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BOLD HYPOTHESES: THE BOLDER THE BETTER?

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If scientists want a good explanation of why certain events or phenomena occur should they propose the boldest hypothesis they can imagine? The answer to this question is a resounding 'yes' according to an influential normative model of scientific rationality. On this model the story of what makes science science is told as follows. Science begins with problems. The scientist invents some hypothesis, set of hypotheses, or theory to solve the problems. These conjectures do not involve inferences; they are guesses. They may be the end result of a complex causal or psychological process in the mind of the scientist, but they are not generated by an algorithm or mechanical method. The discovery of new hypotheses takes creative intuition. Inferences do not play a major role in the context of the discovery of a new hypothesis. Once a conjecture is made, however, the scientist must test the theory empirically. What the scientist does is deduce observational consequences from the theory. These are the testable predictions. If the observational consequences do not occur as the theory predicts, then the theory has been falsified. If the events occur as predicted, then the scientist simply says that the theory has passed a test; it has been corroborated. In the testing of the theory or hypothesis in the context of justification or evaluation inferences do occur but only in the form of deducing observational consequences from the theory or hypothesis and employing *modus tollens* to reject it. Scientists should only concern themselves with making conjectures and subjecting them to severe testing. In the end scientific method is principally a critical process of conjecture and refutation. So in order for science to progress scientists should make as bold a conjecture as possible when they want something explained because the bolder the hypothesis the more susceptible it will be to severe testing and the more we can learn from experience. The more severe tests an hypothesis can pass the better off it will be in the context of evaluation, whereas if it fails a severe test then one can sooner propose new explanations. So, in the context of

* The authors would like to 'thank' an anonymous referee for some severe criticisms of a previous draft of the present paper.

discovery we get the following rule of thumb: when proposing an hypothesis, the bolder the better.[1]

As nice a tale of science as this story tells, its truth has not gone uncontested. As a normative guide for understanding the rational nature of science and scientific progress, it *seems* an oversimplification. Lakatos said that the philosophy of science without the history of science is empty, and surely some 'reflective equilibrium' is required between the historical data concerning scientific development and any adequate account of the rational nature of science and scientific progress. Moreover, logical difficulties have been pointed out in this pure deductivist view of evaluation. Despite the plethora of critiques of this account what has casually been accepted without much ruckus is that there is little philosophically interesting, although much of historical and sociological interest, to be said about the context of discovery. But there is much of philosophical, not just historical or sociological, interest to be found in the problems of hypothesis formulation that this story leaves untold. We propose to bring these issues into clearer view by asking 'Are bolder hypotheses always better'? The answer is, well, yes and no. There are two senses of the 'boldness' of hypotheses which we think go unrecognized and are easily conflated or confused in the above story. We will distinguish between a *psychological* sense of boldness and a *logical* sense of boldness. If one distinguishes between a psychological (subjective-qualitative) sense and a logical (objective-quantitative) sense of boldness, then one can explain in what sense scientists want bold hypotheses and in what sense they want conservative ones. In fact, on our view the virtues of boldness and conservativism go hand in hand when an hypothesis is proposed as a candidate for testing. What scientists want to propose are hypotheses bold in the logical sense, but not in the psychological sense. But even this is an oversimplification as further discussion will show. When one recognizes this consequence of the distinction, one will see more clearly the kinds of reasoning which are involved when an hypothesis is proposed for testing. In

[1] This story of science is of course the one told elegantly and in great detail by Sir Karl Popper, *The Logic of Scientific Discovery* (New York: Harper & Row, Publishers, 1959). *Conjectures and Refutations* (New York: Harper & Row, Publishers, 1963). *Objective Knowledge: An Evolutionary Approach* (Oxford University Press, 1972). Although Popper believed that hypotheses could never be justified or even verified in a strict sense, it should be clear that he was comfortable with some version of the distinction between 'the context of discovery' and the 'context of justification' and that he used such a distinction to explicate his own work. See *The Logic of Scientific Discovery* p. 315, where he employs a version of Reichenbach's original distinction.

fact, we think one will see that the reasoning involved in the context of discovery is not independent of reasoning in the context of evaluation.

