Machine Guessing – II

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Abstract

According to Karl Popper, the evolution of science, logically, methodologically, and even psychologically, is an involved interplay of acute conjectures and blunt refutations. Like biological evolution, it is an endless round of blind variation and selective retention. But unlike biological evolution, it incorporates, at the stage of selection, the use of reason. Part I of this two-part paper begins by repudiating the common beliefs that Hume's problem of induction, which compellingly confutes the thesis that science is rational in the way that most people think that it is rational, can be solved by assuming that science is rational, or by assuming that Hume was irrational (that is, by ignoring his argument). The problem of induction can be solved only by a non-authoritarian theory of rationality. It is shown also that because hypotheses cannot be distilled directly from experience, all knowledge is eventually dependent on blind conjecture, and therefore itself conjectural. In particular, the use of rules of inference, or of good or bad rules for generating conjectures, is conjectural. Part II of the paper expounds a form of Popper's critical rationalism that locates the rationality of science entirely in the deductive processes by which conjectures are criticized and improved. But extreme forms of deductivism are rejected. The paper concludes with a sharp dismissal of the view that work in artificial intelligence, including the JSM method cultivated extensively by Victor Finn, does anything to upset critical rationalism. Machine learning sheds little light on either the problem of induction or on the role that logic plays in the development of scientific knowledge.

4 Critical rationalism

This is the appropriate moment to be more explicit about critical rationalism, and to explain why, despite the prominence it gives to explanatory hypotheses (Popper 1957b, Deutsch 2011, Chapter 1), it has no truck with abduction as a process of inference. Given how often it has been expounded, by Popper himself, by aficionados such as myself, and by antagonists, it may be thought extravagant to devote more space here to a statement of its central ideas. The truth, however, is that critical rationalism has time and again been represented as incorporating justificationist tendencies; tendencies that are unnecessary and unwelcome, despite their having some support in some of Popper's expositions (for example, in his discussions of verisimilitude in 1972, Chapter 2, § 7, and in 1983, Part I, § 2, which are discussed briefly on p. 126 of my 2006). In Chapter 2 of my (1994), I took to task many of the criticisms

A paper presented at the conference J. S. Mill's Ideas on Induction and Logic of the Humanities and Social Sciences in the Cognitive Research and in Artificial Intelligence Systems, held at the Russian State University for the Humanities (URSS), Moscow, in June 2011. The central theme of the meeting was the work of Victor Finn in the theory of intelligent systems, which is briefly discussed in § 7 below. A Russian translation by Delir Lakhuti has been published in *Voprosy Filosofii* 7, pp. 110–119, and 8, pp. 117–126, in 2012. My thanks are due to Ali Paya, who drew my attention to Searle (1999), and to Alain Boyer for some useful hints.

that, in its application to empirical science, critical rationalism (or falsificationism) cannot escape an appeal to inductive inference or to a principle of induction, and must be pronounced a failure. On the other side, some writers influenced by Lakatos have been encouraged by Popper's more conciliatory statements of his position to concoct half-baked versions of critical rationalism with rich (but to my taste, unsavoury) justificationist flavours. In equal measure opponents and proponents 'are, I feel, unconscious witnesses to ... [Popper's] originality; for they fail now ... to grasp his main point', as Popper himself wrote (1945, Chapter 2, note 2) about some of those who belittled the novelty of Heraclitus' doctrine of universal flux.

There are important secondary features, such as the recognition that good hypotheses solve genuine problems, but the spirit of critical rationalism may be condensed into three principles:

(a) Hypotheses may be freely conjectured (as Whewell said). Even if there is some procedure for inferring hypotheses from facts, such inferences are not necessary.

(b) Every attempt must be made to depose a conjecture by deducing from it consequences that may clash with the facts (or otherwise reveal it to be inadequate).

(c) A conjecture that is not rejected remains accepted. No further action is needed (except further tests). Even if 'confirmation' is possible, it serves no function.

According to this position, science really does consist of not much more than positive conjectures controlled by negative refutations. Induction and abduction play no part in the feverish dialectic. The import of principle (a) is that, in the realm of invention, *induction and abduction are not needed*. The import of (b) is that, in the realm of evaluation, *deduction is needed*. The import of (c) is that, in the realm of evaluation again, *induction and abduction are still not needed*. Nothing happens to a conjecture that survives severe testing; it is neither proved nor improved nor supported. But reasoning plays a grand part — in the demolition of conjectures, not in their construction. Critical rationalism fully deserves both parts of its name: *critical* and *rationalism*.

