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WHY BOTH POPPER AND WATKINS FAIL TO SOLVE THE PROBLEM OF INDUCTION

Accepted science not only does, but should, inform our technological practice. If someone wants to build a bridge that will stand up tomorrow or a plane that will fly tomorrow she should assume in particular that currently accepted low-level generalisations will continue to hold tomorrow. Someone who claimed (without evidence) that falling bodies will soon start to fall with an acceleration which increases as the cube of the time of fall would be regarded as downright irrational. Someone who encouraged passengers to fly on an aeroplane built on that supposition about future falling bodies would be regarded as criminally irresponsible. But we know, following Hume, that, since all the observational evidence we have for the generalisations accepted by science is of necessity evidence about the past, and since deductive logic is not content-increasing, we certainly cannot deductively infer that accepted generalisations will continue to hold in the future from any amount of evidence we may have. But what then is the basis for these very firm judgments about rationality and responsibility? This is, of course, the notorious 'pragmatic problem of induction'. John Watkins has recently joined the long list of philosophers who have attempted to solve the problem.

In this paper I shall argue that his crisp and challenging 'solution' entirely fails. However, he himself calls his attempted solution 'neo-Popperian' and sets it against the background of what he sees as Popper's own failure to solve the problem. I begin therefore by explaining exactly why I agree with John Watkins that Popper's solution fails. I feel rather apologetic about this: since I have nothing of real substance to add to the points already made against Popper's alleged solution in the 1930s by Reichenbach and Feigl and later by Ayer, Lakatos, Salmon, Grünbaum, Newton-Smith, and many others.

However, there are some philosophers – notably David Miller – who continue to believe that these criticisms miss their mark and that Popper's solution remains viable.[1] Since the point which seems so obvious to me is clearly not universally regarded as obvious, there may be some merit in trying once more to set out the argument as perspicuously as possible. Moreover, although Watkins accepts that Popper failed to solve the problem, it is not always clear that he accepts the full extent of that failure, since his

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own treatment seems to be to inherit some of the faults of Popper's. My treatment of Popper's 'solution' and Miller's defence of it in Sections 1 and 2 of my paper set the scene, then, for my arguments against Watkins's 'solution' in Section 3.[2]

1. POPPER'S FAILURE TO SOLVE THE PROBLEM OF INDUCTION

Chapter 1 of Popper's *Objective Knowledge* begins with the following striking claim:

> I think that I have solved a major philosophical problem: the problem of induction.[3]

And David Miller agrees – Popper's development of falsificationism means, Miller says, that "the problem of induction is at last well and truly solved".[4] However, as Popper complains, and as Miller documents, almost no other philosopher of science agrees that he has done any such thing. I'm afraid that on this issue the rest of the philosophical world is right, and in this section I try to explain why.

First of all, what *is* the problem of induction? Well, most philosophical problems tend to proliferate into whole sets of subtly different problems once you start to investigate them closely, and 'the' problem of induction is no exception. But the problem on which I shall concentrate (sometimes called the 'pragmatic problem of induction') can be posed by adapting slightly an example of Lakatos's.

