Some Hard Questions for Critical Rationalism

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ABSTRACT

'What distinguishes science from all other human endeavours is that the accounts of the world that our best, mature sciences deliver are strongly supported by evidence and this evidence gives us the strongest reason to believe them.' That anyway is what is said at the beginning of the advertisement for a conference on induction at a celebrated British seat of learning in 2007. It shows how much critical rationalists still have to do to make known the message of *Logik der Forschung* concerning what empirical evidence is able to do and what it does.

This paper will focus not on these tasks of popularization faced by critical rationalists, but on some logical problems internal to critical rationalism. Although we are rightly proud of having the only house in the neighbourhood that is logically watertight, we should be aware that not everything inside is in impeccable order. There are criticisms that have not yet been adequately met, and questions that have not yet been adequately answered. Each of the six difficulties to be discussed arises from Popper's exemplary solutions to the problems of demarcation and induction. They concern the management of contradictions; approximation to truth; the corroboration of already falsified hypotheses; decision making under uncertainty; the role of evidence in the law; and the representation of logical content. In none of these areas does critical rationalism yet offer, to my mind, an account comparable in clarity to its solutions to the problems of demarcation and induction.

This is a personal selection, and it is not suggested that there are not other hard questions ahead. In only one or two cases shall I offer anything like a solution.

0 Introduction

The business of this paper is to bring back into the public eye some of the outstanding logical and methodological problems that critical rationalism, or deductivism, the theory of knowledge,

and of science, that we owe to Karl Popper, has been inclined to neglect, and sometimes even to ignore. One of the hardest of these hard problems (not one of which is treated at any length by Keuth 2000/2005, a recent comprehensive text) could appropriately be called *the problem of outstanding problems.* As I hope will become sufficiently clear, there need be nothing intellectually dishonest about sometimes setting hard problems aside for a while. Inattention to a problem is a conjecture about its importance, and not all hard problems are important. But a conjecture that a problem is unimportant is not a solution, however provisional, and like all conjectures it must be prepared to confront hostile cross-examination. The best I can hope for in this paper is to shine some harsh light on the problems submitted to audit. It is too much to expect from me any brilliant solutions.

1   Demarcation and Induction

It will be taken for granted, however, that for the problem of demarcation, which Popper called 'the central problem ... of the theory of knowledge' (1930–1932, §1), and for the problem of induction, widely regarded as the fundamental problem of the philosophy of science, brilliant solutions already exist. I need to say a modicum here about these two problems, and their solutions, since they are still at times lamentably misunderstood. If the bulk of my paper is not to risk incomprehension, a statement of what is involved in these two problems, if not an explanation of their solutions, is necessary.

The problem of demarcation is solved much as Popper solved it. This commendation may surprise those who are acquainted with such titles as 'The Demise of the Demarcation Problem' (Laudan 1983) and 'The Degeneration of Popper's Theory of Demarcation' (Grünbaum 1989), or the writings of Kuhn (1962) and Lakatos (1973, 1974). But like many others, the authors of these criticisms thoroughly mistake the crucial philosophical task that Popper intended a criterion of demarcation to perform. Its task is not to 'distinguish scientific and non-scientific matters in a way which exhibits a surer epistemic warrant or evidential ground for science than for non-science', which Laudan (p. 118) lays down as a minimal condition for 'a *philosophically* significant demarcation', nor is it 'to explicate the paradigmatic usages of "scientific" ' (*ibidem*, p. 122). Questions of sureness, warrant, and grounds, are of interest principally to justification-ists who live in mighty dread that they may not be 'entitled to *believe* any scientific theories' (Papineau 2006, p. 63); questions of usage, classification, and status, are of interest principally to essentialists, to philosophers who prefer to pursue philosophy unphilosophically, and to educational administrators; and inevitably, of course, to lawyers. Contrary to what Grünbaum resolutely supposes, the problem of demarcation is only incidentally concerned to ratify the unscientific status of psychoanalytic theory (whatever psychoanalytic theory is taken to be), and contrary to what Lakatos likewise supposes, it is only incidentally concerned to ratify the scientific status of Newton's theory (whatever Newton's theory is taken to be). These classifications were incontrovertibly among Popper's aims, and it is important to know whether they have been accomplished. But the main problem of the theory of knowledge, at least for an empiricist, is quite different in kind: it is what Popper described as 'the critical analysis of the appeal to the authority of experience' (Popper 1934, §10). The popular misreadings mentioned are to no small extent excusable, I am sorry to say, since Popper himself often introduced the problem as 'an urgent personal problem ... [that] I did not first think of as a philosophical problem' (1974b, p. 976), and more often than not he extolled falsifiability as a criterion of what is scientific rather than of what is empirical (a misplacement of emphasis of which many of us have sometimes been guilty). But at a more scholarly level, emphatically justificationist, essentialist, and naturalistic misreadings are inexcusable. Popper's philosophy is potently and expressly opposed to all these fashionable tendencies, and to all visions of science as 'a body of knowledge' (1952, Chapter 11, note 6) exciting awe and deference and enjoying magisterial authority (1983, Part I, §33),

and he should have been given the credit for understanding the problem of demarcation in a manner that did not so sympathetically subscribe to these philosophical solecisms.

