should be made to accept the theory as true, probable, reasonable, or reliable. Such an inference would be illegitimate because it is inductive. Scientists do not accept theories; they either refute or corroborate them. In the end, science is purely critical.[20]

Is this Popperian piccture of science an adequate one? Can this methodology alone explain the rational aspects of science and scientific progress? Perhaps Popper has solved the problem of induction only by ignoring a large part of the scientific endeavor. For example, much of the scientific work that goes on once a theory has been corroborated is the search for explanations.[21] Any one proposing the conjecture and refutation methodology should consider three points concerning the relationship between explanation and theory acceptance. First, in such cases where a theory has passed a large number of severe tests and where there is no likely competing theory, scientists tend to *accept* the theory. For example, until Einstein, Newtonian mechanics was the *accepted* theory in a very real sense. Of course, Popper might claim that such acceptance reflects an unreasonable attitude on the part of those scientists and that reasonable or 'the best' scientists would not have such an attitude. A second point should show why this line is difficult to accept.

Second, a theory can be accepted in a 'very real sense' as follows. After Newtonian mechanics had passed its initial tests, scientists no longer attempted to falsify the theory because: (A) it explained an unbelievably large range of complex physical phenomenon; and (B) there was no competing alternative. At this point, observations which seemed odd given the theory or which were not predicted by the theory *were not* counted as falsifications of the theory. The theory was accepted as well established. For example, observations about the orbit of Mercury were odd given Newtonian theory, but such observations were by no means a falsification of the theory. Scientists thought that such anomalies could be explained within Newtonian theory; they never thought of rejecting the theory because of these anomalies. Not until Einstein's theory did such observations make of difference in evaluating Newtonian theory. Again, Popper might claim that such an attitude of acceptance in light of falsifying instances is unreasonable. The fact that Newtonian theory provided explanations for a large range of physical phenomena gives no reason to accept it. Astrology also offers explanations of a large range of phenomena, but this is no reason to accept astrology given the anomalies of falsifying instances of such theories. But there is a difference. What makes astrology unreasonable is that there are better competing explanations of the phenomena which are the subject matter of astrology. For example, the disciplines of biology, psychology, and meteorology provide much better accounts of many of the phenomena astrology supposedly predicts and explains. In such circumstances the astrologers' attitudes to the falsifying instances of their theory is all important. Astrology lacks empirical content either because it excludes nothing empirical or conventionalist stategies protect it from falsification. Even in light of competing explanations, the astrologer is not concerned with the anomalies. The attitudes of the Newtonian theorists were different. They maintained their attitude of acceptance in the face of anomalies because there were at the time no competing explanations. Clearly, their attitude is reasonable and scientific in a way the astrologers' is not.[22] If this is so, then the reasonableness or unreasonableness of maintaining a theory cannot be determined entirely by whether the theory has falsifying instances or not. In his paper 'The Rationality of Scientific Revolutions', Popper concedes as much when he writes:

> A limited amount of dogmatism is necessary for progress: without a serious struggle for survival in which the old theories are tenaciously defended, none of the competing theories can show their mettle; that is, their explanatory power and their content. Intolerant dogmatism, however, is one of the main obstacles to science. Indeed, we should not only keep alternative theories alive by discussing them, but we should systematically look for new alternatives; and we should worry whenever there are no alternatives – whenever a dominant theory becomes too exlusive. The danger to progress in science is much increased if the theory in question obtains something like a monopoly.[23]

The Newtonians are not unreasonable because they maintained their explanations in light of anomalies but are unreasonable because they did not search carefully enough for alternative explanations of the anomalies which were falsifying instances of their theoretical explanations. When one will make *ad hoc* adjustments to one's theory come what may in order to save the theory then one is intolerably dogmatic and fostering an unscientific monopoly. Many times what is relevant to the rationality or reasonableness of a theory is the theorist's *attitude* toward certain anomalies given that there are good competing explanations of the phenomena of which he is concerned.[24] If, however, even this much is true, then it seems that questions of methodology alone are never all that is relevant for considering the reasonable/unreasonable or scientific/unscientific status of a theory. Therefore, Popper's move to considering the scientist's attitude toward falsifying instances of a theory should lead him to some uncomfortable conclusions concerning the importance of his strict conjecture and refutation methodology. The methodology has become a kind of psychologism.