The idea of proposing the boldest imaginable hypothesis in order to explain something may on the face of it seem silly. Why should guessing at the *wildest* in the sense of *least probable* explanations conceivable ever afford a good explanation? Would not such guessing lead one on a wild-goose chase rather than closer to the truth? These may seem reasonable questions with which to challenge the old story, but they consider bold conjectures independent of the context of evaluation. What makes proposing bold hypotheses reasonable is determined by the role they play in testing. Given a certain notion of boldness, bold hypotheses are more susceptible to potential refutation and therefore can pass severe tests or be refuted. The critical role of testing and refutation is what prevents pure conjecture from being a wild goose chase and what points it toward good explanations. For Popper, this is the way we learn from experience.

Although the idea of proposing bold hypotheses is not so unreasonable as it might seem at first sight, it should be obvious that making it reasonable depends on understanding 'boldness' in a special sense. On this account one wants boldness to be connected with a greater number of possible tests and it is natural that one should define the *measure* of *boldness* for an *hypothesis* as the number of observation statements it rules out. The measure of *boldness* is also the measure of *content*. Because a bold hypothesis rules out more observation statements than a less bold hypothesis, it will be susceptible to a greater number of tests. That is, it will make more predictions that could in principle falsify it. Therefore, simply on the basis of this 'measure' of boldness it is worthy of testing. So, if one is careful to define boldness in the above manner, then bold hypotheses are obviously better as candidates for explanations and testing.

'Boldness,' however, is not itself a technical term in Popper's work. It is, of course, a metaphor. It, along with related notions like *risk* and *severity*, provides Popper's methodology with an atmosphere of aggressiveness, courage, one might almost say *machismo*. Of course these terms do have explications, but sometimes the fit is far from perfect and sometimes the relationship between terms like 'risk' and 'boldness' raises some questions. Usually *boldness* is explicated in terms of falsifiability, information or content. In terms of falsifiability, we can compare the boldness of two hypotheses in

terms of the sets of basic statements incompatible with the hypothesis. The bolder then in this sense appears to be a quantitative notion; the more basic statements excluded the bolder. There are technical problems involved with comparisons between infinite classes, but let us suppose as is likely that they are soluble. Notice that boldness here is objective in that its explication does not involve the beliefs, expectations etc. of members of the scientific community. It does not have even the whiff of psychology about it. It seems also that we can apply this kind of boldness to basic statements themselves, in that *a* is a bolder statement than *b* if *a* is incompatible with more potential falsifiers than *b*. So precise predictions would be more bold than imprecise ones because they exclude more basis statements and hence are easily falsifiable. We have thus a quantitative objective sense in which statements, hypothetical and basic, stick their necks out in the sense of exposing themselves to refutation. It is rational to identify bold conjecture with risky conjecture. After all boldness seems to involve nothing more than risk of falsification. Yet, on occasion when Popper comes to talk of risk he does not employ the above objective-quantitative notion. So we have to be careful about how we are to follow the bolder the better methodological rule of thumb.

Popper speaks of risky predictions. We see no way of distinguishing risky predictions from bold predictions and no way of sharply separating bold predictions from the bold hypotheses which engender them. Here is what Popper has to say about 'risky predictions.' 'Confirmations should count only if they are the result of *risky predictions*; that is, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory – an event which would have refuted the theory.'[2] Here riskiness involves our expectations in a crucial way; it fits in well with the notion of *a priori* or initial probability in Bayesian probability theory. What Popper recommends is that we consider hypotheses involving predictions with low initial probability. Here is a sense for 'the bolder the better,' but a rather different one than the objective-quantitative one discussed above.

There is even a sense in which testability gets tinged with psychology, since a genuine test of an hypothesis, Popper tells us, involves an attempt, sometimes a *sincere* attempt to falsify it, but that means taking seriously the expectations of the investigator or the community of investigators and adopting the subjective-

[2] 'Science: Conjecture, and Refutation' In *Conjectures and Refutations*, p. 36.

qualitative idea of risk, or severity of test. [Popper himself is aware of some 'formal analogies' between Bayes theorem (see subsequent discussion) and his own theory of degree of corroboration.] Should we define *falsifiability*, *information*, *content* and related notions in terms of risky tests in this sense we would destroy the purely objective content of these notions. Perhaps Popper, at the methodological as opposed to the logical level, really does have a use for such a notion; but we do not find Popper explicit about this kind of problem.

We have been dealing with the riskiness of predictions and it does seem we seek hypotheses which yield many such predictions; but do we want our hypotheses to be risky relative to our expectations or background theories? Of course, if we do not have background theories to appeal to, where, outside of empirical content, do we get our boldness measure from? On the other hand, given our background theories we want our hypotheses not to be all that surprising, since our Bayesian approach counsels us to maximize initial probability. We combine high initial probability of the hypothesis under investigation with low initial probability of the test prediction. So here is where psychology will play a role. In this last context where our subjective expectations concerning an hypothesis are determined relative to a background theory, the bolder hypothesis does not seem to be the better at all.