Not wisely overlooked is a passage that occurs at the end of Popper's analysis (1983, Part I, § 18) of parts of Freud's *The Interpretation of Dreams* (it differs only stylistically from a passage in the draft of the *Postscript* from the 1950s; see box 235, folder 15, in the Hoover Institution Archives):

> In the present context, it hardly matters whether or not I am right concerning the irrefutability of any of these three theories [those of Freud, Adler, and Marx]: here they serve merely as examples, as illustrations. For my purpose is to show that my 'problem of demarcation' was from the beginning the practical problem of assessing theories, and of judging their claims. It certainly was not a problem of classifying or distinguishing some subject matters called 'science' and 'metaphysics'. It was, rather, an urgent practical problem: under what conditions is a *critical appeal to experience* possible — one that could bear some fruit?

Here is a clear philosophical, even logical, problem: under what circumstances is an empirical investigation worth undertaking? The solution is also clear: since the formulation of a hypothesis, its acceptance as a candidate for the truth, must precede its consideration, the task of an empirical investigation cannot be to promote hypotheses, but only to demote them. Empiricism demands that a hypothesis be retained unless it clashes in an appropriate way with experience. An accepted hypothesis therefore remains accepted until it is rejected. No further action is needed (Miller 2006a, Chapter 4, § 1; 2007, § 1).

I hope that a logician may be forgiven for according this logical problem, and its solution, prominence over partly factual inquiries (whose interest I do not care to contest) concerning how, and to what extent, specific theories (such as psychoanalysis, Newtonian mechanics, and the doctrine of intelligent design) can be investigated empirically. Grünbaum, who has wisely not overlooked the above passage from Popper (1983), but has not read it equally wisely, insinuates that 'it insouciantly repudiates . . . [the] major, central tenet of his whole philosophy' (*ibidem*, p. 155), and goes on to demand, as if it mattered so much, 'what *other* theories for which scientificity has been wrongly claimed can be adduced to furnish such a vindication *vis-à-vis* the much older criterion of evidential support, which he wants to replace as unduly permissive?' (p. 156). I suggest that one need only browse awhile in the annals of pseudoscience. What is patently absent from Grünbaum's advocacy of a 'criterion of evidential support', and all its inductivist and justificationist congeners, is any explanation of the objective advantage that is imagined to accrue to a hypothesis when it is empirically supported. That you learn anything when you are told something that you already know is a dogma that Popper's solution to the problem of demarcation ruthlessly discards.

It is perhaps as well to add that, according to deductivism, well-practised theoretical science is, despite periodical difficulties in procuring decisive refutations, unrelentingly empirical. A few scientists may value only results flattering to their own reputations and careers, and even those who hope to discover a fragment of the truth — surely the majority — are disappointed if their pet hypotheses prove to be incorrect. Yet almost all investigators prefer their mistakes to be eliminated before it is too late. It is our desire to be well that motivates us to undergo potentially disturbing medical examinations.

The problem of induction too is solved much as Popper solved it. The principal deductivist insight here is that since ampliative (that is, inductive) inferences are invalid, their conclusions are no better supported than unsupported guesses, obtusely resistant to justification but, it is to be hoped, acutely susceptible to refutation. For 75 years the principal line of criticism has been to identify in scientific activity places where guesses have to be made, and either to declare these guesses to be the conclusions of 'inductive inferences' or to castigate them for being unjustified. Lo! — Popper's anti-justificationist deductivism is a failure. McGinn

(2002, p. 48), for example, who has a multitude of precursors (and even some successors, such as Velupillai 2008, p. 145), announces that '[w]e have to be able to infer that if a falsifying result has been found in a given experiment it will be found in future experiments; ... this is clearly an inductive inference'. This old criticism, and others like it, were fully answered long ago (Popper 1972, Chapter 1; Miller 1994, Chapter 2, §2d). Some opponents of deductivism, blind to the possibility of rationality without induction, nonetheless continue to repeat the same canards. The remark that '[it] is a feature of Popper's philosophy ... [that] when the going gets tough, induction is quietly called upon to help out' (Bird 1998, p. 180) is a topsy-turvy inversion of the real truth: that when the allegations of the inductivist authorities start to fall apart, a recourse to induction (or some similar procedure of justification) is quietly planted on deductivism and then dramatically exposed. This uncouth behaviour is fully in keeping with justification's resemblance to a heavy-duty narcotic (Miller *ibidem*, §3).

More tenacious and widespread, however, than these feeble innuendos is the accusation that deductivism, despite the great success it can chalk up in the domain of speculative explanatory hypotheses, is unable to make sense of the practical application of scientific results, what Salmon (1981) labelled the task of 'rational prediction'. This criticism too is mistaken, though again Popper's own writings on the subject, 'the pragmatic problem of induction' (1972, Chapter 1, §9) or 'the problem of tomorrow' (1983, Part I, §4.III), are in part to blame for the prevailing disarray. In my (2006a), Chapter 5, and especially in my (2006c) and (2008), I have tried to set matters straight in a way that conforms entirely to the spirit of critical rationalism. It must nonetheless be conceded that Popper's deductivism, in contrast to some forms of inductivism, and especially in contrast to Bayesianism, has no extensively developed account of what is usually called decision making under uncertainty and risk. This is one of the hard questions that I shall address in this paper (in §2.3 below), and one that I hope to say something positive about, though scarcely to solve without remainder.

2   Six Hard Questions

These two fundamental problems of the theory of knowledge, demarcation and induction (Popper 1930–1932), may have been comfortably settled, but not all is at peace. Other problems, quite as bewildering, are waiting to take their place. This is no more than we should expect if we take seriously the natural extension, illustrated below, of Popper's famous tetradic scheme (1972, Chapter 6, §XVIII, theses 7f.) of intellectual development. For '[e]ven when we solve a problem to universal satisfaction, we create, in solving it, many new problems over which we are bound to disagree' (Popper 1954c/1963, §4).