Third, and most importantly, a serious tension arises when one considers what Popper considers the 'aim of science' and his deductivist methodology. Popper claims straightforwardly that 'the aim of science is to provide *satisfactory* explanations.[25] Because scientists are interested in explanations at a certain stage in the life of a theory and not just in falsification it seems science sometimes involves induction. When certain odd observations would count as falsifying a theory and when explanations for these observations are sought within that theoretical framework, the scientist has accepted the theory on the basis of its passing severe experimental tests. In other words, the scientist makes an inductive inference. To deny this fact or to argue that Newtonian scientists were wrong in making *ad hoc* adjustments is to ignore a very important part of science. What sense does it make to accuse Newtonians of *ad hocness*? They were operating with the only theory they had. Their adjustments were *ad hoc* only in light of Einstein's later discoveries.[26] At the time, they were making the best guesses conceivable to them. Also, such procedures were scientifically fruitful. To use a hackneyed example, given Newtonian mechanics and the background assumptions of 1846, the orbit of Uranus was not correctly predicted by the theory. Instead of rejecting Newtonian mechanics as falsified, scientists predicted the existence of another planet besides the seven known at the time. The result was the discovery of Neptune. Had the scientists been intent on falsifying Newton's theory, the discovery of Neptune would not have occurred until much later. Here, it seems, is a perfectly clear instance where scientists were right in accepting a theory based on its past performances. Notice that the Newtonian's 'limited amount of dogmatism' in this case was not justified on the Popperian grounds that it forced competing theory to 'prove its mettle'. There was no competing theory. They were right because, given the past successes or corroboration of the theory, it was only reasonable to assume an explanation would be had for this anomaly within that framework. Also, there seems to be another kind of inference made in this case. Having 'accepted' the theory based on its past successes or corroboration, the scientists, when confronted with the deviant orbit of Uranus, inferred the hypothesis about the existence of Neptune in the following fashion. They reasoned that, given Newtonian mechanics and the relevant background information, the hypothesis postulating the existence of another planet would *explain* Uranus' deviant orbit if true. They then concluded that the hypothesis was worthy of consideration or testing. Notice how falsifiability is increased by the new auxiliary hypothesis. Such nondeductive reasoning is usually called 'abduction' to distinguish it from induction and such inferences do take place when hypotheses are proposed.[27] The conclusion of the nondeductive inference is not the content of the hypothesis but the claim that the hypothesis is worth testing.

One should not be surprised that such inferences do occur in scientific reasoning if the aim of science is to provide adequate explanations: the form of inference tells us that an hypothesis is worth testing since given what we know it would be a good explanation of what we want explained if it were true.[28] One should now be able to understand why the scientists' conjecture of an hypothesis is no mere guess, why guessing like Feynman involves more than guessing like the amnesiac boy in my earlier example. It may take a great deal of 'intuition' to come up with the *content* of an hypothesis; that is, a meaningful sentence with a particular interpretation. Arriving at hypotheses with rich, bold content takes imagination and creativity. One can define the *semantic content* of an hypothesis as the truth conditions of its statement and understand the *boldness of an hypothesis* (the measure of content) in Popperian terms as the class of its potential falsifiers, which will be a subclass of basic statements.[29] But *merely* creatively arriving at a statement with rich *semantic* content does not make that statement into an hypothesis. In order for one with great scientific imagination to arrive at an *hypothesis* one must not only propose a bold empirical statement but one must also recognize that *if the statement were true it would explain what one wants explained and if it were true it would provide some nontrivial support for the theory in relation to which it explains what one wants explained.* It is in this recognition that inference is involved that makes hypothesis formulation no mere guess or conjecture but a 'calculated guess'. The deductivist misses this point and wants to talk of pure conjecture of hypotheses – conjecture devoid of inference – only because he fails to distinguish between the content of the hypothesis, which takes imagination and creativity to discover, from the role of the statement that makes it an hypothesis, which takes inference to recognize.[30] That there was a planet beyond Uranus may have been a wild, bold imaginative idea in the mind of Leverrier, but for it to have become a testable hypothesis worthy of testing he had to realize that given Newtonian theory, if his idea was right, he could explain the deviance of Uranus and the correctness of this explanation would in turn add further credence to Newtonian theory.