Although the critical rationalist hopes to identify, among the available explanatory hypotheses, the one that accounts best for the phenomena in the domain under investigation, he will not infer it non-demonstratively from the empirical evidence, but will attempt to bring it into the limelight by disqualifying its competitors. The more wedded he is to empiricism, the more these disqualifications will be underwritten by empirical refutations, rather than by metaphysical objections or other critical broadsides. (Nothing in this paper should be taken as recommending an unaffected empiricism.) Should more than one explanatory hypothesis escape refutation, the critical rationalist will regard his project as unfinished. From this point of view, resorting to a non-demonstrative inference to complete the job looks like an unempirical swindle. As Russell saw, empiricism is threatened by an impatient demand for finality. What he did not clearly comprehend was that by jettisoning justification, both empiricism and rationality may be saved.

Many authors have thought that leaving the origination of conjectures to the unbridled and undisciplined imagination leaves too much to chance. Peirce, for example, asked (1903, *ibidem*):

> But how is it that all this truth [in science] has ever been lit up by a process in which there is no compulsiveness nor tendency towards compulsiveness? ... Is it by chance? Consider the multitude of theories that might have been suggested. ... A physicist comes across some new phenomenon in his laboratory. ... Think of what trillions of trillions of hypotheses might be made of which one only is true. And yet after two or three or at the very most a dozen guesses, the physicist hits pretty nearly on the correct hypothesis. By chance he would not have been likely to do so in the whole time that has elapsed since the earth was solidified. You may produce this or that excellent psychological account of the matter. But let me tell

> you that all the psychology in the world will leave the logical problem just where it was. ... You may say that evolution accounts for the thing. I don't doubt that it is evolution. But as for explaining evolution by chance, there has not been time enough.

It is true enough that guesses are not random, or completely at the mercy of chance (but neither is evolution). Guesses are blind. Even this thesis, which says little more than that what is known implies nothing about what is not known, is too much for many knowledgeable people, who are often displeased to be told that the bright ideas that they have made such efforts to produce are no more than blind (though perhaps brilliant) guesses. Inexplicably they value the weight of their entrenched learning more than the sparkle of their unconstrained imagination. (Einstein 1931, p. 97, thought differently, though the passage is open to other interpretations.) New hypotheses may well be guesses, the objectors say, but they are not *mere* guesses; they are *informed* or *educated* guesses. Quine put it this way: 'Jumping to conclusions is our way of daily life. Jumping to reasonable conclusions is the busy scientist's way ... ' (1986, p. 332). But informed guesses may also be blind. What informs informed guesses are earlier guesses, no less blind, that have survived the critical investigation to which they have been exposed. That is, they are guesses that are informed by what is conjecturally known, but blind to what is unknown (that is, blind to what goes beyond earlier guesses). As Campbell declared wisely (1974, p. 422): 'In going beyond what is already known, one cannot but go blindly. If one can go wisely, this indicates already achieved wisdom of some general sort.' Some guesses may indeed turn out, on examination, to be implied by what is already known, and accordingly not to advance our objective knowledge. Even here it is a case of the blind leading the blind (Miller 2005b, p. 79).

Guesswork has no rational component. As Peirce stated boldly in the passage quoted in § 3 above (p. 7): 'No reason whatsoever can be given for it[,] ... and it needs no reason since it merely offers suggestions.' Discovery, whether scientific or unscientific, is rational only to the extent that it uses reasoning to expose the errors in these suggestions. Popper, at that time knowing little about Peirce's philosophy, said much the same in 'Back to the Presocratics' (1958), § XII: 'There is only one element of rationality in our attempts to know the world: it is the critical examination of our theories. These theories themselves are guesswork.' This fact too engenders much disquiet. Bernays (1974), § 14, for example, having quoted the same words, added that 'it seems that ... [in these words] there is the hidden assumption that rationality must be *knowing*', and called for an extension of the role of reason beyond the critical process. In Bernays's paper I can find no substantial argument against the idea that guesswork involves no reasoning, and this is surely what Popper had in mind, Popper's reply was respectful, but he repudiated the assumption attributed to him, and conceded only that conjectures anticipate rational criticism — which has a purpose only if the target is held conjecturally (1974b, § 28.IV).