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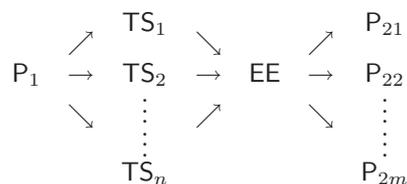
(initial problem → tentative solutions → error elimination → new problems)

In what follows I shall discuss six hard questions that critical rationalism needs to confront. I want to make it clear from the outset that some of these questions have been extensively and illuminatingly discussed by others, and I shall indeed refer to some of these contributions. But my acknowledgements will be only cursory. My purpose is to explain why deductivists need to exercise themselves about these difficulties, not why others have seen fit to do so.

2.0 Is progress possible if our knowledge is always contradictory?

A problem that is not simply a practical impasse typically takes the form of a conflict between a more or less well understood assumption and a hypothesis, or result, coming from elsewhere, that contradicts it. One of the two, we realize, has to give, but until something does give, it is unclear what has to give. In the meantime we have a contradiction on our hands, and we generate many more explicit contradictions when we try to resolve the difficulty by formulating alternative hypotheses in abundance (as recommended by Feyerabend 1963; earlier by Popper *ibidem*; and much earlier by Chamberlin 1890). But a contradictory set of statements, according to classical logic, is explosive: that is, it implies any statement that might be considered, and is therefore useless (a point stressed by Popper in 1940/1963, §1). It is difficult to understand how intellectual progress is feasible if we are forever in the midst of a swarm of mutually inconsistent tentative solutions. Lakatos's remark that 'some of the greatest scientific research programmes progressed on inconsistent foundations' (1974, §1(c)) somewhat understates our true predicament. No matter how many problems we solve, by rejecting all but one of the conflicting alternatives, what remains is an inconsistent jumble (or, more accurately, it is the unique, impeccably trim, set composed of all statements in the language, jumbled out of recognition by our lack of discernment). A rejected hypothesis, that is, 'comes back by the window just when you think you have got rid of him by the door' (Popper 1954b/1963). Although it cannot seriously be doubted that '[w]ithout contradictions, ...there would be no rational motive for changing our theories: there would be no intellectual progress' (Popper 1940/1963, §1), the difficulty is in seeing how there can be progress even when we do change our theories. This may be called *the problem of outstanding problems*.

It may seem at first sight that those who put their trust in induction and confirmation, and in the presumption that only justified knowledge is genuine knowledge, can easily brush aside this obstacle to progress. Coherence is surely a necessary condition for justification, even if it falls short of being a sufficient one, and what we know in the traditional sense cannot contradict anything else that we know. Even inductivists, however, will admit that in our attempts to find out what is what we sometimes have to stray into unknown territory, and it is here, at the boundary of unknown territory, that the problem under discussion can be expected to regroup its forces anew. A strategy of research that allows investigators to handle only consistent sets of hypotheses grievously diminishes the importance of the role that problems perform in our thinking. What is worse, it ignores the ubiquity of idealizations and approximations (which will be discussed in more detail in §2.1 below). From many of our more advanced hypotheses we can squeeze useful empirical information only by methods of approximation, and in most instances the information contradicts the hypothesis from which it has been squeezed.

It seems plain that what we usually do in order to avoid the explosiveness of contradictions is to treat the investigation of a hypothesis in isolation (consistent isolation, we hope) from much else that we know, even though this strategy drastically reduces, in unknown ways, the critical potential of our investigation. Critical rationalism cannot rest content with this spare logical description. An obvious alternative approach is to appeal to some system of paraconsistent logic, a generic title for subsystems of classical logic in which contradictions are not in every case explosive; that is, in which the law $A \wedge \neg A \vdash B$ of non-contradiction is not universally valid. Restricting our derivations to those licensed by some paraconsistent calculus — there exist many such calculi — need not jeopardize any attachment to the classical correspondence view of truth. Nor is it incumbent on the user of such a calculus to learn to think paraconsistently, if that is even possible. The expectation is rather that paraconsistent logic may enable us to paper over what looks like (but technically is not) a yawning discontinuity between consistent hypotheses, which can imply at most one of any pair $\{C, \neg C\}$ of mutually contradictory sentences, and the inconsistent theory $\bot$, which implies both members of every pair. What is needed for this

purpose is a study of the various algebras of deductive theories (that is, deductively closed sets of statements) generated by paraconsistent calculi. In Chapter 13 of my (2006a), I considered a somewhat primitive paraconsistent system, the direct dual of intuitionistic logic (in which the law $B \vdash A \lor \neg A$ of excluded middle, whose dual is the law of non-contradiction, is not universally valid). It is not as well known as it deserves to be that the variety of algebras (known as Brouwerian algebras) that correspond to this logic contains the algebras of deductive theories of all logical calculi in which the distributive law holds; for almost every extant paraconsistent calculus, therefore, the corresponding algebra of deductive theories is Brouwerian. A closer study of these algebras in this context seems to be imperative. But at this stage it is less than obvious what we can hope to learn that is applicable to the present problem.