One might wonder how representative this example is of scientific inference. Although it is impossible in the course of this paper to discuss a list of examples, one more example which is significantly different from the discovery of Neptune should help illustrate that it is plausible that the strategy extends to other cases in the history of science. The hypothesis that there was a planet beyond Uranus which caused Uranus' deviant orbit was proposed in the context of well-established Newtonian theory. One could say that the discovery of Neptune was a piece of 'normal' science. The nondeductive strategy I proposed above can also be illustrated by scientific discoveries that are considered revolutionary. For example, Wegener's hypothesis of continental drift. Wegener's hypothesis was a bold one, but it did not come from nowhere. It was posed in an inherited theoretical context in which the movements of the earth's crust were explained by the principle of isostasy: that all the elements of the system are in hydrodynamic equilibrium. The principle together with the background information about the shapes of the continents, the fossil record, and statistical analysis of the earth's topography made Wegener's hypothesis better than the competing one. But in this context Wegener's hypothesis was still bold in the sense to be explained below. Wegener's idea of continental drift may have been wild, bold, and imaginative, but for it to have become an hypothesis worthy of consideration he had to realize that given the principle of isostasy, if his hypothesis were right, he would not only explain the shape of the continents and the fossil record but would also add further credence to the principle of isostasy that was at the base of his geological theory.

The notion of calculated guessing can be nicely illustrated by Bayesian analysis. Bayesian strategy has long been at the forefront of discussions of confirmation theory and that such Bayesian theories of confirmation are compatible with Popperian methodology has been pointed out by both Wesley Salmon and Mary Hesse.[31] Just how such theories of confirmation illuminate and connect with a theory of hypothesis formulation, however, has either been ignored or misunderstood. The hypothesis that there is a planet beyond Uranus, whose dimensions and position can be fairly accurately specified, is put forth as an hypothesis only in the context of a theory and certain other observational information, such as the deviant orbit of Uranus. Only in this context does a statement with a specific *semantic* content become an *hypothesis*. Bayesian strategy illustrates what kind of reasoning must go on in fullblown hypothesis formulation. Instead of simply dreaming up random statements with bold *semantic* content, one must 'calculate' one's conjectures so that when one reaches statements with bold content such statements can play a certain role in the context of the theory doing the explaining.[32] The semantic content of the statement alone may insure that the statement has a low probability. Given the understanding of the truth conditions for the statement alone, one may judge that it is highly unlikely that the world is such that the statement could ever be true. That is, the initial subjective probability for the statement might be very low. Therefore, holding such a statement would be very bold in a psychological sense. If all one wanted to do is to make bold conjectures in the psychological sense, then one has all the reason one needs to put forth the statement as an hypothesis worthy of testing. But this is not only impractical advice for formulating hypothesis, it is also implausible as an account of how scientists really work or ought to work.

Although the initial probability of an hypothesis might be very low based upon the understanding of the truth conditions of its statement, the probability of the hypothesis given the theory and other relevant information might be considerably higher. For example, the initial subjective probability in 1846 of the statement concerning a specific planet beyond Uranus was surely quite low. That there was just such and such a specific planet in such and such a specific location beyond Uranus must have been incredibly implausible independent of Newtonian theory and specific information about the orbit of Uranus. That in itself makes such a statement bold in the *psychological* sense. How could anyone believe it? One who puts forward the statement, however, does so already within the context of Newtonian theory and knowledge of the relevant information concerning Uranus. For the scientist who puts forward the statement concerning the new planet as an *hypothesis*, the probability of the hypothesis given the theory and the other relevant information will be quite high. If the theory plus the relevant information entails the hypothesis, then the hypothesis must be as probable as the theory itself given the relevant information. So, with this understanding, which already takes some sophisticated reasoning, the hypothesis will not be bold in the *psychological* sense. Given the scientists' knowledge of Newtonian theory and the relevant information, one will no longer ask how he could believe such an hypothesis.

Nevertheless, there is still a sense in which the hypothesis can be considered bold (and this is the sense discussed by the Bayesians). Since the initial probability of the hypothesis independent of the theory and other relevant information is low, but high given the theory and other relevant information, any confirmation of the hypothesis will count as important confirmation of the theory itself. Such an hypothesis is bold in a *logical* sense: Any confirmation of the hypothesis will count greatly in the confirmation of the theory itself. The hypothesis can play this role because, as application of the Bayesian rule shows, the initial probability of the statement of the hypothesis independent of the theory is low while it is high given the theory.