Suppose you are admiring the view from the top of the Eiffel Tower and fall into conversation with a fellow view-admirer. You have both decided to return to ground level to continue your conversation over a cognac, when your new companion suddenly tells you that, rather than take the lift, he intends to leap over the balustrade and float gently to the ground. This fellow is, let's suppose, entirely sincere, not consciously or subconsciously suicidal, not into hallucinogenic drugs and has no hidden parachute under his T-shirt, no hidden super-powered motor in his pocket – he just genuinely believes that he will naturally float gently to the ground below. He turns out to be no intellectual slouch. He has taken A-level physics and knows all about Galileo's law (or, has he prefers to call it, 'alleged law) of free fall and he accepts that all the evidence so far in accords with this law – or at any rate with suitably weakened versions of it. For he also knows history of science and accepts that Galileo's law, as originally formulated, is strictly false. This is partly because of frictional effects of the air, and partly because, even in a vacuum, falling bodies would not be constantly accelerated, instead their acceleration depends on their distance from the centre of the earth (though this distance changes so little in 'normal' falls as to make Galileo's law good enough for practical purposes). He does not want to base his unorthodox proposal on points of detail such as this – he is happy to accept that there are weaker versions of Galileo's law, which so far as past evidence goes, are exceptionless. For example the generalisation that humans stepping out from heights comparable to that of the Eiffel Tower and not meeting any obstacle on the way arrive at the ground with sufficient velocity to prove horrifically fatal. And he accepts that what he proposes depends on a prediction which is quite inconsistent with this so far universally instantiated hypothesis. (Let me, for brevity, carry on referring to 'Galileo's law' – though with the understanding that this is to be understood in the above weakened sense.) Finally your new friend has also studied and accepted deductive logic; and indeed he insists that only deductively valid inferences infallibly transmit truth from premises to conclusion. However, he also insists – clearly correctly – that all the actually observed evidence is evidence *about the past.* Since deductively valid inference is non-content-increasing, it cannot then follow deductively from this evidence, no matter how much of it there may be, that Galileo's law, or alleged law, holds for all past *and future* falling bodies. In particular it cannot deductively follow from the evidence that this law will continue to hold for bodies that are released from a height in 5 minutes' time, which is when he intends to start his float. So far as deductive logic is concerned (the only sort that *guarantees* transmission of truth from premises to conclusion), all the evidence about what happened to bodies in the past is consistent with *any* assumption about what will happen to falling bodies in the future: it is consistent, for instance, *both* with the assumption that future freely falling bodies will, just like their predecessors, fall with roughly constant acceleration *and* with the assumption that they will quickly achieve and then maintain a gentle 'terminal' velocity' of, say, 1″ per second. This means, your new acquaintance points out, that your belief that he will come to a grisly end if he carries out his plan is no more entailed by the existing evidence than is his assumption that all will be well. Moreover, he points out, your own assumption that the safe plan is to take the lift is no better grounded than his own.[5] Your plan takes for granted various general claims, for example, about the tensile strength of steel cables of certain constitutions. These claims have admittedly been well-tested and have passed these tests – but all of them were of course performed in the past. So all *this* evidence is perfectly consistent, so far as deductive logic is concerned, with those steel cables in just 5 minutes' time suddenly exhibiting the tensile strength of, say, a normal cotton thread (not because of any 'fault' in this particular cable, but 'because' that's the way steel cables in general are going to be). If this were to happen then you'd better hope that he's right about the future of falling bodies. He makes no claim, therefore, that his plan is any *more* rational than yours, but only that his plan and your own *stand on a par* so far as reason is concerned. Indeed he had earlier flipped a penny intending to 'float' or take the lift depending on whether heads or tails came up; and heads had won. Wishing you luck in your venture, he prepares to climb over the balustrade.

What would you say to your new acquaintance? Well, one thing you *might* say is "Yes; on reflection you're right. There *is* no reason to prefer my proposal to yours. If we have good reasons for accepting anything at all, it is only for accepting the results of observation and the principles of deductive logic: anything else would call for a justification, and the justification for a justification and so on; so that any other claim that we relied on would ultimately have to count as ungrounded assumption. And your argument is precisely that nothing follows from established observation statements by deductive logic about what will happen to falling bodies in 5 minutes' time. My gut reaction is still that you will certainly come to a grisly end, but since I am a man of reason, I accept that there is no good reason to prefer my proposal to yours: go ahead you *may* be right."

You *might* say this, but you wouldn't. As Russell said, it might be impossible to fault the inductive sceptic's logic but it is downright impossible to take his claims really seriously.[6] Someone who was suffering from a bad case of over-philosophising might convince himself that he would let the chap on the Eiffel Tower go ahead, but put in the actual situation, then like me and you he would say that the chap is frankly 'bananas' and, if he persisted in trying to carry out his plan, would restrain him while sending someone else off to summon psychiatric assistance.

So that's precisely what you start to do. But then you have a better idea: since our floating friend is clearly a believer in argument and certainly *seems* otherwise sane enough, perhaps what the chap needs is not a psychiatrist but a *philosopher*. You vaguely recollect that the difficulties he is raising have to do with something called the problem of induction and you remember that there's a famous philosopher who actually claims to have *solved* this problem. So you put in an emergency call to Sir Karl himself, who agrees to come, accompanied by David Miller, who has interesting views on how to handle any criticism of Popper's solution that might come up.