2.1   To what truths do approximate truths approximate?

The problem just outlined — how can there be progress from one logically false hypothesis to another? — is an intensified form of a problem that is a good deal more familiar to deductivists, the problem of verisimilitude: how can there be progress from one false hypothesis (or theory) to another? In Chapter 10, § 3, of (1963), Popper proposed that false hypotheses may in principle be compared by *verisimilitude* (or *truthlikeness*); that is, by the extent to which one of them is closer to, or approximates better, in a well defined sense, the whole truth T, the class of all true statements in the language under consideration. Popper's own attempt to give substance to this idea of (comparative) verisimilitude was by no means a success, and it is now generally recognized that the task is obdurately technical; on which account, rather than because it is deemed inconsequential, most deductivists have excused themselves from pursuing the problem with any zeal. Much valuable work has meanwhile been accomplished by logicians with other leanings, such as Hilpinen (1976), Kuipers (1982), (1997), (2000), Part III, Mormann (2005), (2006), and Niiniluoto (1987), (1998). It is disappointing that most student texts — O'Hear (1980) and Keuth (2000/2005) are exceptions — do no more than sketch a proof of the insufficiency of Popper's definition, and then dismiss it. The truth is that, although there is plenty of work still to be done, we understand verisimilitude much better now than we understood it in 1974.

     My preferred approach to verisimilitude (*ibidem*, Chapter 10, § 4b; 2005), nicely developed by Mormann (2006), is to set out some transparent axioms for a distance function or pseudo-metric operation $\mathfrak{d}$ on pairs of hypotheses, and to define the verisimilitude $\mathfrak{v}(h)$ of h as $1 - \mathfrak{d}(h, \mathsf{T})$, where 1 is the maximum value of $\mathfrak{d}$ (it is, for example, the distance from the contradiction $\perp$ to the tautology $\top$). Since $\mathfrak{v}(h)$ evidently measures the degree to which the hypothesis h approximates T, this simple-minded theory of verisimilitude, even with its inevitable shortcomings, has some intuitive appeal. The question being raised here concerns the viability of using this geometrical apparatus to provide a deepened understanding not only of the verisimilitude $\mathfrak{v}(h)$ of a hypothesis h but also of what may be called its *degree of approximation to the truth*, or (for short) its *approximate truth* $\mathfrak{w}(h)$ or even its *degree of truth*. This idea is sometimes conflated with verisimilitude (as by Popper 1963, addendum 3, § 4), but the two ideas can readily be distinguished (Hilpinen *ibidem*, § I). A hypothesis has high verisimilitude only if it has great content, but it may be approximately true even if its content is meagre; if h is true, then $\mathfrak{w}(h) = 1$ independently of its content. Making adequate sense of approximate truth is of some importance, since there are countless false hypotheses proposed in science and technology that aim at truth but do not aim to get anywhere near the whole truth T. I say this without endorsing the prevalent structuralist doctrine (also called the 'semantic conception of theories') that contemporary scientific research rightly concerns itself almost exclusively with the building of models, and seldom with the development of true explanations. Koertge (2006)

is a most welcome criticism, from a predominantly deductivist perspective, of the principal methodological weaknesses of structuralism.

One way to explain $\mathfrak{w}$, following Swinburne (1973), p. 213, is in terms of $\mathfrak{v}$: the approximate truth $\mathfrak{w}(h)$ of h is its verisimilitude $\mathfrak{v}(h)$ per unit of content (Miller 1994, Chapter 10c). Another suggestion, due to Hilpinen *ibidem*, §II, (2), is that a hypothesis h is approximately true if and only if it is a logical consequence of a maximal theory close to T (in the material mode of speech: it is true in some possible world close to our world). The suggestion of Smith (1998, §2) and several others that the approximate truth of a hypothesis h is equivalent to the truth of 'approximately h', may sound innocent, but cannot routinely be extended to unaxiomatizable theories. The driving question is whether a more convincingly geometrical explanation than any of these is feasible, one that is less remote from practical everyday judgements. It would be valuable also if the scope of the principle of transmission of approximate truth (that is: a valid inference with approximately true premises has an approximately true conclusion), which is false in general, could be accurately delineated (Miller 1994, Chapter 10, §2). What seems unfortunately not to be possible is to extrapolate effectively from the single well-understood case of approximate truth, that is, the approximation of a hypothesized value of a quantity ϑ to the true value of ϑ (Miller ibidem, Chapter 11; 2006a, Chapter 11). In the general case, we may be tempted to say that $\mathfrak{w}(h)$ is a measure of the degree of closeness of the hypothesis h to some part of the whole truth (rather than it all); that is, to some true proposition (or theory) weaker than T. The immediate question is: which true proposition? The immediate difficulty is that, according to the proposed geometry, every false hypothesis h is a relatively small constant distance, namely $\mathfrak{d}(\mathsf{T}, \perp)$, from a true theory (namely our old friend its truth content $h \vee \mathsf{T}$). More discrimination is needed.

A possible way ahead, which I indicate with decided unease, is to go back to an old idea of Popper's (1979, appendix 2 (3)) that measures of content and verisimilitude may usefully be relativized to problems or problem situations. Popper's particular proposal concerning verisimilitude has been damagingly criticized by Mongin (1990), and I shall not resurrect it. Several authors, including Popper himself on one occasion (*ibidem*, final paragraph), in bids to rescue their definitions of verisimilitude and approximation to truth from the plague of language dependence, have resorted to similar strategies. For details and criticisms, see my 2006a, Chapter 11, §5. The present timid proposal is different, and assuredly does not countenance the possibility that $\mathfrak{w}(h)$ may depend on the language in which h is formulated. The idea, a booming echo of Kuipers's (1982) distinction between descriptive and theoretical truth, is that, unlike $\mathfrak{v}(h)$, which is a function of h and T alone, $\mathfrak{w}(h)$ depends also on the true proposition (or theory) that is taken to be the target for the hypothesis h in the given problem situation. A specific error may be unforgivable in one context, incidental and harmless in another. (This paragraph itself contains an example.) It is not denied that we may often find it hard to say what the target is at which a specific false h is aimed (Miller 1994, pp. 222f.). A greater worry is that, contrary to the hope expressed in my (2006a), Chapter 11, §5.1, it is no longer easy to keep logical and pragmatic issues clearly apart. But note that different problems, such as measurements, that pragmatically settle for different levels of approximation, may logically involve the same target.