There are several difficulties with the quantitative notion of boldness. First, as has been pointed out many times, no hypothesis or statement by itself rules out any observation statements. Popper is aware of this, though he does not always take the trouble to stress it. A second objection questions the clarity of the notion on another point. As we indicated, the notion of a definite measure of boldness depends on the notion of *a number* of observation statements ruled out by the hypothesis. So, this notion of boldness is only as clear as the notion of identifying observation or basic statements. What is not clear is that there is any effective way of individuating and counting observation sentences. Popper is quite aware of the vagueness of the notion 'empirically basic sentence', but until this can be explained, defining the 'measure' of boldness in terms of the number of basic statements ruled out is inevitably vague.

One might suggest that this problem can be solved by employing Quine's definition of an observation sentence. According to Quine a sentence is an observation sentence if and only if all competent speakers of the language would assent to the sentence under the same stimulations of their senses and dissent from the sentence

under the same stimulations of their senses.[3] Although this definition seems to afford an effective method for determining the observationality of any sentence, it is a practically useless method for enumerating observation sentences in order to determine how many are ruled out by an hypothesis. If one is concerned with testing an hypothesis by determining whether the observation sentences predicted by the hypothesis are true, then one had better begin with a clear idea as to what are the observation sentences predicted by the hypothesis and what are not. Being told that an observation sentences is one that all competent speakers of the language will assent to given the same stimulations of their senses is of no practical help at all. What is one to do? Take all the sentences of the theory or entailed by the theory and test all competent speakers of the language under the same sensory conditions to see if they come to complete agreement? This is practically impossible and is certainly fruitless as a pre-condition of scientific testing of theories. So, it seems that if one is to test an hypothesis against observation sentences one either must have an intuitively clear idea of observation sentences to begin with or concede that the notion of observation sentence is vague and that testing a theory or hypothesis against observation statements is not a cut-and-dried affair.[4] In either case any clear-cut technical or quantitative 'measure' of boldness of an hypothesis goes by the board without certain additional functions or assumptions. Popper would reject the Quinian approach anyway, as psychologistic.

As suggested above, one way to save this notion of measure of boldness from the foregoing problem and so maintain the old account of bold hypotheses being better is to adopt a sort of conventionalism, something of which Popper is quite aware.[5] On this position, the relevant group of scientists determine by conventional decision what are to count as the observation sentences against which a theory or hypothesis is tested. This move certainly solves the aforementioned problem concerning the vagueness of the notion of an observation sentence. Of course, if

[3] W. V. Quine, *Word and Object* (M.I.T. Press, 1960), pp. 42–45. For a similar definition of observation statement see Paul K. Feyerabend, 'An Attempt at a Realistic Interpretation of Experience,' in *Realism, Rationalism, and Scientific Method: Philosophical Papers* vol. 1 (Cambridge University Press, 1981): 17–36, pp. 17–19.

[4] It is the latter line which Quine adopts in the form of Duhemian holism. Quine readily admits that observationality is a matter of degree and therefore confirmation or justification of a theory is never a cut-and-dried affair. Many times what justifies a theory will be vague pragmatic considerations like simplicity or conservatism.

[5] This conventionalism, which makes the 'basic statements' against which a theory or hypothesis is tested a matter of convention, is the position adopted by Popper.

observation sentences are a matter of conventional decision on the part of the relevant group of scientists doing the testing, then the special epistemological role of observation statements on which the virtue of bold hypotheses was based is weakened. The empirical tribunal by which science is judged becomes somewhat arbitrary.

Let us now retell the old story by focusing on a specific case. Scientists who make a proposal do so because they want to explain some events or phenomena which go unexplained given current theory and other relevant information. The call for explanation arises only within a pre-existing problematic and theoretical context. Because it is *this* context which is the context of discovery, there will be certain rational constraints on what makes an hypothesis worthy of proposal. Determining these rational constraints will amount to elucidating a 'logic' or methodology of discovery; that is, elucidating a kind of reasoning that goes on when hypotheses are proposed as worthy of testing. In this context, the two different notions of 'boldness' we stated above become clear. Distinguishing these two senses will make clear how the logic of discovery is connected with the logic of evaluation.