Our friend, very keen to cross intellectual swords with the great man, agrees to postpone his float for a few hours. While waiting, however, you begin to get cold feet, knowing that some philosophers sometimes hold pretty dotty views and even sometimes have strange views on what counts as a 'solution' of a problem. Luckily someone has left some philosophical books lying around up there and one of them happens to be *Objective Knowledge*. So you check; and yes there it is: Popper definitely claims to solve the problem in the positive sense – he *does* positively recommend the lift as opposed to the air as the rationally preferable action if your aim is to get to the ground safely. Popper gives the following characterisation of the pragmatic problem:

Which theory should we prefer for practical action, from a rational point of view.[7]

And he gives the following unequivocal answer:

My answer...is...we should prefer as a basis for action the best-tested theory.[8]

Not, you are pleased to notice, '*will* as a matter of fact prefer' but *should* prefer "from a rational point of view".

Given this, and Sir Karl's reputation for being hard-headed about claims to *know* anything, you feel confident that you have called on exactly the right person – he will surely convince your new friend that, while his arguments might be cogent, his proposal to float down is irrational.

So Sir Karl and David Miller presumably choose the airplane based on the best corroborated theories of aerodynamics and choose to take the lift up the Eiffel Tower rather than, say, try to levitate: and, one way and another, arrive safely. However your new friend has spent the intervening time reading *Conjecture and Refutations*, *Objective Knowledge* and some of the other books that were lying around up here and himself starts off the debate.

FLOATER: I found what you had to say Sir Karl about the scientific method all very congenial; that we can never have positive reason to think a scientific theory true, that the best we can have is positive reason to think such a theory false. This seemed to be very much in line with my own view that, no matter how many freely falling bodies we have in the past observed to accelerate roughly uniformly, we cannot know that the next one will accelerate roughly uniformly, nor does all that positive evidence even make it more probable than not that the next falling body will roughly uniformly accelerate. You stress more than once that your methodology delivers only corroboration appraisals, that is, accounts of how well various theories and generalisations have stood up to empirical testing *so far*. You stress more than once that these corroboration appraisals have no synthetic forward-looking element. You stress more than once that the attempt to use the fact that a particular theory is best corroborated so far as an argument for believing it to be true or probably true, and hence for believing its predictions to be true or probably true is an inductivist error. All seemed to be confirming my view that it is no 'more rational' to take the lift than to take to the air. I was just about to use your arguments to quieten this tiresome chap who keeps calling me irrational, when I turned to Chapter 1 of *Objective Knowledge* and found to my astonishment that you assert that the only rational course of action is to act (in the future) on the best corroborated theory. This advice just seemed to be slapped down against the whole spirit of all your previous, powerful arguments and certainly without the least justification.

This fellow Wes Salmon, whose paper also happens to be lying around, seems to sum up very clearly the puzzlement that I (and apparently many others) feel:

> We begin by asking how science can possibly do without induction. We are told [by Popper] that the aim of science is to arrive at the best explanatory theories we can find. When we ask how to tell whether one theory is better than another, we are told it depends on their comparative ability to stand up to severe testing...When we ask whether this mode of evaluation does not contain some inductive aspect, we are assured that...since this evaluation is made entirely in terms of past performance, it escapes inductive contamination because it lacks predictive import. When we then ask how to select theories for purposes of rational prediction [and hence as a basis for rational action], we are told that we should prefer the theory which is 'best tested'..., even though we have been explicitly assured that testing...[has] no predictive import.[9]

As I understand it, Salmon (like your other critics) is ready – at any rate for the purposes of this argument – to concede that your deductivist account of theory-testing is entirely correct. Certainly no one any more believes (if indeed anyone ever *did* believe) that scientific generalisations are arrived at or justified by simple induction by enumeration. Everyone accepts that Galileo did *not* arrive at his 'law' by simply observing a whole series of freely falling bodies, noticing that they were all uniformly accelerated and then inductively generalising. The process by which Galileo's law became accepted as part of science is altogether more subtle – undoubtedly involving conjectures, refutations of alternatives, checks to see that other factors do not affect the observational outcome, to say nothing of a certain amount of idealisation. Let's assume that the judgement that Galileo's law (or, better, the weakened version mentioned above) is scientifically superior to any rival is as pure-white deductivist, as 'corroborationist' in your sense, as you like. Still the question arises whether this acceptance has any implications for *future* action. You say eventually that it *does*: but what else is your assertion that it is rational to work on the future predictions of the so far best corroborated theory than an inductive principle, connecting past performance with (likely) future performance?