That conjectures and refutations are intricately intermingled is not, I suppose, in dispute. Where the critical rationalist position, that pure guesswork contains no element of reasoning, differs from the popular view may be only with regard to the level at which succeeding conjectures are analysed and severalized. Unconscious conjectures, condemned in summary justice, and eliminated from contention without ever being fully formulated, will be invisible at the stage at which pencil is put to paper. On this point, see Popper's remark about the ease with which Russell evidently composed his manuscripts (1978, p. 245; 1994, p. 21), his anecdotal report about Einstein's method of working (*ibidem*), and the claim that it was the 'consciously critical attitude [that] ... made it possible for Einstein to reject, quickly, hundreds of hypotheses as inadequate before examining one or another hypothesis more carefully, if it appeared to be able to stand up to more serious criticism' (1966, § XXI). A neglect of what is never written down is the only excuse (apart from an unfamiliarity with mathematical work) for the failure to recognize that in 'new mathematical proofs there is first the intuitive and imaginative idea of how the proof might run; and then comes the critical testing of the various steps of the proof — a critical revision which, more often than not, reveals that the proof is not valid' (Popper 1974b, § 28.IV).

5 Exaggerated deductivism

Critical rationalism is sometimes known more briefly as *deductivism*, plainly not a bad name for a doctrine that holds that all genuine inferences are deductive. I have, however, shunned the term here, since several authors have extended the hegemony of deductive logic even further, and have construed as products of deductive reasoning even the guesses from which, according to critical rationalists, the growth of knowledge must begin. According to Worrall (1995, p. 91):

> The fact is that scientists don't simply guess their theories; they don't make 'bold' Popperian conjectures. Instead they arrive at their theories in a way that, while it no doubt involves intuition and creativity, can nonetheless be reconstructed as a systematic and logical argument based on previous successes in science and parts of 'background knowledge' taken as premises. ... [But] even in the absence of a general analysis of theory-construction, it is not difficult to show by examining the *details of particular historical episodes* that such an analysis must exist.

Zahar (2007), Chapter VI, § (H), and Musgrave (2009), pp. 218–222, have also proposed what are represented as deductivist *heuristics* or logics of discovery. There seems to be agreement that only at the psychological level can discoveries be made in this way, since the deductive consequences of a hypothesis, viewed objectively, are already present within it. Deduction is not creative in the sense of § 0 above. In his exposure of the pretensions of syllogistic reasoning Mill rightly dismissed the attempt 'to attach any serious scientific value to such a mere salvo as the distinction drawn between being involved *by implication* in the premises, and being directly asserted in them' (1843, Book II, Chapter III, § 2). But we need not concede much to the advocates of deductive heuristics. It is unusual, to say the least, for a surprising conclusion to be uncovered by the process of deduction. What is more common is that the conclusion (or its negation) is first guessed to be a theorem, and then proved to be a theorem. A conclusion that typically turns up unexpectedly in the course of a deduction or calculation is the identity $0 = 0$.

It is a mystery to me, indeed, what the import of Worrall's thesis is. There can be no doubt that previously unformulated hypotheses can sometimes be incorporated into a deductive network composed of 'previous successes in science and parts of "background knowledge" '. This happens more than sometimes in modern mathematics (most of which can be assimilated into axiomatic set theory). But it cannot be the norm, since the deductive network (or one of its ancestors) must have been one of those ' "bold" Popperian conjectures' that have escaped Worrall's attention. There can be no doubt either, as we have just seen, that the work needed to fit a hypothesis into the 'structure of scientific doctrines [that] is already in existence' (Popper 1934, preface) is more 'trial and error, experimentation, guesswork' (Halmos 1985, p. 321) than 'systematic and logical argument'. The earlier doctrines control the later ones much as the banks of a river control the river's flow. They do not direct it, but they do redirect it. I do not deny the historical and scientific value of sorting out connections in this manner, but I resist the conclusion that the deductive organization of the finished article reveals the logic of its discovery.