Popper's discussion of 'bold' conjectures seems to conflate these two senses. In fact, most of the discussion of the logic of discovery seems to conflate the psychological with the logical sense of boldness. Hanson and Salmon both claim that there is reasoning involved in the logic of discovery which takes the form of 'plausibility' considerations. But both seem to have in mind only the considerations of plausibility which allow one to claim that a conjecture of a scientist was not bold in the *psychological* sense. That is, given the scientists' theory and other information, the probability of the hypothesis was not low. So, in a *psychological* sense the hypothesis was not bold. The logic of discovery for both Hanson and Salmon is concerned only with considerations of the plausibility of the hypothesis in this sense: the scientist must have reasons for believing that do not make the belief bold in the psychological sense. These considerations, however, cannot be all there is to the logic of discovery because the logic of discovery would thus make it sound as if reasonable scientists put forward hypotheses simply because they would explain, given the theory, what they want explained. Scientists also put forth hypotheses recognizing that if they are true, they will help support the theory itself. That is, when formulating an hypothesis they not only consider whether the hypothesis is plausible, that it is not bold in the psychological sense, but they also consider whether it can play a certain role in helping confirm the theory, that is, they consider whether it is bold in the logical sense.

That the two considerations go hand in hand in formulating an hypothesis should now be obvious. If one strikes upon an idea whose *semantic* content alone makes its initial probability low, then one needs some reasons to believe it is plausible so that it is not bold to believe it in the psychological sense. The reasoning will involve seeing that given the theory and other relevant information the hypothesis is plausible. But once this is done, that is, once one has alleviated boldness in the psychological sense, one cannot help but realize boldness in the logical sense. The statement initially improbable is made plausible by the theory and other relevant information, so if the statement is true it will play a significant role in confirming the theory of which it is part. When one recognizes that a statement has these qualities, then one is putting forward an hypothesis in the sense of an 'educated guess' or a 'calculated conjecture'. All these considerations are involved in the logic of discovery in the sense in which the logic of discovery is concerned with the kind of reasoning that makes a scientist conclude that a statement is *worthy of testing*, that is abductive reasoning. The question is whether the conclusion of an abductive argument to the effect that a claim that a particular hypothesis is worthy of testing involves inductive considerations.

4. A DILEMMA FOR PURE DEDUCTIVISM

If a 'creeping inductivism' is avoided at all costs however, then a tension underlying the method of falsificationism surfaces. The job of the scientist is predominantly critical – invent bold claims and try your best to refute them. If this idea captures the rationality of the scientific endeavor, then how can this negative methodology ever realize its positive aim of providing understanding of reality by providing satisfactory explanations? The problem is made insurmountable by the way Popper understands explanation. Popper provides two criteria for a satisfactory explanation:

> First, it must logically entail the *explicandum*. Secondly, the *explicans* ought to be true, although it will not, in general, be known to be true; must not be known to be false even after the most critical examination.[33]

That a satisfactory explanation, if it is to provide real understanding of reality, must be true, is not at all unreasonable. How can a false explanation provide understanding? But the knowledge of such explanations is permanently beyond the reach of the critical methodology of conjecture and refutation: all the method of science can establish is that certain claims have not proven false. Simply because an hypothesis has yet to be falsified, even after the most critical examination (how can one tell when the time of 'most critical examination' has passed?), does not mean it provides a satisfactory explanation of reality. The method of science will always fall short of its goal. If the adequacy of a method is at least partially a function of how well it fulfills its purpose, then one cannot fail to conclude that what the Popperian proposes as the best normative guide for understanding the rationality of science is one that will doom that very rational endeavor to failure. What can one say of a methodology that ensures such a gulf between the ends and means of the scientific 'enterprise?'

In defense of Popper, Miller has an easy answer to these questions. According to Miller, underlying these questions is a fundamental philosophical blunder. What these questions fail to distinguish is the difference between *attaining truth* and *knowing that one attains the truth*. The conjecture and refutation methodology can never *justify* the belief that truth has been attained but that does not mean it will not lead one to the truth.[34] The goal of science, according to Miller, 'is to separate as thoroughly as it can the true statements about the world from those that are false, and to retain the truths'.[35] The method of conjecture and refutation is supposedly all that is needed to reach this goal, although it will never give one good reasons to believe that it has been reached. Since the method can only show that certain theories have not yet been falsified, how does it lead one to suppose that one's classification of truths is the true one? Miller explains:

> We should act as though the best corroborated of the hypotheses we have is true, *not because there are any reasons* for supposing that it is true (and that its predictions are) but because there are no reasons for supposing that it is not true. The hypothesis that has best survived the critical debate is the one that we have least reason to be false (emphasis added).[36]