In order to illustrate the idea let us consider a hackneyed example – the discovery of Neptune.[6] Given the well-entrenched Newtonian theory of 1846 and other relevant information such as the measurements of the orbit of Uranus, a certain anomaly existed which, *in that context*, called for explanation. The measurements of the orbit of Uranus were not those predicted by the theory. The theory was seemingly falsified, but the scientists of the day did not reject the theory as refuted and propose a bold new theory. No. Instead they held to the theory and proposed an explanation from within it: there must be another planet out there of this size and at this location. Thus, it seems they did not propose a bold hypothesis, but a conservative, safe one. And they were right. The move seems all the more reasonable because the need for

[6] Although we use only this example, we think it is easy to extend our points to many other cases. 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. That 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. This case is importantly different from the hypothesis about Neptune because Wegener's hypothesis is usually considered a revolutionary one in geology, whereas the discovery of Neptune fell within the 'normal' science of the Newtonian paradigm. We think our discussion of boldness and conservatism applies to both kinds of case.

explanation, the context in which the discovery was made, arose from strong commitment to the theory. Does this famous example not show that the conjecture and refutation model of science is flawed, and that the best advice when proposing an hypothesis is not 'the bolder the better' but something more like 'preserve the *status quo*'?

All this does not show that the hypothesis was not 'bold' in a reasonable sense. Indeed, we think we can use the example to illustrate how the virtues of boldness and conservatism often coincide. When the 'logic' of discovery is discussed, it is usually referred to by the kinds of 'plausibility considerations' one recognizes when one proposes an hypothesis.[7] Such 'plausibility considerations' in the context of the logic of discovery provide for the *alleviation of boldness in the psychological sense*. An hypothesis is bold in a *psychological sense* if it is surprising that someone could possibly believe it.[8] If one proposes a conjecture and everyone (or almost everyone) concerned with the issue asks 'How could you possibly believe that?', they are expressing their astonishment at what they take to be a bold hypothesis (in the *psychological* sense). This kind of boldness is not usually what is desired in a good hypothesis. One usually alleviates this sense of boldness by making it plausible within the context of a background theory and other relevant information. The kind of reasoning involved in seeing that the hypothesis plays a certain explanatory role in this theoretical context is usually considered part of the logic of discovery. If that were all there was to the logic of discovery, then one can see that reasonable advice would be to propose hypotheses which are not bold (in this sense). Because an hypothesis is not bold in the *psychological sense* in so far as it can be made to play an explanatory role in a given theoretical context, it is easy to see that proposing hypotheses that are not bold in this sense will go hand in hand with conservatism in science. So, if this psychological sense were the only relevant sense of the boldness of an hypothesis, then it would not be reasonable to propose bold hypotheses instead of safe, conservative ones.

[7] That the logic of discovery is simply a matter of plausibility considerations seems to be the position of both N. R. Hanson, 'The Logic of Discovery,' *Journal of Philosophy*, 55 (1958): 1079–89, and Wesley Salmon, 'The Foundations of Scientific Inference,' (University of Pittsburg Press, 1966), see esp. pp. 111–121.

[8] Obviously this notion of boldness in psychological sense is a somewhat vague notion and boldness in this sense will be a matter of degree. How bold an hypothesis is in this sense will also be a subjective matter, but that is perfectly acceptable given an explicit characterization of the notion in terms of subjective probability.

A prediction is bold or risky in the psychological sense if its probability independent of theory is low, but is high given the theory and other relevant information. As Bayesian confirmation theorists have long pointed out, one wants one's predictions to have the former property because if they do the test of the hypothesis will be an important test.[9] If the predictions are bold in the psychological sense and turn out true, this will add greatly to the corroboration or probability of the theory based on the simplified Bayesian rule:

$$\mathrm{pr}(h,p) = \frac{\mathrm{pr}(h)}{\mathrm{pr}(p)}$$

Therefore, based on an ordinary Bayesian understanding of evidence, one can conclude that confirmation of a bold hypothesis in the sense of yielding bold predictions greatly increases the confirmation of the theory of which it is part.[10] Note also that other things being equal the logical boldness of a prediction is desirable because it usually goes hand in hand with the relevant psychological boldness of prediction. That a precise prediction comes out right is usually more unexpected than when this happens to an imprecise prediction. So, what scientists want to propose are hypotheses that are bold in the logical sense.