The educational cult known as *critical thinking*, or *informal logic*, is another activity where an excessive effort is often made to impose a deductive structure on diffuse segments of conversation and writing. The main task allotted to students of 'critical thinking' (I feel that the name must be permanently confined within deprecatory scare quotes) is the identification and repair of what are regarded as bad arguments, which often means the identification of suppressed or omitted premises that are able to restore deductive validity. It is rarely recognized that the suspect passages may not be, objectively considered, arguments at all, even bad ones, despite their containing such markers as *if*, *so*, and *because*. Since the supposed conclusions are admitted to be badly disconnected from the supposed premises, it must be better to regard these conclusions as simple conjectures, and the argumentative flavour, if any, to be given instead by such words as *unless*, *but*, and *despite*, which indicate that there are reservations afoot and negative considerations at play. From an objective point of view, 'critical thinking' is therefore triply ill conceived (Miller 2005a). In the first place, it wrongly construes conjectures as the conclusions of enthymemes; in the second place, it directs critical attention away from what a conjecture says towards the spurious argument that is alleged to be mustered in its support; and in the third place it creates the impression that if only that argument were made deductively valid, all would be well. In brief, 'critical thinking' mistakes both the purpose and the power of criticism. After all, the only function of argumentation (except, conceivably, in mathematics) is to help us to work out what is true (or worthy or wise or prudent) and what is not. An invalid argument that seeks to justify its conclusion, however provocative, deserves to be ignored, since it tells us nothing about what we want to know, the truth value of the conclusion. Even if the other premises are not in dispute, the 'missing premise' that enforces validity gives no guidance, since the truth value of the conclusion is within our grasp only if this new premise is apprehended as true. Musgrave is surely right that '[n]ot every [invalid] argument should be counted as having a missing premise' (*ibidem*, p. 206). But only if the fruitless task of criticizing arguments is conflated with the fruitful task of criticizing their conclusions is he right that '[d]eductive logic will be deprived of any critical function' if all invalid arguments are interpreted as enthymemes.

6 Enter machines

In § 3 above I asked that we should use the term *inference* only for those transitions to new hypotheses that are constrained in some way by rules, and acknowledge that other transitions are best regarded as guesswork (p. 8). I have been careful not to say that non-demonstrative (inductive or abductive) rules of inference cannot be formulated, since it is undeniable that they can be. Equally easily formulated are *counterinductive* rules, which permit us to infer from a report that all encountered instances of X have been followed by Z to the conclusion that no future instances of X will be followed by Z. An agent could of course allow himself to be guided by such inductive rules, and a machine could be designed to work according to them. We need to inquire into the question whether the feasibility of such induction machines, as they may be called, gives the lie to Popper's view that induction exists neither logically nor psychologically. For according to Gillies (1996, p. 53), reporting on 'advances in machine learning', this view 'can no longer be maintained in the light of programs such as ID3 or GOLEM which do make inductive inferences based on many observations and have become a part of scientific procedure'.

Without denying the ingenuity that doubtless went into the design of these programs, and others, we ought, I think, to adopt a cautious view of the methodological import of such claims. In the first place, as Tamburrini has argued in his careful study of this question (2006), p. 268:

> AI investigations on learning systems do not compel one to relinquish Popper's radical scepticism towards induction. A proper understanding of both learning-theoretic and machine learning results does not require any appeal to alleged principles of induction, which are supposed to provide partial justification for hypotheses that are effectively generated on the basis of available data by computational agents.

He goes on to say that 'the intelligent behaviour exhibited by learning systems can be properly accounted for in terms of trial and error-correction processes'. If something that may be called induction is being used, therefore, it is not regulative in the sense of § 0 above. Secondly we should notice an unavoidable feature of all approaches that introduce rules for generating hypotheses, or inferring conclusions: that is, that the hypothesis that the rule is appropriate to the situation in which it is adopted does not fall within the scope of the rule, and therefore, if not blindly guessed, must be generated or inferred by some other rule. I have noted elsewhere that the adoption of a rule for making decisions does not insulate the decision to be made from the uncertainty inherent in the quandary that makes the decision necessary. The *outcome* of the decision remains uncertain, of course, but that is not what I mean. For no less uncertain is the advisability of abiding by the rule. It is almost an axiom of Bayesian philosophy, for example, that good decision making is decision making according to Bernoulli's rule of maximizing expected utility. Yet Bernoulli's rule is not an incontestable 'rule of rationality', especially if the probabilities involved are subjective, and the decision to be guided by it can be underwritten only by the wholly conjectural judgement that it is an appropriate rule to use (Miller 1998, § 1).