What exactly is the advice suggested by this passage which borders on philosophical double-talk? The scientist is advised to act in a certain manner: suppose the best corroborated theories true. There are two ways in which to interpret the nature of this advice, both of which make the advice dubious. First, one might say that one is to suppose that the best corroborated theories are true for no good reason, that is, one is not being advised to act rationally. Second, one is given a reason to act in this manner, but the reason is a bad one. (Although Miller says that there are no reasons for acting as if the best corroborated theories are true, he seems in the next breath to give a reason. Is this not philosophical double-talk?) If a reason is being given, it is that one has least reason to think the best corroborated theory false. But to suppose (or act as if) it were true, based on this reason is simply to commit the fallacy *ad ignorantiam*. From the fact that one cannot disprove the theory (prove not-T) one cannot reasonable conclude, suppose, or act as if it (T) is true. So, Miller's defense of Popper on this point is undermined by a dilemma. One might be tempted to think, however, that the argument will turn in some way on a technical discussion of the notion of verisimilitude. Such is not the case. Suppose that the notion of verisimilitude is a perfectly pellucid notion, which, even Miller admits, it is not. The same problem will arise. Consider another passage from Miller:

> As long as B's claim to be closer to the truth than A is open to observational or experimental refutation, but not actually refuted, B may be preferred to A as a better approximation to the truth. This does not mean *we have reason to suppose* that B is closer to the truth than A is–only that we have no reason to suppose that it is not. Quite obviously, there is nothing resembling induction involved here.[37]

Obviously, there is no induction, but only either the advice to act in a nonrational fashion or the fallacy *ad ignorantiam*! So the question remains: how is the conjecture and refutation methodology compatible with the aim of science as the attainment of truth or true explanations?

Questions like this one point out a kind of vacuum that the tension in Popper's views created in the philosophy of science, a vacuum that has been filled in most densely by the 'irrationalist' movement in the philosophy of science. Irrationalists take Poppers's philosophy as demonstrating the impossibility of understanding science as a rational methodology for understanding reality in terms of attainable truth. Instead, one should view science as one among the many ways in which people interpret their experiences for certain purposes. The influences in the development of science are various and multifaceted, as much nonrational and irrational, social and psychological, as they are rational. The understanding of reality is as much a function of the knower and his needs as it is of some independent, objective reality. In fact, man as much creates the reality which is the subject of science as he does discover it. Truth as such goes by the board and scientific hypotheses are seen to provide explanations and understanding because they tell an interesting tale or spin a good yarn of the universe not because they deductively ensure certain testable consequences.

The strategy of such an 'irrationalist' picture of science is simple: Interpret the statements, hypotheses, and theories which make up science antirealistically. That is, divorce the semantics for such statements from any such connection with truth, since on Popper's account we never can have any idea that we are getting to the truth anyway. (We can, of course, 'approach' the truth in the sense of Popper's versimilitude. But why care about versimilitude when truth always escapes us?) When this move is made, one will of course abandon the idea that the aim of science is statisfactory explanations in terms of truth. Explanations in science are then viewed as providing understanding or bringing enlightenment in a 'hermeneutic' fashion, not unlike any of the other interpretive arts. Of course, the need for any method of rational justification vanishes and the tension created by Popper's deductivism is alleviated. With this picture a new vision of science emerges where philosophy of science, having forgotten methodological and normative concerns, is inseparable from the sociology and psychology of science.[38]

Although this strategy does represent an alternative to the Popperian dilemma I described and it certainly represents the current trend in the philosophy of science, this move is not one Popper would be willing to make. Consider how Popper discusses the relation between his method and the truth about reality:

> The task of science, which I have suggested, is to find satisfactory explanations, can hardly be understood if we are not realist...
> And yet it seems to me that within methodology we do not have to presuppose metaphysical realism... And although a rational treatment of methodology may be said to depend upon an assumed, or conjectured, aim of science, it certainly does not depend upon the metaphysical and most likely false assumption that the true structural theory of the world (if any), is discoverable by man, or expressible in human language.[39]