Actually, when an hypothesis is proposed two contrasts between psychological and logical boldness must be emphasized. One must recognize that the distinction applies not only to the proposed hypothesis itself but also to its predictions. An hypothesis bold in the psychological sense is undesirable. One wants an hypothesis bold in the logical sense. The logical boldness of an hypothesis, however, will make psychological boldness desirable in another respect. The psychological boldness of the hypothesis itself will not be desirable, but the psychological boldness of its predictions will be since the psychological and logical boldness of the predictions will coincide. We are thus suggesting two contrasts concerning psychological boldness. On the one hand, one should propose hypotheses bold in the logical sense but not in the psychological sense. On the other

[9] Both Mary Hesse, in *The Structure of Scientific Inference* (University California Press, 1971), and Wesley Salmon, *op. cit.*, have pointed out how Bayesian confirmation theory can accommodate Popperian points about evaluation.

[10] Certainly, we do not want to suggest that the Bayesian view of confirmation is not without problems, such as the problem of 'old evidence.' However, we think that problem can probably be handled along the lines of Daniel Garber, 'Old Evidence and Logical Omniscience in Bayesian Confirmation Theory,' in *Testing Scientific Theories, op.cit.*, pp. 99–132.

hand, however, one will want to propose hypotheses which together with a background theory make psychologically bold predictions, even though the hypothesis itself is not bold in the psychological sense. The first contrast is between the psychological and logical boldness of an hypothesis, the second is between the psychological boldness of an hypothesis and its predictions.[11]

What seems to have gone unnoticed is that reasoning in the context of discovery will involve recognizing both that a good hypothesis usually is not psychologically bold, though its predictions may be, and that it is logically bold. If, when proposing an hypothesis, one were simply concerned with proposing conservative hypotheses – ones which would explain what wants explaining assuming true the theory one already holds, then one would always be tempted to employ ad hoc devices to preserve the initial commitment. However, when an hypothesis is proposed it is proposed as a *candidate worth testing*. So one must recognize when proposing an hypotheses for testing in hopes of getting good explanations one must not only consider whether the hypotheses is plausible given one's other theoretical beliefs, but one must also determine that the candidate for testing is worthy of testing.

The two kinds of considerations, plausibility and boldness, often go hand-in-hand. If the *semantic content* of an hypothesis is determined by the truth-conditions, then one can see how the psychological boldness of an hypothesis is determined by its semantic content. An hypothesis will be *psychologically bold* if the initial subjective probability of the statement is low in the sense that our knowledge of its truth-conditions alone makes it highly unlikely that it could be true. This psychological boldness is alleviated by proposing the hypothesis within the context of a theory which already has a high subjective probability. That is, one proposes a conservative hypothesis which is such that its probability is high, given the theory and other relevant information. So, one proposes an hypothesis which is plausible, given what one already believes. But having done this one will have placed the hypothesis in a context where one can recognize its boldness in the *logical* sense while alleviating its boldness in the psychological sense. And this is what will make the hypothesis worthy of proposal. What makes a statement with a certain semantic content *an hypothesis* is that one recognizes that, if it were true and given what one already believes, it would explain what one wants explained. However, what makes

[11] We are indebted on this point to a suggestion by Edward Craig.

an hypothesis *worthy of testing* is that one recognizes not only that, if true, it would explain what one wants explained, but also that it would, if true, provide some non-trivial support or corroboration even for the background theory in the context of which it does the explaining.

Consider again the discovery of Neptune. The initial subjective probability of the prediction that there was a planet of a particular size at a particular location beyond Uranus must have been very low in 1846. Independent of the knowledge of Newtonian theory and the measurements of the orbit of Uranus, the hypothesis would have been psychologically bold. However, Leverrier proposed the hypothesis within the context of Newtonian theory and recognized that the hypothesis, if true and given the theory, would explain the measurements of the orbit of Uranus. This context relieved his conservative proposal of psychological boldness. But once one recognizes that a conservative hypothesis which is not bold in the psychological sense can yield predictions which would have been bold independent of the theory, one will recognize that the prediction will be an important test case for the theory. So, not only will one recognize that the proposal is plausible but also that it is worthy of testing. These complex and complicated considerations which go on when an hypothesis is proposed can be referred to as the 'logic' of discovery.[12] If abduction is reasoning in which the conclusion is a claim that a particular hypothesis is worthy of testing, then abduction, far from being a simple matter, will involve the kinds of considerations just mentioned. But in that case 'abduction' or 'the logic of discovery' will not be sharply separable from the 'logic of justification.' Therefore, the conjecture of a bold hypothesis is not simply the outcome of a psychol[illegible]l process of guessing, but will involve some complicated ir[illegible]ences and reasoning as well. So, any story of how best to unde[illegible] and science as a rational endeavor which belittles the role of reasoning in discovery can only be part of the story.[13]

[12]We should emphasize that we are not claiming that scientists should or do use the Bayesian rule and calculate subjective probabilities to decide whether an hypothesis is worthy of testing. The Bayesian strategy is simply used to illustrate the rationale of the kinds of informal considerations and reasoning that go into determining whether an hypothesis should be proposed.