Quite a bit of trial and error activity is involved along the way, but the results delivered by ID3, which is 'concerned to induce classification rules' (Gillies, *ibidem*, p. 33), and GOLEM, which is designed to induce generalizations (pp. 41–44), are not deductively implied by the input data. These programs (and others) do indeed make inductive inferences. Each program incorporates a rule of inference, and it is reported that the inferences that have been licensed by the rule are good inferences in some agreed sense. This is not to say that the conclusions have all been true, or accurate, but (I suppose) only that they have for the most part been successful. So far there is no real problem. But if there is a hypothesis that the rule itself is a good rule that will continue, in the future and in new areas, to license good inferences, it has not been obtained by using the rule. Hume's argument is not so easily bypassed. If there is no such hypothesis, then the successes chalked up by the rules are an interesting curiosity of history, nothing more.

The possibility of induction machines that are successful when the conditions are right was explained by Popper himself at the end of § v of (1957a), and in more detail in § 13, of Part II of (1983). Popper's machine generates a data stream (a succession of balls of different materials and colours) and uses what he calls the *simple inductive rule* (sometimes called the *straight rule*) to infer, from the observed frequencies of various events, the probabilities of their occurrence, and universal generalizations if there are any valid ones. For example: 'It may discover, in this way, that the probability of a steel–copper pair to be followed by a copper event, or by a steel–copper pair, or by any triplet except a steel–steel–copper triplet, is zero . . . . [T]he machine has discovered the "law" that steel events tend to occur . . . in a succession of exactly three.' Popper notes that it is assumed in the machine's construction that the world to be investigated is such that '*the simple inductive rule can be successfully applied to it*'. As we know from the game *Red or Blue* (explained in *ibidem*, Part II, § 8), this condition is not a trivial one. Popper concludes:

> I never said that . . . we cannot successfully use the simple inductive rule — if the objective conditions are appropriate. But I do assert that we cannot find out by induction whether the objective conditions are appropriate for using the simple inductive rule. . . . the architects of the machine . . . must decide what constitutes its 'world'; what things are to be its individual events; *what constitutes a repetition.*

It is of course possible to build a machine with the function of learning about the world but without any idea of the conditions under which it is supposed to work. It is impossible to evaluate the effectiveness of such a machine, unless it happens to be successful under all conditions. Induction machines are known not be like that. If the claim to have a successful machine is to be worth considering, it is therefore essential that the designer of the machine specify its range of application. Popper's point is that the induction machine itself cannot help us to discover the conditions under which it works (though it might help us to discover conditions under which it crashes). Behind every successful induction machine or rule of inductive inference there is a wild conjecture. The more precise we are about the necessary and sufficient conditions for successful operation, the closer we are to a deductive explanation of what the machine does.

Tamburrini made much the same diagnosis of ID3: if the '*presuppositions* ... or biases embedded in ID3 proper (which determine both the language for expressing concepts and the construction of decision trees) ... can be suitably stated in declarative form, then a concept learning algorithm such as ID3 can be redescribed as a theorem prover' (*ibidem*, § 4). To this Gillies had two responses (2009, p. 107). The first response, voicing unspecific doubt that ID3 can in practice be so redescribed, may be ignored. The second response was to assert that ID3,

> redescribed as a theorem prover, would be much more complicated than the original ID3 presented as an inductive learning system. Why should we introduce all this unnecessary complication, which would never be adopted in practice? [If we] allow the introduction of inductive rules of inference, we get simple computer induction systems which are successful in practice. ... [If we] allow only deductive rules .... we are forced to try to transform these systems into equivalent theorem provers .... This is a difficult, probably hopeless, task which adds complexity with no practical gain.

Such a response might be expected from an engineer, concerned only with a machine's efficiency, or from an accountant, concerned only with costs and benefits. Simon (1973) too seems interested much less in epistemological issues than in the relative efficiency of various methods of processing data. A more philosophical response would be to ask why ID3 (or any other induction machine) is successful on the occasions on which it is successful. Is there something objective about the world that explains the machine's success, or is the success an artefact of the machine's design and of the way in which it is applied? It is undeniable that the phenomenal world is partly regular and partly irregular, and that one of the main tasks of science is to identify regularities lying below the surface. If workers in AI have discovered even deeper and more general regularities that explain the success attributed to learning machines, they ought to let the rest of us know.