There is evidently a long long way to go before we have a good working understanding of how some hypotheses can be closer to the truth than others. It is a logical question that critical rationalism cannot comfortably disregard.

2.2   Does a disproof require a death sentence?

One indefatigable criticism of deductivism, popularized by Kuhn (1962) and Lakatos (1974), sometimes strangely in tandem with the claim that empirical falsification is rarely encountered, or even impossible, is incorporated in the historical observation that falsifications of otherwise

successful hypotheses are often little attended to, and on occasion more or less ignored. A household example is the inconsistency of classical celestial mechanics with undisputed empirical reports of the perihelion of Mercury, but cases abound throughout the physical and biological sciences. It is perhaps a little hypocritical to parade such incidents as uncompelling empirical anomalies and also as compelling methodological counterexamples, but even the staunchest enemies of methodological naturalism have to admit that there is a question here for deductivists to ponder. As noted in § 2.0 above, intellectual life would grind to a halt if we tried to deal at the same time with all the difficulties confronting us. The situational logic of our responses to anomalies and near misses may be poorly specified, but the lesson to be learnt is that there is more at stake than truth, rather than that truth does not matter.

Popper was quite right, in his reply (1974b, § 12.III) to Lakatos (1974), to insist on a clear distinction between falsification and rejection (see also 1934, § 22). But the logical materials for a more forceful and less pragmatic answer had been available to him since the early 1960s. In 1971, when (1974b) was written, before the theory of verisimilitude had become suspect, he could have asserted boldly something that he barely hinted at, that the purpose of empirical testing is not only to falsify individual hypotheses, but also to discriminate among competing hypotheses; in particular, to falsify the default judgement that they are equally close to the truth. He did say things like this elsewhere, and he repeatedly emphasized that judgements of verisimilitude are unavoidably comparative. But what he never did explicitly was to connect verisimilitude, or approximate truth, adequately with the outcomes (both positive and negative) of empirical tests. In (1963), Chapter 10, § XII, he wrote

> I do not suggest that the explicit introduction of the idea of verisimilitude will lead to any changes in the theory of method. ...my theory of testability or corroboration by empirical tests is the proper methodological counterpart to this new metalogical idea. The only improvement is one of clarification.

and in a notorious later passage he suggested that '[although] the degree of corroboration of a theory ...cannot be interpreted simply as a measure of its verisimilitude, ...it can be taken as an indication of how its verisimilitude *appears* at the time' (1972, Chapter 2, § 33). This is an unfortunate edict. After all, the degree of corroboration of every falsified theory is -1 (a value from which there is no escape, unless the falsification is undone), and gives no indication, however dim, either of the theory's verisimilitude or of how well it approximates the truth. At the back of Popper's mind, no doubt, was the thought that a hypothesis that has failed some tests, but not failed them too badly, a hypothesis with some tested predictions definitely beyond the limits of experimental error, but not gravely wrong, will appear to be closer to the truth than a radically unsuccessful rival, even though both are falsified. The prime difficulty is that these 'appearances' of closeness to the truth can only be conjectures that, knowing as little as we know at present about verisimilitude and closeness to truth, are in no perspicuous way under the control of empirical evidence (that is, falsifiable).

The absence of a solution to this difficulty is no excuse for a retreat to instrumentalism, inductivism, or irrationalism, and should not deter us from looking for a more modest response to the undeniable fact that, as Kvasz puts it, 'not all cases of falsification are the same' (2004, p. 263). Granted the truth of the test statements involved, a falsified hypothesis is definitely false, but empirical tests ought to be competent to give us more information — negative information, inevitably — than this. It is hardly stretching usage unduly to say that a falsified hypotheses may be successful in later tests, and in that way be further corroborated, or fail them, and in that way be further falsified. To provide a succinct report of how a hypothesis has endured the regime of tests to which it has been submitted, we therefore appear to need a measure of degree of corroboration that 'assigns positive degree of corroboration ...even to refuted theories' (Lakatos 1974, note 122; 1978, p. 166) or even 'a notion of support in which grey

ravens can be said to support "All Ravens are Black" ' (Feyerabend 1975, pp. 158f.). There is some connection here, which deserves more investigation, with Keynes's discussion of the weight of arguments (1921, Part I, Chapter VI). Lakatos's own solution to the problem (1968a, § 32) succeeds only by pretending that the refutations were never obtained, while other treatments, such as that of Faust (2007), prematurely abandon classical logic. Let me conclude my remarks here by sketching an alternative proposal.