So I ended up feeling lost: your philosophy in the end does contain an inductive principle, and, just like all the other inductivists, you can give no reason for this principle. You *tell* me that it is 'irrational' to try to float down, but *why*? Surely you don't expect me just to take this on trust? I concede of course that I may be in a minority of one – but isn't it the fate of all great innovators to be called 'irrational' by their stick-in-the-mud contemporaries? I also concede, of course, that it is easy for you to produce an account of rationality which will brand me as irrational by just making the bald *assertion* that it is rational always to act on the best corroborated theory. But I could equally well create a different theory of rationality which baldly asserted that it is rational to act on the worst-corroborated theory or fourth best corroborated theory – or, surely a better idea, since it seems downright rash *always* to act on either the best or the worst corroborated, I could create a theory of rationality which asserts that it is rational to act on the best corroborated theory on Mondays, Wednesdays and Fridays and rational to act on the worst corroborated theory the rest of the week, or, most rational of all, my own policy: play 'corroborationist' or 'countercorroborationist' on the flip of a fair coin. What makes your theory of rationality, your bald assertion of the inductive link between past and future, correct and any of my theories of rationality incorrect?

SIR KARL:[10] I see that, like everyone else, you have totally misunderstood my straightforward solution of the problem. Let me hand you over to David Miller to see if he can root out the sources of your misunderstanding.

DAVID MILLER: Well, first of all, you are correct that Popper's position yields the positive advice to prefer the predictions of the best-corroborated hypothesis that we have. And this means, if you like, acting *as though* the best-corroborated theory were true. For Popper, however, this is not because we have any reasons for supposing that it is true, but because there are no reasons for supposing that it is *not* true. 'The hypothesis that has best survived the critical debate is the one that we have least reason to think false.'[11] Now to quote a paper of mine:

> This answer manifestly involves no recourse to any principle of induction. There is no suggestion that the proposal that best survives criticism is one that we have any reason to expect to be successful. In fact we will not have any reason to expect it to be successful; but this hardly means that it will not be successful.[12]

FLOATER: Now I'm really lost: if anything is manifest here it's surely precisely that this answer *does* involve recourse to a principle of induction. But let's proceed one step at a time, the first question I want to raise is this: why exactly has the critical debate so far given me least reason to think Galileo's law false? After all, I accept of course that all the so far observed falling bodies have instantiated it, but this means, since all those observations were made in the past, that they equally well instantiate this hypothesis: that all falling bodies up to 5 minutes hence will fall with roughly constant acceleration and all future bodies achieve the gentle terminal velocity I mentioned. It seems clear to me that I have just as little reason – *good* reason in yours and Popper's sense – to regard this alternative as false as I have for regarding Galileo's law as false: both equally fail to be empirically refuted so far.

DAVID MILLER: Ah, I see you have been reading that gruesome stuff by Nelson Goodman. You really ought to learn to use your time better, especially since you seem to have so little of it left. Gruesome hypotheses are banned from consideration.[13]

FLOATER: I assume that you would agree that if you need to ban such hypotheses from consideration in order to solve the problem, then you would need to cite good, *non-inductive* reasons for the ban – and I can't see any cogent Popperian reason to rule them out. But let's leave this for the moment and go along with your ban.[14] This will force me to change the details of my position slightly, but the essential point remains the same. The universal theory whose prediction I now propose to act on is simply the 'natural' generalisation that '*All* freely falling bodies accelerate up to 1″ per second and then retain that velocity'. Now I accept of course that I have plenty of reason to regard this as false – as I said right from the start I accept observation results and deductive logic, and my theory has of course been multiply refuted. And given that I'm accepting your gruesome ban, I accept that the only articulated generalisation already in this area which there is no reason so far to think false is Galileo's.

Still, as you and Popper have often emphasized, *all* the universal theories that we have so far articulated – perhaps even all the universal theories that we *can* articulate in our language – may well turn out to be false. Certainly as far as observation and deductive logic go we have no reason to think that Galileo's law won't be falsified in exactly the *next* instance. So why exactly doesn't the advice you give – namely to act as though it *won't* be the case that Galileo's law will be falsified in the next instance, given that it has failed to be falsified in all the cases so far – why exactly doesn't that advice 'involve recourse to a principle of induction'?