7 The JSM method

JSM reasoning is a method of moving automatically from more or less disorganized data to an appropriate causal explanatory theory. In my brief remarks here I shall rely on the excellent summary of Burch (2000), which describes the JSM method as a method of *data fusion*, and to some extent on the more technical material in Finn (2011) and in Anshakov, Finn, & Vinogradov (2005). A detailed exposition is not needed for the simple philosophical points I wish to make.

The use of the JSM method begins with ill-defined and incomplete data about a well-defined field of entities with respect to a number of (monadic) properties. Typically, for a property P there is information in the database that some of the entities under study possess P and that others do not possess P. For other entities there is no information, and for still other entities there is conflicting information: the database says that they possess both P and $\neg P$. The first task, as I understand it, is to amplify and to correct the initial information, by extrapolating any limited generalizations that hold, and by exploiting analogies. The method proceeds to bring order to the database by what seems to be a method of trial and error, gradually plugging gaps and negotiating gluts. At some point, the procedure comes to a halt, and nothing new emerges. This marks the beginning of what Finn calls the abductive stage of the automated inquiry. The plan is to find, for any P of interest, what should be conjectured to be a cause of P, using a method akin to Mill's method of agreement: all possible causal hypotheses are considered, and the one that fits best the description of the *maximal cause* is accepted as the explanation of P.

In the Conclusion to his (2000), Burch says of the JSM method that it 'can produce useful results on databases that contain only small amounts of information'. In Finn's words (2002, pp. 407f.), the method 'is capable of solving problems of a definite type which demand the use of automated reasoning ... [and] cannot be solved by human persons in real time. ... [It is] a synthesis of cognitive procedures — induction, analogy and abduction ... in the sense of C. S. Peirce — ... [that has been] tested experimentally in different subject domains (pharmacology, biochemistry, technical diagnostics, and sociology)'. As I understand the matter, some of these successful tests have used the JSM method in order to determine causal connections in areas that are independently well understood. But Popper's celebrated thesis that confirmation, if uncritically sought, is usually easy to find, should have warned us not to be too impressed by successes of this kind. I should like to learn more about domains where the method has been unable to unearth any causal connections, and to learn by what independent criteria it is decided whether such a result is a success or a failure. I should especially like to learn about the performance of the JSM method in unpromising cases, such as the astrological data assembled by Gauquelin and others that appear to show dependencies between the times of birth of eminent people and the fields in which they achieved eminence (Gauquelin 1988; Eysenck & Nias 1982).

Above all I should like to learn what are the conditions under which the JSM method claims to be effective. Finn lists a number of syntactic '[c]onditions of applicability for the JSM Method of AHG [automatic hypothesis generation]' (*ibidem*, p. 413). But, as explained in the previous section, if the worth of the JSM method is to be empirically evaluated, we do need rather more.

Finn, like Simon (1973) and Michalski (1983), pp. 87f. (quoted by Tamburrini *ibidem* p. 268), and many others, thinks that the philosophical problem of induction needs a radical facelift: 'the problem of induction as related to universal theories ... should be replaced by the problem of adequacy of formalized heuristics in IS [Intelligent Systems] for the corresponding classes of problems' (2002, p. 408). If this replacement problem is treated as one of pure engineering, then we may allow that some steps have been made towards its solution. But if 'the problem of adequacy of formalized heuristics in IS' is a philosophical problem about how we can come to obtain knowledge of the world, then I do not see that any advance has been achieved. Nor do I understand why Hume's problem, which is not solved by being replaced, should be replaced; that is, why it should be allowed to drop out of sight unsolved, as has been recommended even by philosophers (as we saw in § 1). One may of course live without a solution, that is, without a theory of rationality. But I do not know what those who have abandoned the search for a theory of rationality mean by such logical expressions as 'plausible reasoning' (Finn 2011, Appendix 4).

Portraits of Mill, Peirce, and Popper, the heroes of the JSM campaign, decorate the front board of Finn's book (2011). I have tried to indicate in what way Peirce's and Popper's insights have not been fully taken advantage of. As for Mill, this passage from Chapter II of *On Liberty* (1859) offers a wiser philosophy of knowledge than does the whole of *A System of Logic* (1843).