Let $\mathfrak{p}$ be a strictly positive numerical probability function defined on propositions. It is customary to use $\mathfrak{ct}(h) = 1 - \mathfrak{p}(h)$, the improbability of h, as a simple measure of the content of h. The content shared by the hypothesis h and the evidence e is therefore measured by $\mathfrak{ct}(h \vee e)$. Provided that the informal requirement that the evidence e be obtained in severe tests designed to eliminate the hypothesis h if it is false, rather than in a fact-collecting spree, is met, this function, which we may write as $\mathfrak{t}(h, e)$, gives a rough measure of the information that the tests, positive and negative, supply concerning h; or how thoroughly the tests reported in e have probed the hypothesis h, and how h valiantly has acquitted itself in the tests. For each h, the maximum value of $\mathfrak{t}(h, e)$ is $\mathfrak{ct}(h)$, which is achieved when e logically implies h. In this respect, and in several others, $\mathfrak{t}(h, e)$ resembles Popper's degree of corroboration $\mathfrak{C}(h, e)$ (1954a; 1959, appendix *ix, 9.2). Its minimum value, however, is not -1 but 0, which is achieved when the contents of e and h are disjoint; that is, when h and e are maximally independent of each other (in old-fangled terminology: when e and h are subcontraries). There exist, of course, other deliberate discrepancies between $\mathfrak{t}$ and $\mathfrak{C}$. Because the value of $\mathfrak{p}(e)$ never increases, but may decrease, when e is strengthened (that is, when more evidence is accumulated), the value of $\mathfrak{t}(h, e)$ never decreases, but may increase, when e is strengthened. It follows that $\mathfrak{t}(h, e)$ may exceed 0 when e contradicts h. Note also that if $\mathfrak{t}(h, e)$ is normalized so that it attains the same maximum value for every h, the result $\mathfrak{q}(h, e) = \mathfrak{t}(h, e)/\mathfrak{ct}(h) = \mathfrak{p}(\neg e, \neg h)$ is identical with the degree of *deductive dependence* of the hypothesis h on the evidence e defined by Miller & Popper 1986 (see also Miller 1994, Chapter 10.4c), which measures the proportion of the content of h that is included within the content of e. Since $\mathfrak{q}(h, e) = \mathfrak{q}(h, e \vee h)$, it may even be thought to measure the degree of approximation of h to e, so that if e is an appropriate true target proposition, as explained in § 2.1 above, $\mathfrak{q}(h, e)$ is a measure of the degree of approximation of h to the truth. Sketchy though this account is, it may encourage the hope that, notwithstanding the pessimism expressed two paragraphs ago, the overall performance of a hypothesis in empirical tests may offer a way of adjudging any claim it makes to be approximately true.

2.3   What do you do if you don't have knowledge?

To plan effectively our actions and reactions, we must know something — that is to say, we must conjecture something — about the future. But since even the most immaculately empirical of predictions can be directly falsified only when it is no longer a prediction, and gives no guidance about the future, a more theoretical, or anyway more general, approach to planning seems to be rationally compulsory. Few will disagree that scientific knowledge, precarious and fragmentary as much of it may be, plays here an indispensable part. Provided that there is no suggestion of illogicality, *scientifically informed decision making* (which needs no hyphen) is accordingly a better term for this field than *rational decision-making* (and much better than *rational-decision making*; see Miller 1994, Chapter 7, § 8). How can the scientific knowledge we possess be used to enrich our decisions, and how can we compensate for the lack of such knowledge?

Since critical rationalism incorporates much the most coherent theory of scientific knowledge available, it would be pleasant to report that there exists also a nicely articulated deductivist theory of practical decision making. That nothing quite like this is on display may be attributed to two failures: (a) the logical mistake of supposing that we persistently act 'on the

basis of' scientific hypotheses or laws, so that the practical problem of which course of action to follow can be reduced to the theoretical problem of which scientific hypothesis to prefer; and (b) the oversimplification of supposing that the deductivist explanatory scheme of situational logic can be converted into a straightforward rule of action: *Act in a way that is appropriate to your situation as you see it!* Misled by (a), many deductivists (including most of those adversely influenced by Lakatos 1968a, § 3.3, and 1974, § II) have rightly been unable to understand how the critical appraisal of the past performance of rival hypotheses can bear deductively on their comparative usefulness and applicability in the future, and have drifted back into a deadening and futile inductivism. But despite Popper's name for it, 'the pragmatic problem of induction', and his treatment of it in Chapter 1 of (1972), the problem of scientifically informed decision making is not really a version of the problem of induction. In applied science, and throughout the practical sphere, it is not empirical evidence, but theoretical science, that provides the critical ammunition; the recipients of the criticism are not scientific hypotheses, but individual designs, recipes, and proposals for action (Miller 1994, Chapter 2, § 2g; 2006a, Chapter 5, § 3; 2006c). An abundance of unrefuted scientific laws at our disposal therefore reduces rather than inflates the variety of courses of action open to us. The dominant aim of technology is, admittedly, to create devices and designs whose *modi operandi* are governed by explicitly stated low-level empirical generalizations. But these generalizations are earned by sustained trial and error (Michl 2006), not by deduction from pre-existing theoretical laws.

On account of (b), deductivists have not, in my judgement, paid sufficient attention to what is usually called decision making under uncertainty or risk. All decision making worthy of the name is, I need hardly say, made in conditions of chronic uncertainty. The question before us is not the question of whether the decisions the agent makes can be justified (however inconclusively), nor one of whether the available scientific knowledge is itself amenable to justification, but of how that knowledge can be most intelligently exploited. If it is so vast, and so restringent, that essentially only a single exit is permitted, then that has to be the way forward. This is situational logic in its simplest form. At the other extreme, if next to nothing is known (or too much), then trial and error is the only remaining option (Miller 2006b).

There are intermediate circumstances, between know-all and know-nothing, where more needs to be said. With a nod, but not too deferential a nod, at the standard terminology, I shall call the two situations now to be outlined *decision making under uncertainty* and *decision making under risk*. In the former, there is one proposed course of action that has been subjected to, and has survived, serious scientific criticism, but there remain others that have not been effectively criticized, or perhaps not properly criticized at all (it is this problem of decision making that is studied by Nordin 2006 in detail). Common sense recommends that in such a situation the canny agent follow the course of action that has been thoughtfully examined, and eschew the unexamined possibilities, even though no material objection has been levelled against them. In decision making under risk, in contrast, there are several courses of action that sometimes have been successful, and sometimes have been unsuccessful, and our scientific knowledge provides stable (that is to say, well-tested) statistical information about the relative frequencies of success of each option. Common sense here recommends that, other things being equal, the course of action with the greatest frequency of success in the past is the one to adopt in the future, and that the alternative courses of action are to be avoided. Does a deductivist underwrite, or abrogate, these judgements?