Let's, in order to make things simple, assume that the total duration of the universe is finite – that the theory that everything, including time itself, started with the big bang and will end with a big crunch – is correct. In that case, there will only ever be finitely many bodies released above but close to the earth's surface and the generalisations we are considering will all reduce to enormous finite conjunctions. Let's suppose, just to pluck a few figures out of the air, that there have so far been 10^{25} falling bodies and that there will be 10^{250} further falling bodies before the big crunch. And suppose finally that my initial gruesome hypothesis is in fact true – as I'm sure you will concede is logically possible despite your ban. This means that the universal theory I am now proposing is overwhelmingly *less false* overall (in a perfectly definite sense now that we are dealing with only finitely many instances) than Galileo's theory which admittedly looks less false *so far*. Surely then, in advocating the action based on the prediction of Galileo's theory as rational, you are taking it that it is rational in circumstances like these to suppose that the sample we have so far is representative of the whole population – a classic inductive assumption. Of course everyone agrees with me and you that the prediction *may*, logically speaking, be wrong – but everyone apart from me seems to think it is rational to act on that prediction nonetheless. Your way of describing the present evidential situation in this negative way, of least reason to think certain theories false, may be preferable to the descriptions adopted by other philosophers, but when it comes to linking the past evidential situation to rational prediction, you all seem to be in the same boat. In taking it that it is reasonable to rely on the predictions of a theory we presently have least reason to think false, you are invoking an inductive principle, no less than if you had taken it that it is reasonable to rely on the predictions of the theory we have presently most reason to think true.

DAVID MILLER: What you say is 'really quite silly'[15] and the possibility that you point to of a change in the course of nature really quite 'boring'[16] – I am beginning to think that you don't exist and are nothing more than the product of a philosophically-overheated imagination. After all, Galileo's law is an accepted part of science, indeed part of background knowledge, and it gives conclusive reason to regard your prediction as false. No one can really hold the view you claim to. If you have a *serious* criticism of Galileo's law please state it. If not then *mere* 'doubt is not criticism'.[17]

FLOATER: I have a whole list of things to say about that. One is that you would surely find it difficult to say what a "change in the course of nature" *means* if you insist on language-independence, as you do in your interesting work on verisimilitude.[18] But my response to the main point is this.

The question, of course, is not whether there are many (or even any) people like me. The question is about *why* I am in such a minority. The question is *not* about whether the possibility I point to is 'silly', it is about what *grounds* you have for ruling it out. The question is *not* whether science *does* as a matter of fact accept and 'project' into the future certain theories that my opponents seem to think are the intuitively right ones to project. Of course it does this – that's why I'm such a revolutionary figure in refusing to go along with what is, so far as I can tell, a universally adopted but entirely groundless practice. Of course, if you are allowed to appeal to the fact that science just does contain many generalisations on whose predictions everyone else relies, and if you are allowed to count *that* as a solution of the problem then the problem was "solved" long before Sir Karl by David Hume himself. Hume never for a moment thought that he was bringing induction into serious practical doubt or that the logical possibilities he pointed to were other than 'silly' from the practical point of view – we all do of course use induction. But Hume – surely more accurately – saw this as *constituting* the problem, *not* as the solution of it.

You say that I am not raising a serious criticism of Galileo's law, but only a 'boring' doubt. But what if science had accepted the gruesome hypothesis that I think true? In that case someone who suggested that the accepted theory *might* be false because the next falling body might fall with roughly constant acceleration would be raising a 'mere doubt' not a serious criticism. You might say that this itself is not a serious possibility. But *why* not? Surely the answer is because science is just unthinkingly inductive in the usual sense. But then in assuming science or any part of it, such as so-called background knowledge, you are taking induction on board. You can't presuppose the very point at issue and then claim that you have solved the problem! The history of philosophy is littered with the corpses of just such attempted solutions.

DAVID MILLER: O.K., let me try a different argument. Your claim that Popper's position and that of the self-confessed 'inductivists' are in the same boat here is quite wrong. The inductivists *do* invoke a principle of induction because they take the past evidential record as justifying the prediction of the so far best confirmed theory – that is, as lending that prediction inductive certainty or, at any rate, greater rational credibility than any conflicting prediction. Thus an inductivist would tell you that you are entitled to a greater degree of rational confidence in your eventual safety, if you decide to take the lift, than if you take to the air. Popper's position, on the contrary, involves no inductive principle because he does *not* assert that you will be entitled to any more rational confidence in your eventual safety if you abandon your present plan and you decide to take the lift. According to Popper, if you are rational then you will not *assume* that you can have greater confidence in the predictions of the so far best corroborated theory; it's just that if you are rational you will always act on those predictions and therefore will always act *as though* you were entitled to such a greater degree of confidence.[19]