[13] On Popper's view it is really hard to understand how it is possible to hold that the logic of discovery is totally independent of the logic of evaluation. If Popper's whole logic of discovery can be partly summed up in the rule of thumb – 'always make the boldest conjectures imaginable that would explain what one wants explained,' then it should be obvious that how 'boldness' is understood on this rule is determined by the role it plays in the logic of justification. For discussion of related points see, Herbert A. Simon. 'Does Scientific

This tension between the objective, quantitative (logical) and the subjective, qualitative (psychological) in connection with discussions of boldness and risk appears to be not much appreciated by Popper.[14] Perhaps we can dramatize the tension a bit more effectively if we employ notions from Popper's later trinitarian ontology: Popper's worlds one, two, and three.

Concepts like *truth content, information, degree of falsifiability*, even *verisimilitude* belong to Popper's world three. Put roughly, these concepts can be explicated completely in terms of relations among abstract objects. Of course, *truth* itself is not such an example but if we assume *truth* as already explicated then we need not leave Popper's third world. To say for instance that theory ∂ is scientific and theory ß unscientific is at least to say that theory ∂ is bolder than theory ß insofar as theory ∂ contains a testable consequence and theory ß does not. That is to say theory ∂ is informative at least to some degree where theory ß is not. All this is third world talk. The empirical content of the theory does not change over time. Our tests affect matters like corroboration but they do not affect what cannot be affected. This does not mean of course that our knowledge of the content of theory does not change. But this is not a world three matter but at least in part a world two matter. Now the concept of risk that we have explicated in a Bayesian way is in part a world two notion because it involves our expectations or our acceptances, however tentative, of background theories. This kind of risk obviously changes in time. A risky prediction three times tested and not falsified is the fourth time much less risky, but its empirical content has not changed. Our background knowledge has changed. But how, without inductivist assumptions, will this change make future repetitions less risky? Popper in addenda to 'Conjectures and Refutations' provides an explication of this latter concept of risk in terms of probability theory. But the interpretation of the probability calculus in this area involves the apparent world two concept of tentative acceptance, whereas the use of the probability calculus to explain objective notions like content or information does not require such an interpretation. For instance, the principle that probability varies inversely with information or content involves no world two notions at all. Perhaps Popper

Discovery Have a Logic?' *Philosophy of Science* Dec 1973 pp. 471–481. Simon's analogy between discovering good scientific theories and discovering good moves in chess is quite convincing.

[14] See for instance, Popper's reply to Musgrave in P. A. Schilpp, editor, *The Philosophy of Karl Popper* p. 1079. He claims to see only verbal problems and not genuine problems.

believes there is an easy way to reconcile the two sorts of concepts or more to the point that he can show how what appears to be a world two concept is not really such a concept at all. If he could show that apparent references to subjects are just that, merely apparent, then of course much of the tension disappears and the remainder of the task becomes simply that of reducing or relating some world three concepts to others. In the technical addenda previously referred to, we get the idea that this is indeed Popper's position but we cannot be sure. The position certainly is not worked out, and until it is we can only conclude that a reasonable logic of discovery cannot be explicated in purely objective terms but must remain in the sense explained here partially psychologistic.[15]

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[15]For what appears to be the main Popperian line of response see his discussion of background knowledge in 'Truth, Rationality and the Growth of Knowledge' in *Conjectures and Refutations*' especially p. 240.

ERRATUM

The article by Timothy Cleveland and Paul Sagal entitled 'Bold Hypotheses: The Bolder the Better?' (*Ratio* II: 2 December 1989) included (p. 117) the simplified Bayesian rule. Unfortunately this was printed incorrectly.

The simplified Bayesian rule should read:

$$\text{pr (h, p)} = \frac{\text{pr (h)}}{\text{pr (p)}}$$

instead of

$$\text{pr (h, p)} = \frac{\text{pr (h)}}{\text{pr (p, h)}}$$