> The beliefs which we have most warrant for, have no safeguard to rest on, but a standing invitation to the whole world to prove them unfounded. If the challenge is not accepted, or is accepted and the attempt fails, we are far enough from certainty still; but we have done the best that the existing state of human reason admits of; we have neglected nothing that could give the truth a chance of reaching us: if the lists are kept open, we may hope that if there be a better truth, it will be found when the human mind is capable of receiving it; and in the meantime we may rely on having attained such approach to truth, as is possible in our own day. This is the amount of certainty attainable by a fallible being, and this the sole way of attaining it.

References for Part II

Anshakov, O. M., Finn, V. K., & Vinogradov, D. W. (2005). 'Logical Means For Plausible Reasoning of JSM-Type'. On line at `http://www.ifispan.waw.pl/studialogica/AnshakovFinnVinogradov.pdf/`. Accessed 24.v.2011.

Bernays, P. I. (1974). 'Concerning Rationality'. In Schilpp (1974), pp. 597–605.

Burch, R. W. (2000). 'Semeiotic Data Fusion'. *Proceedings of the Third International Conference on Data Fusion.* Volume II, pp. 11–16. Paris: ISIF. Reprinted as Appendix 5, pp. 437–444, of Finn (2011). On line at `http://www.student.nada.kth./se/~tessy/RobertBurch.pdf/`. Accessed 24.v.2011.

Campbell, D. T. (1974). 'Evolutionary Epistemology'. In Schilpp (1974), pp. 413–463.

Deutsch, D. (2011). *The Beginning of Infinity. Explanations that Transform the World.* London: Allen Lane (The Penguin Press).

Einstein, A, (1931). *Einstein on Cosmic Religion and Other Opinions and Aphorisms.* New York: Covici-Friede.

Eysenck, H. J. & Nias, D. K. B. (1982). *Astrology: Science or Superstition?.* London: Maurice Temple Smith.

Finn, V. K.(2002). 'The Use of Induction in Plausible Reasoning in Intelligent Systems'. Abstracts, Karl Popper 2002, p. 19. Vienna: Karl Popper Institute. References are to the reprint as Appendix 4 of Finn (2011).

——— (2011). *Artificial Intelligence. Methodology, Applications, Philosophy.* Moscow: Krasand/URSS.

Gauquelin, M. (1988). *Written in the Stars.* Wellingborough: Aquarian Press.

Gillies, D. A. (1996). *Artificial Intelligence and Scientific Method.* Oxford: Oxford University Press.

——— (2009). 'Problem-Solving and the Problem of Induction'. In Z. Parusniková & R. S. Cohen, editors (2009), pp. 103–115. *Rethinking Popper.* Springer Science + Business Media B. V.

Halmos, P. R. (1985). *I Want to be a Mathematician. An Automathography.* New York: Springer-Verlag.

Keuth, H., editor (1998). *Karl Popper. Logik der Forschung.* KLASSIKER AUSLEGEN **12**. Berlin: Akademie Verlag.

Michalski, R. S. (1983). 'A Theory and Methodology of Inductive Learning'. In R. S. Michalski, J. Carbonell, & T. M. Mitchell, editors (1983), pp. 83–134. *Machine Learning, An Artificial Intelligence Approach.* Berlin: Springer Verlag.

Mill, J. S. (1843). *A System of Logic, Ratiocinative and Inductive.* London, New York, & Bombay: Longmans, Green & Co. Ltd.

——— (1859). *On Liberty.* London: J. W. Parker & Son.

Miller, D. W. (1994). *Critical Rationalism. A Restatement and Defence.* Chicago & La Salle IL: Open Court.

——— (1998). 'On Methodological Proposals'. In Keuth (1998), pp. 67–81.

——— (2005a). 'Do We Reason When We Think We Reason, or Do We Think?'. *Learning for Democracy* **1**, 3, October 2005, pp. 57–71. On line at `http://go.warwick.ac.uk/dwmiller/lfd−.pdf`.

——— (2005b). 'A Reply to Trudy Govier and Philip Adey'. *Ibidem*, pp. 78–80.

——— (2006). *Out of Error. Further Essays on Critical Rationalism.* Aldershot & Burlington VT: Ashgate.

——— (2007a). 'El modo único para aprender'. *Estudios de Filosofía* (Antioquia), 36, agosto 2007, pp. 25–39. On line at `http://www.scielo.org.co/pdf/ef/n36/n36a03.pdf/`.