Clearly from this statement Popper intends to live with his dilemma. But only with the most unshakable dogmatism could one be comfortable with such an answer. Although Popper would not appreciate an analysis of his philosophy in terms of philosophy of language, what Popper has done is to separate the truth conditions for scientific statements and theories from any possible way of knowing that they obtain by using his methodology. That Popper is a semantic realist in the above sense (at least with respect to scientific language) should be obvious from his discussion of and use of Tarski's definition of truth. Although Popper's interpretation of Tarski's truth definition as a vindication of the correspondence theory of truth is controversial, his methodology seems to make semantic realism with respect to scientific statements largely irrelevant. His realistic interpretation of the semantics of scientific statements is out of touch with his methodology of science.[40] His realistic semantics for science and his conjecture and refutation methodology guarantee that metaphysics and epistemology will never meet, or if they do one will never known that they do on the sole basis of this methodology. The statements of scientific theory are true, Popper tells us, when they correspond to the facts. However, the conjecture and refutation methodology, while it may allow us to arrive at true theories, will never allow us to come to know that we have arrived at true theories, or know that our belief in the theories is justified. So, the problem is not exactly an inconsistency between his account of method and his account of truth; instead, the problem is that anyone interested in knowing one has attained the truth in his sense cannot in principle depend solely on his methodology. A methodology for arriving at the truth that in principle cannot provide knowledge that one has arrived at the truth is surely an unhappy one. This is why the tension of Popper's position seems most unbearable.[41]

NOTES

[1] Karl Popper, *The Logic of Scientific Discovery* (New York: Harper & Row, 1969), p. 30. Further references to this work are to this edition and are cited in the text as *L.S.D.*.

[2] Actually, in order to avoid misunderstanding one must realize that Popper does say that corroboration 'may lead us to prefer some theories to others', *Objective Knowledge*, Rev. Ed. (Oxford, 1979), p. 18. But this preference is simply based on the past performances of the theories. He continues, 'But it says nothing whatever about future performance, or about the 'reliability' of a theory' (*op.cit.*). Therefore, corroboration cannot provide reasons for accepting a theory as true.

[3] For an excellent discussion of these issues see Susan Haack, 'The Justification of Deduction', *Mind*, 85 (1976): 112–119.

[4] 'Reply to Lejewski', *The Philosophy of Karl Popper*, vol. 2, ed. Paul A. Schilpp in *The Library of Living Philosophers.* (La Salle Illinois: Open Court, 1974), pp. 1095–96. Popper's original attempt occurs in his papers, 'New Foundations for Logic', *Mind* 56 (1947) 193–235; 'Logic Without Assumptions', *Proceedings of the Aristotelian Society*, 47 (1947), 252–92; 'Functional Logic Without Axioms or Primitive Rules of Inference', *Proceedings of the Koninklijke Nederlandsche Akademie von Wetenschappen*, 50 (1947), 1214–24; 'The Trivialization of Mathematical Logic', *Proceedings of the Xth International Congress of Philosophy*, 1 (Amsterdam, 1948), 722–27.

[5] Czeslaw Lejewski, 'Popper's Theory of Formal Inference', *The Philosophy of Karl Popper*, Vol. 1, ed. Paul A. Schilpp, 632–670, p. 647.

[6] John Etchemendy, *The Concept of Logical Consequences* (Harvard, 1990), p. 6.

[7] Popper, 'Reply to Lejewski', p. 1096.

[8] David Miller, 'Conjectural Knowledge: Popper's Solution of the Problem of Induction', in *In Pursuit of Truth: Essays on the Philosophy of Karl Popper on the Occasion of His 80th Birthday*. ed. Paul Levinson (Humanities Press, 1982): 17–49, p. 39. The amount of critical material on Popper is voluminous and, of course, I can not consider it all in this paper, but Miller's paper seems to be considered the definitive apologetic for Popper.

[9] *Op cit.* Why could Popper not admit them both as scientific or testable? They simply cannot both be true. In what sense is Miller's criterion an extension of Popper's?

[10] *Op cit.*

[11] Miller's evolutionary comments about the true paradox sound surprisingly similar to Quine's. Quine, however, defends induction and our projection of green in terms of natural selection with the full knowledge that his account is circular. He says:

"...Let me say that I shall not be impressed by protests that I am using inductive generalizations, Darwin's and other, to justify induction, and thus reasoning in a circle. The reason I shall not be impressed by this is that my position is a naturalistic one; I see philosophy not as an *a priori* propaedeutic or groundwork for science, but as continuous with science". W.V. Quine, 'Natural Kinds', in *Ontological Relativity* (Harvard, 1970): 114–138, p. 126.

The similarity with Quine is curious because Popper cannot envoke Quine's kind of naturalism. Popper is a methodologist *par excellence* and he considers his methodological claims as outside the realm of empirical science; on the normativity of Popper's claims see page 10 below.

[12] *Op. cit.*, p. 40.