The question here posed, of how we make scientifically informed decisions in these situations where science does not tell us all that we should like to know, is perhaps the most difficult question confronting deductivism. In (2008) I have begun hesitantly the task of answering it.

2.4 Does a death sentence require a proof?

Deductivists ought to be more agitated than they are about the seemingly justificatory use of evidence and testimony in courts of law, and the various 'standards of proof' that are invoked in criminal prosecutions and civil actions. The original meaning of the verb 'to prove' may well have been 'to probe', but few lawyers regard the legal process as just a publicly conducted process of severe testing. Legal argument is venerated in much the way that the theory of confirmation is venerated by inductivists; it is credited with the power to make probable, on appropriate evidence, and even to establish, factual conclusions that go far beyond the evidence. All this appears to conflict with what deductivism says about the role of evidence, and about the impotence of arguments, even deductive ones, to generate any level of justification or 'good reasons' in favour of any conclusion (Miller 1994, Chapter 3; 2006a, pp. 128f.).

A deductive argument is incapable of justifying its conclusion (because the premises remain unjustified), but this should not matter in a court of law. The testimony of witnesses, the evidence of experts, and generalizations of common sense (often unstated) are what constitute the premises of a legal argument, supplemented, of course, with a statement of the relevant law or laws. Together, in a criminal trial, these premises may deductively imply a statement that the accused committed the offence in question. For the case to succeed, the court does not require justification (in any sense that interests traditional epistemology) of any of the premises mentioned; testimony and expert evidence are required to survive cross-examination; commonsense generalizations are taken for granted, though they may be contested and have to be replaced; and the law is given. It is therefore possible, I suggest, with some give-and-take, to understand legal proceedings, at least in criminal trials in the Anglo-American system, in an epistemologically pure light. My suggestion is indeed so obvious as to need little embellishment. It amounts to saying that it is not the responsibility of counsel for the prosecution to do the impossible and construct a proof (in any sense that interests traditional epistemology) of the guilt of the accused. Counsel's responsibility is to present a 'case': a deduction of the statement of guilt from premises that are sturdy enough to resist all the criticism that the defence can mount against them. As in science, so in the law, justification is replaced by survival of critical scrutiny.

I am aware that the picture just drawn is oversimplified to an extent that most lawyers will find insulting. I shall be told (perhaps only on payment of a fee) that the law is not simply given; it requires expensive interpretation. It will be pointed out that minor blemishes in a case are not always sufficient for it to be thrown out (but as we have seen in § 2.2, the same is true in science). An especially salient defect of my sketch is that it ignores all considerations of probability, and unrealistically insists on a deductive argument from premises to verdict, whereas the most that is ever demanded, even in a criminal trial, is 'proof beyond reasonable doubt'; that is, an argument whose conclusion is made highly probable by the premises, but not logically implied by them. I plead guilty to all these charges, and can offer in mitigation only the admission that I am conscious of how much work needs to be done if legal reasoning is to be brought into the safe harbour of deductive logic. Even if it is remote from deductivism in spirit, the work of Cohen (1977), Parts I and II, recently revived by Stein (2005), Chapter 2, § C, could well be relevant here. There are some far-reaching similarities between Cohen's Pascalian probabilities and the measure q of deductive dependence briefly mentioned in § 2.2 above. It may turn out that a verdict needs to be approximately true, given the truth of the premises, rather than be probable in the sense of the calculus of probability.

This therefore is the hard question that I put to all deductivists: can the proceedings of criminal and civil courts, in all reasonable jurisdictions, be so reconstructed in a deductivist framework that practising lawyers will both recognize and applaud the outcome? The question is a hard one if only because it requires that the reconstruction be carried out. I do not doubt

its significance. An often heard *obiter dictum* says that the purpose of a criminal trial is not to find out what happened but to find out whether the guilt of the accused can be proved. It would be splendid to have this judgement overturned.

2.5 For want of a nail . . .

Let h be a hypothesis and e an item of evidence. Of all the after-effects of the proof in Popper & Miller (1983) that, whatever other significance it may have, the probabilistic support $\mathfrak{s}(h, e) = \mathfrak{p}(h, e) - \mathfrak{p}(h)$ cannot be interpreted as inductive support, none was noisier than the rumpus over their identification of the *excess content* of h over e with the conditional proposition $e \rightarrow h$. The reader may be interested to consult some of the items in Rochefort-Maranda & Miller (2008), especially the contributions of Howson and the extraordinary intervention of Salmon (2005), pp. 207–209. This identification, which applies whether or not h logically implies e, was far from original, as acknowledged in Popper & Miller (1987), p. 579 (see also Miller 2006, pp. 201f.), and it had excited little adverse comment until it began to threaten the holy citadel of probabilistic inductive logic. Some arguments in its defence are mustered in Popper & Miller *ibidem*. They mention also a puzzling formal difficulty that arises from the use of the material conditional in this way, which has not been remarked on by other writers. It is this formal peculiarity that I wish to look at here.