FLOATER: Let me get this straight – if you're saying what I think you're saying, then in the immortal words of Frankie Howerd 'never has my flabber *been* so gasted'. I'm no philosopher of course, but are you seriously claiming that you can issue the advice always to act *as if* you assumed that the future will (in these very specific respects) be like the past, that you can state that this is not a simple *description* of what everyone apart from me will in fact do but instead that it is the rational way to act, that you can issue this allegedly rational advice to act *as if* a certain assumption were true and yet still not invoke that assumption and therefore avoid any obligation to defend it if challenged?

Let me chance an analogy – a rather tasteless one, I'll admit, but one that seems to make the point, and after all my life is at stake here. Consider a white South African whose every action is *as if* he held that blacks are innately inferior, he prevents them from having any political power, he bars them from every place that whites like to frequent, and so on. However he asserts that the *assumption* that blacks are innately inferior is no part of his theory of the world and indeed, if asked, explicitly allows that he has no good reason to think so. Does he on this account escape the charge of being racist?

Why, returning to the induction problem, should it be rational to act *as though* the best corroborated theory were true, if it is not rational to have greater faith in the best corroborated theory's prediction? What you seem to be saying is that the difference between the 'inductivist' and the Popperian man of rational action is that, while both take the lift, the latter is as scared as hell while doing so!

SIR KARL: Let me step in here. Despite your claim to know the history of science, you must in fact be quite ignorant of it. No one who knew the history of that great intellectual adventure could possibly think that it is rational to have complete faith in the prediction of even the best best-corroborated theories – even the best-corroborated theories are eventually empirically refuted. Moreover, as I have shown, the whole enterprise of producing a probabilistic inductive logic which would justify rational *degrees* of confidence has run into sand.

INTERESTED BYSTANDER: I couldn't help overhearing your interesting debate. I wonder if I could say a few words at this point on behalf of the so-far silent majority. It seems to me, Sir Karl, that you are not quite appreciating how revolutionary a position our friend the floater adopts.

Everyone nowadays is, I take it, a fallibilist about scientific *theories*: by this I mean not a fallibilist *in principle* (this position seems to be dictated by logic alone) but a fallibilist *in practice* – the history of science clearly shows that even the most successful high level theories may eventually be rejected (even if they do standardly 'live on' as 'limiting cases').[20] Certainly a well-corroborated theory may make empirical predictions which are *false*. Suppose some 19th century man of science was planning an action where success, rather improbably, depended on the details of Mercury's observed orbit over the succeeding six months. No one, I take it, would say that his only rational course of action was to depend on the predictions of the best corroborated high-level theory available to him. The best corroborated theory was Newton's and, as is well known, it got Mercury's orbit quite wrong. Even where the best corroborated theory in a certain field has *no* obvious 'anomalies' we shall surely not rely on its untested predictions if these are of a *new kind* of event. For example, Fresnel's wave theory was in 1830 undoubtedly the best corroborated theory of light. In that year Hamilton showed that the theory made the startling prediction that a beam of light directed along a certain line into a crystal of a certain type would open up inside the crystal into a hollow cone of rays. In 1833 Humphrey Lloyd set up the experiment to test this prediction. Suppose (implausibly, of course, since the experiment is fairly delicate) that you were guaranteed that Lloyd's experiment would detect the effect if there was an effect to detect. Would the rational man *rely* on Fresnel's prediction being correct? Would he to make things precise, agree to be strapped into a device which fired a bullet into his brain if the light beam in Lloyd's experiment failed to open out into a cone? Well, I like to think of myself as fairly 'rational' and I can tell you that there is no way you would have got me into that contraption under those conditions. I don't myself see how we can do without some notion of rational degrees of belief in cases like this – if he *had* to bet, surely the rational man would back Fresnel, given his enormous success with other types of optical effect, or at any rate, he would be considerably readier to back Fresnel's prediction than, say, the prediction of some religious figure who predicted the end of the world in 1833. But there is surely no question here of anything even approximating absolute rational reliance.