[13] John Watkins, *Science and Scepticism* (Princeton University Press, 1984). The account of the organic fertility requirement is put forth concisely on page 205.

[14] This 'proof' occurs in *Science and Scepticism*, pp. 313–315.

[15] *Op. cit.*, p. 315.

[16] Peter Achinstein, *Law and Explanation* (Oxford, 1971), pp. 114–116.

[17] Popper is explicit about the normative character of his claims concerning methodology in *L.S.D.*:

"...What I call 'methodology' should not be taken for an empirical science. I do not believe it is possible to decide, by using methods of an empirical science, such controversial questions as whether science actually uses a principle of induction or not" (p. 52).

He claims that he rejects the principle of induction 'not because such a principle is as a matter of fact never used in science, but because I think that is not needed; that it does not help us..'. (pp. 52–53). Popper again emphasized the normative character of his view in a footnote to 'The Rationality of Scientific Revolutions' in *Scientific Revolutions* ed. Ian Hacking (Oxford University Press, 1981): 80–106, pp. 99–100, 'my gospel is not 'scientific', that is, it does not belong to empirical science but it is, rather, a (normative) *proposal*'.

[18] Richard Feynman, *The Character of Physical Law* (M.I.T. Press, 1965), p. 156. Several things must be kept in mind when considering this as supporting Popper's claim. First, Feynman is writing much later than Popper's *L.S.D.* and his own ideas about methodology could be taken directly from reading Popper. Second, many times scientists are not the best judges of which methods they actually employ in their practice. And third, for as many scientists we find confirming Popper's method of conjecture and refutation we can find as many who are explicitly inductivists in their methodology. Consider the following passage from Einstein's popular exposition *Relativity: The Special and General Theory*; trans., Robert W. Lawson (New York: Crown Publishers, Inc., 1961) originally published in German in 1916:

From a systematic theoretical point of view, we may imagine the process of evolution of an empirical science to be a continuous process of induction. Theories are evolved and are expressed in short compass as statements of a large number of individual observations in the form of empirical laws, from which the general laws can be ascertained by comparison. Regarded in this way, the development of a science bears some resemblance to the compilation of a classified catalog (p. 123).

Einstein in this passage is clearly an inductivist, but consider a passage written two years later in an address celebrating Max Planck's sixtieth birthday:

The supreme task of the physicist is to arrive at those universal elementary laws from which the cosmos can be built up by pure deduction. There is no logical path to these laws; only intuition, resting on a sympathetic understanding of experience, can reach them. 'Principles of Reseach', in *Ideas and Opinions* (New York: Crown Publishers Inc., 1954), p. 226.

The Einstein of this passage (1918) sounds very Popperian. I think these quotes illustrate my last two points: (1) it is not clear that Einstein had any consistent methodology in mind when he worked, and (2) at least some of the time great scientists are explicitly inductivist in their methodological claims. Care must be taken therefore before one claims that any statements of actual scientists are evidence for the truth of a specific methodological doctrine. If one is selective enough almost any normative methodology can be shown to be employed in the history of science.

[19] This point was suggested to me by an anonymous referee.

[20] Of course, even for Popper, there is a sense in which theories must be 'accepted', at least tentatively, if they are to be tested. But this Popperian sense of 'acceptance' has nothing to do with epistemic warrant. (See footnote 2) Acceptance of a theory is at best heuristically justified. By the Popperian methodology one can never know that one has good grounds for *accepting the theory as true*. But this point anticipates the argument of section 4 below.

[21] This point and several of the following are made by Hilary Putnam in 'The "Corroboration" of Theories', *The Philosophy of Karl Popper*, vol. 1, pp. 221–240.

[22] This point is made very well by Paul R. Thagard in 'Why Astrology is a Pseudoscience', *Proceedings of Philosophy of Science Association*, Vol. 1, ed. P.D. Asquith and I. Hacking (East Lansing: Philosophy of Science Association, 1978) pp. 223–224. I disagree, however, with some of Thagard's claims about falsifiability, in particular, the falsifiability of astrology.

[23] Karl Popper, 'The Rationality of Scientific Revolutions' in *Scientific Revolutions*, ed. Ian Hacking (Oxford, 198), p. 98.

[24] I say 'many times' because it seems clear that scientists have recognized that a theory has been *de facto* falsified and conclude that it must be rejected (that its acceptance is *not justified*) although there was no competing theory or explanation available. For example, many scientists saw that the Michelson-Morley experiment decisively falsified Newtonian theory in a way that made it unacceptable even though there was no adequate competing theory at that time.