Musgrave, A. E. (2009). 'Popper and Hypothetico-Deductivism'. In S. Hartmann, editor (2009), pp. 205–234. *Handbook of the History of Logic.* Volume 10: *Inductive Logic.* Amsterdam: Elsevier B. V. On line at `http://stephanhartmann.org/HHL10_Musgrave.pdf/`. Accessed 5.vi.2011.

Peirce, C. S. S. (1903). 'Lectures on Pragmatism'. In C. Hartshorne & P. Weiss, editors (1934–1935), pp. 11–131. *Collected Papers of Charles Sanders Peirce*, Volume V. Cambridge MA: Harvard University Press.

Popper, K. R. (1934). *Logik der Forschung.* Vienna: Julius Springer Verlag. Expanded English translation 1959. *The Logic of Scientific Discovery.* London: Hutchinson Educational. Repaginated Classics edition 2002. Abingdon: Routledge.

——— (1945). *The Open Society and Its Enemies.* London: George Routledge & Sons. 5th edition 1966. London: Routledge & Kegan Paul. Repaginated Classics edition 2003. Abingdon: Routledge.

——— (1957a). 'Philosophy of Science: A Personal Report'. In C. A. Mace, editor (1957), pp. 155–191. *British Philosophy in the Mid-Century.* London: George Allen & Unwin Ltd. Reprinted as Chapter 1 of Popper (1963).

——— (1957b). 'The Aim of Science'. *Ratio* **1**, 1, pp. 24–35. Reprinted also as Chapter 5 of Popper (1972), and as § 15 of Part I of Popper (1983).

——— (1963). *Conjectures and Refutations.* London: Routledge & Kegan Paul. 5th edition 1989. Repaginated Classics edition 2002. Abingdon: Routledge.

——— (1966) *Of Clouds and Clocks: An Approach to the Problem of Rationality and the Freedom of Man.* St Louis MO: Washington University Press. Reprinted as Chapter 6 of Popper (1972).

——— (1972). *Objective Knowledge. An Evolutionary Approach.* Oxford: Clarendon Press. 2nd edition 1979.

——— (1974b). 'Replies to My Critics'. In Schilpp (1974), pp. 961–1197.

——— (1978). 'Natural Selection and the Emergence of Mind'. *Dialectica* **32**, 3–4, pp. 339–355. References are to the abridged version in D. W. Miller, editor (1985). *Popper Selections.* Princeton NJ: Princeton University Press.

——— (1983). *Realism and the Aim of Science.* Edited by W. W. Bartley III. London: Hutchinson Educational. Current edition. Abingdon: Routledge.

——— (1994). *Knowledge and the Body-Mind Problem.* Edited by M. A. Notturno. London & New York: Routledge.

——— (1998). *The World of Parmenides. Essays on the Presocratic Enlightenment.* Edited by A. F. Petersen. London & New York: Routledge.

Quine, W. V. (1986). 'The Sensory Support of Science'. *Discursos* (Granada). Reprinted as Chapter 25 of W. V. Quine (2008). *Confessions of a Confirmed Extensionalist.* Edited by D. Føllesdal & D. B. Quine. Cambridge MA: Harvard University Press.

Schilpp, P. A., editor (1974). *The Philosophy of Karl Popper.* The Library of Living Philosophers, Volume 14. La Salle IL: Open Court Publishing Co.

Simon, H. A. (1973). 'Does Scientific Discovery Have a Logic?'. *Philosophy of Science* **40**, 4, pp. 471–480. Reprinted as Chapter 11 of Keuth (1998).

Tamburrini, G. (2006). 'Artificial Intelligence and Popper's Solution to the Problem of Induction'. In I. C. Jarvie, K. M. Milford, & D. W. Miller, editors (2006b), pp. 265–284. *Karl Popper: A Centenary Assessment.* Volume II: *Metaphysics and Epistemology.* Aldershot & Burlington VT: Ashgate.

Worrall, J. (1995). ' "Revolution in Permanence": Popper on Theory-Change in Science'. In A. O'Hear, editor (1995). *Karl Popper: Philosophy and Problems*, pp. 75–102. Royal Institute of Philosophy Supplement **39**. Cambridge & elsewhere: Cambridge University Press.

Zahar, E. G. (2007). *Why Science Needs Metaphysics.* Chicago IL: Open Court.