This marks a clear difference between this type of case, exemplified by conical reflection, and the type of case exemplified by the weakened version of Galileo's law that you have been discussing. I just report it as a fact about me (and I conjecture about most people) that while there is no chance of my agreeing to be strapped into the bullet-firing contrivance in the Lloyd case, if someone asked me to be strapped into it on condition that it was absolutely guaranteed to fire *only if* our friend the floater carries out his plan and gets to the ground successfully (without tricks) then, at the expense of a donation to my favourite charity to cover my inconvenience, I'd accept.

So, while we may or may not have rational degrees of belief in them, it seems to me that we do *not rely* on a high-level theory's predictions, no matter how well corroborated that theory might be. What we *do* however rely on are the further predictions of already well tested *low level* empirical generalisations. (Of course the hitherto unsuspected observational generalisations entailed by high-level theories will standardly very quickly *be* empirically – repeatedly and independently – checked. If these checks are positive, we shall be in the position of relying at the same time on *both* the predictions of the best corroborated high level theory *and* the predictions of an exceptionless low-level observational generalisation, but it's the latter that really counts.) Many recent philosophers (some of them influenced by you, Sir Karl) have held that *all* observation statements, and therefore all observational generalisations, are 'theory laden' in a sense which makes them not only open to correction *in principle* but which also makes their corrigibility a serious *practical* concern. They seem to me to make their point, however, by treating *very* 'high level' statements as observational. (One of them, I believe, even treats 'The Brownian particle is a *perpetual* motion machine of the second kind' as directly empirical![21]) But if you go to a low enough level (to the level of what Poincaré called 'crude facts' and Duhem 'practical facts' – meter readings, angles of inclination of telescopes, digital printouts and the like) then there is, I claim, no case in the whole history of science where a once accepted, well and independently checked, low level observational generalisation turned out subsequently to be falsified.[22]

This is why our friend the floater here is such a revolutionary! He is taking his stand on the possible future falsity of an overwhelmingly confirmed empirical generalisation at the practical factual level, in precisely the area where it has been confirmed again and again.

Concerning predictions made by a theory of a type not already empirically well confirmed (even though the theory overall may be far and away the best confirmed we have), due regard to Popperian fallibilism is entirely in order – because indicated by the history of science itself. But it is precisely in the case of already well-confirmed low-level practical empirical generalisations on which we *do* rely and must rely for survival, precisely in those cases where the history of science supports an 'optimistic' rather than a 'pessimistic' meta-induction, precisely in those cases where 'rational to rely on' clearly means 'the right thing to rely on for survival purposes', that we all do make inductions and where it seems clearly irrational *not* to do so.

FLOATER: I agree that I am a true radical. I am not merely claiming that predictions about new kinds of phenomena made by the so-far best scientific theory may be false – a possibility clearly instantiated in the history of science. I am claiming that, even if there is not a single case in history of a well-tested *low-level empirical generalisation* which suddenly turned out to give an incorrect prediction, still we have no *reason* to act as though the predictions of any such generalisation will turn out correct in the next instance. Certainly I am still waiting to hear a Popperian reason for always acting in this way, and so I am still waiting to hear a Popperian reason why my proposal to float down is irrational.

SIR KARL: Your chief problem appears to be that you are still caught up in what I call the 'justificationist framework'. If you keep asking for *reasons*, keep on asking 'why', then – as the Greeks already saw – you end up in an infinite regress and hence in scepticism. You must accept certain assumptions when neither you nor any one else has produced any *good* reason to criticise them. The Greeks thought that the only way out of scepticism is through dogmatism – through the simple assertion of certain claims which are regarded as self-evident and therefore not in need of any further justification. I, however, have shown how to 'transcend' the dogmatist-sceptic controversy. We must, as I have indicated, accept certain substantive assumptions without justification; but this is not dogmatism because, should anyone subsequently raise serious criticisms of these assumptions, then we could then challenge them, and perhaps replace them – though this would of course involve taking for granted, *again temporarily*, other assumptions.[23]

FLOATER: But, as I indicated before, your notion of what counts as a 'good reason' or a 'serious criticism' is carrying all the weight here and indeed simply prejudges the debate against me. After all, there is, of course, within the framework I adopt, no serious criticism of my counter-inductive claim – only 'mere doubt'. I need hardly add that if you argued that there *is* a serious criticism of my counter-inductive policy even within my framework – namely that anyone working on this policy in the past would not have survived – then I will reply that this criticism presupposes induction. It seems to me that for all your arguments there are just the two, selfsame options that we started with: you *either* allow that my framework and yours are equally respectable and that there is no rational way of adjudicating between them (and that's just scepticism) *or* you *assert* that induction (at any rate of this very limited kind) is rational (and that's just dogmatism).