[25] Karl Popper, 'The Aim of Science', in *Objective Knowledge: An Evolutionary Approach*, rev. ed. (Oxford University Press, 1979); 1991–205, p. 191.

[26] As Lakatos says, 'One can be "wise" only after the event'. Imre Lakatos, 'History of Science and Its Rational Reconstructions', in *Scientific Revolutions op cit*: 107–127, p. 118.

[27] This point has been made by both N.R. Hanson, 'The Logic of Discovery', *Journal of Philosophy*, 55 (1958) p. 1079–89, and Peter Achinstein, *Law and Explanation*.

[28] It should be clear that I don't mean to equate this kind of reasoning with what Harman calls 'inference to the best explanation' although it may be of a piece with it. See Gilbert Harman, 'The Inference to the Best Explanation'. *Philosophical Review*, 64 (1965), 89–95.

[29] Actually, this notion of measure of content may make things too easy on Popper. It is not at all clear the notion of 'basic statement' can be elucidated well enough to make sense of enumerating how many basic statements an hypothesis rules out. Unless such problems are solved talk of a measure of content is hopelessly vague. To be fair to Popper he sometimes does not take the notion of *measure* all that seriously. See e.g. the discussion in 'The Two Faces of Common Sense' in *Objective Knowledge*. Moreover, a Bayesian analysis of 'measure of content' in the spirit of Popper has been proposed by Roger Rosenkrantz, 'Why Glymour is a Bayesian' in *Testing Scientific Theories*, ed. John Earman, *Minnesota Studies in the Philosophy of Science*, vol X (University of Minnesota Press, 1983): 69–97, see esp. pp. 81–82.

[30] This distinction is clear when one understands an hypothesis as Quine and Ullian do: 'Calling a belief a hypothesis says nothing as to what the belief is about, how firmly it is held, or how well founded it is. Calling it a hypothesis suggests rather what sort of reason

we have for adopting or entertaining it. People adopt or enter in a hypothesis because it would explain, if it were true, some things that they already believe'. *The Web of Belief*, 2nd ed. (New York: Random House, 1970), p. 66.

[31] Wesley C. Salmon, *The Foundations of Scientific Inference* (University of Pittsburg Press, 1966), esp. pp. 111–124, and Mary Hesse, *The Structure of Scientific Inference*. (University of California Press, 1974), esp. pp. 136–137. Although Salmon also vaguely connected his discussion with Hanson's talk of a logic of discovery, his view differs significantly from mine.

[32] I put 'calculate' in scare quotes to emphasize that scientists do not usually calculate in the strict sense of probabilities according to the Bayesian rule. The Bayesian rule just illustrates the kind of informal reasoning or 'calculation' that makes hypothesis formation into educated guessing.

[33] 'The Aim of Science', *op. cit.*, p. 192.

[34] Actually, one should be more careful. The conjecture and refutation methodology *might* justify, give good reasons for the belief, but one can never know via that methodology alone that this is the case. I owe this point to an anonymous referee.

[35] Miller, *op. cit.*, p. 18. This seems a bizarre characterization of the task of empirical sciences. One could indefinitely list truths and separate them from falsehoods without ever doing anything remotely resembling empirical science. The empirical sciences are obviously more ambitious than Miller's claim suggests.

[36] *Op. cit.*, p. 40. In this paper, Miller claims to 'refute all such falsifications of falsificationism' (p. 18). Although I cannot possibly in the scope of this paper respond to all of Miller's apologetic points, I believe that this argument represents the crux of the paper which is of a piece with his attempt to drive a wedge between attaining the truth and knowing that one attains the truth.

[37] *Op. cit.*, pp. 42–43.

[38] I don't claim by any means that the development of irrationalism and antirealism is simply the result of recognizing a certain tension in Popper. I do claim, however, that difficulties like these motivated the development of the movement. Moreover, specific figures do seem to have emerged by filling the gap left by Popper in just the way I suggest; e.g., Feyerabend.

[39] 'The Aim of Science',*op. cit.*, pp. 203–204.

[40] This point is similar to the one Paul Benacerraf makes concerning the relationship between semantics for mathematical statements and mathematical knowledge in 'Mathematical Truth', *Journal of Philosophy* 70 (1973): 661–680.

[41] I would like to thank Paul Sagal, Bill Throop, Thomas Uebel, Michael Williams and two anonymous referees for their comments on an earlier version of this paper.

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