In his (1963), Chapter 15, §1, text to note 8, and indeed as early as (1948), Popper drew attention to the fact that, in the logical calculus that is dual to intuitionistic logic, there is not always defined a conditional that satisfies the standard laws of modus ponens and conditional proof. Perhaps too hastily, he dismissed this calculus as 'an extremely weak system . . . of no use for drawing inferences although it may perhaps have some appeal for those who are specially interested in the construction of formal systems as such' (*ibidem*). Now the algebra of (finitely) axiomatizable and unaxiomatizable deductive theories of classical logic (and many other logical calculi) has the same formal structure as this 'extremely weak system' of dual-intuitionistic logic, as was noted near the end of §2.0 above. Popper & Miller *ibidem*, pp. 583f., made the trite observation that if **X** and **Y** are deductive theories that are not finitely axiomatizable then there may exist no conditional theory **X** → **Y**. In other words, there may not exist any theory that can be called the excess content of **Y** over **X**. The paragraph that follows, which may be omitted, gives a proof of a striking example: if **ZF** is Zermelo–Fraenkel set theory and **ZFC** is **ZF** supplemented with the axiom of choice AC, then there is no theory that can properly be called the excess content of the theory **ZFC** over the theory **ZF**.

The logical background is summarized in Miller (2005), pp. 18–21. It can be demonstrated, but will not be demonstrated here, that the conditional **X** → **Y** exists if and only if **X** ∧ ¬**X** ⊢ **Y** (where ¬**X** is a kind of non-classical negation of **X**, obeying the law of excluded middle but not the law of non-contradiction), and that this implies that **X** → **Y** exists whenever **X** is finitely axiomatizable. Let **ZF**, AC, and **ZFC** be as above, and let **AC** be the theory that consists of all the consequences of AC. It can be demonstrated also that, even when **ZF** is a consistent theory, its negation ¬**ZF** is identical with **L**, the trivial theory composed only of logical truths. This implies that **ZF** ∧ ¬**ZF** = **ZF**, and it is very well known that (unless it is inconsistent) **ZF** does not imply the axiom of choice AC. We may conclude that there exists no conditional **ZF** → **AC**, and thence that there exists no conditional **ZF** → **ZFC**.

We need to be clear that the theory **AC** is not a counterexample to this result. Because **ZF** and **AC** have in common many logical consequences (for example, the statement that every finite set can be well ordered) that are not logical truths, **AC** is not the weakest theory that, combined with **ZF**, is equivalent to **ZFC**. The result is nonetheless a little disturbing, especially as the failure of a theory to be finitely axiomatizable can sometimes be blandly redressed: a

recursively axiomatizable theory with no finite models can always be finitely axiomatized by rewriting it in a richer language (Kleene 1967).

Is this just a trifling and insignificant curiosity, or does it reveal a deeper malaise in the way we understand and apply deductive logic, and in the way that we manipulate logical contents? I hesitantly suggest that it may do so. Traditional logical calculi, which are designed to serve the remorseless aims of accumulation and consolidation and proof, rather than those of elimination and liquidation and disproof, are in some respects intransigently at odds with the uncompromisingly restless, we might even say dialectical, spirit of critical rationalism. In traditional calculi, the conditional operation $A \to B$, where it exists, inverts the operation of conjunction: conjoining $A \to B$ to A is the most economical method of increasing the content of A to that of $A \wedge B$. The inversion of disjunction, in the same sense, is performed by the *remainder* operation $A - B$, where it exists; disjoining $A - B$ is the most economical method of decreasing the content of B to that of $A \vee B$. (In classical logic, $A - B$ is equivalent to $A \wedge \neg B$ and $A \to B$ is equivalent to $\neg A \vee B$.) Where B implies A, things are simpler: conjoining $A \to B$ is the most economical method of increasing the content of A to that of B, while disjoining $A - B$ is the most economical method of decreasing the content of B to that of A.

It can be shown that the remainder $\mathbf{X} - \mathbf{Y}$ (which is sometimes properly stronger than $\mathbf{X} \wedge \neg\mathbf{Y}$) exists for any two deductive theories $\mathbf{X}, \mathbf{Y}$ based on classical logic. As in the case of statements, if $\mathbf{Y}$ implies $\mathbf{X}$, things are simpler. If there exists a most economical method of increasing the content of $\mathbf{X}$ to that of $\mathbf{Y}$, it is by conjoining $\mathbf{X} \to \mathbf{Y}$. In contrast, disjoining $\mathbf{X} - \mathbf{Y}$ is always the most economical method of decreasing the content of $\mathbf{Y}$ to that of $\mathbf{X}$.

Critical rationalists hold that the chief purpose of logic is not to make advances but to regulate retreats; deductive reasoning is used not to amplify content but to diminish it (Miller 2006, Chapter 4). Yet we all impose on deductive inferences a structure of premises and conclusion that belies this revisionist attitude. I have attempted here to hint at the dire inadequacy of this mode of presentation (see also *ibidem*, Chapter 13). The deductive-reductive logic of Łukowksi (2002) may be a first step in the right direction.

3   Conclusion

In this paper I have drawn renewed attention to six hard questions of a logical or methodological character that are prompted by Popper's imperishable deductivist solutions to the problems of demarcation and induction. Unless I am badly mistaken, deductivism needs more elaborate answers to these six questions than it has at its present command. But this is not the end of the trouble. Careful attention may need to be given also to some further technical questions that I have had no space to examine, questions that probe central theses of Popper's philosophical achievement but are less logical and more distinctively metaphysical than those posed above. In my judgement there exist some unresolved difficulties relating to one of Popper's most cherished themes, metaphysical freedom, the ability of humans to intervene freely in the workings of the cosmos. I may mention the necessity of the laws of nature, the existence of historical laws, and the prospects for an optimistic account of creativity in a world of propensities. It is to be hoped that there will arise soon an occasion on which these further difficulties may be pitilessly explored.

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