Isn't it better just to come clean and admit that you simply *assert* as part of your theory of rationality that it is rational to take the best corroborated generalisations (at any rate of this low level kind) as your guide to future action? Why not admit that this is an inductive principle and you simply assert it? You can then call this a 'solution' of the problem of induction if you like, but solutions by fiat seem perilously close to admissions that the problem is insoluble.

DAVID MILLER: But how can someone call himself a rationalist if he simply dogmatically asserts some substantive principles of rationality? The whole Popperian position was aimed at taking nothing for granted beyond *possibly* the results of low-level observations and the principles of deductive logic.

FLOATER: Yes, but the basic problem for the so-called rationalist was that if that's all you have in your rationality theory, if you settle for really hardnosed deductive rationality then you must end up in inductive scepticism: precisely my own position. It must indeed be embarrassing for a rationalist to admit that his theory of rationality is based on substantive assumptions which, since they partially constitute rationality, cannot themselves be accredited as rational – to admit that we must, as I believe Popper himself once acknowledged,[24] make the irrational (or, better, non-rational) decision to be rational. What I am suggesting is that deductive logic itself with its great first principle that, as they say where I come from, 'you don't get owt for nowt', demands that if you *are* going to bridge the acknowledged deductive gap between past and future and thus characterise my proposed float as 'irrational', then you must make some substantive inductive principle part of your theory of rationality.[25]

DAVID MILLER (producing a gun):[26] You're right, I just have to assert an inductive principle dogmatically – now get in that lift!

FLOATER: Hang on – let me toss my coin again. Ah: this time it has landed 'tails', so I'd better go along with the prediction of the so far best-corroborated theory about what would happen to me if you actually fired that thing. I'm still doubtful about that lift, but at least if we *do* reach the ground safely, I'll be able to explain why there are no reasons, apart from inductive ones, for banning gruesome hypotheses.

2. MILLER AND POPPER ON GRUESOME HYPOTHESES

FLOATER: Well, we did get down safely. Of course, that has no consequence at all about what it will be rational to do when I ascend the tower again tomorrow. But before you go, let me explain to you my worries about your treatment of what you called 'gruesome hypotheses'.

You will remember that the alternative hypothesis to Galileo's 'law' that I originally asked you to consider was that all freely falling bodies are, let's say, uniterm – this is a new technical term: in your old-fashioned language it means "either falling before time t_0 and roughly uniformly accelerated or falling after t_0 and quickly achieving the terminal velocity of 1″ per second'. (Remember, though, that I don't actually assert that this alternative is the one to act on, but only that it has equal claims with your own favourite. It is you who wants to stick out his neck, I take a nice (pre-Thatcherite) English middle of the road position, deciding on the toss of a coin.) Now clearly you need to ban this alternative hypothesis if your general advice to prefer that theory we have least reason to think false is ever to deliver any practical advice at all. For by 'reason to think false' here, you intend, I think, what you call a '*good* reason': that, after all, was the whole appeal of the falsificationist position – that it needed no doubtful inductive principle, but only deductive logic and basic statements. But clearly we have no more 'good reason' to think this alternative false than we have for thinking that Galileo's 'law' is false. Both are equally unrefuted so far.

DAVID MILLER: Exactly – that is why it is a general principle that 'conflicting hypotheses must not be admitted to empirical science unless there are crucial tests that can eliminate at least one of them...If we are not prepared to delay until [after time t_0] a decision between [the two]...we must not admit them both into science...'.[27]

FLOATER: Well, since my original hypothesis took t_0 to be about half an hour ago, and since I just noticed that someone droppped a tennis ball from the top of the Tower and it fell with roughly uniform acceleration, if you *had* been prepared to delay a decision and had not forced me into the lift then I should have been dead by now. By the way, I of course rejoice in the refutation of this erstwhile theory of mine; and am very happy to have let the theory die in my stead. I need hardly add however that I now have a new suggested alternative theory to Galileo's 'law', which differs from my old one only in reidentifying t_0 as noon tomorrow. But let's go back to the old situation: clearly one of the two generalisations needed to be excluded if you were to give me advice on whether or not to carry out my float. But *which one?*

DAVID MILLER: 'The answer is all too obvious.'[28]

FLOATER: Well it *is* obvious which one you would *like* to be barred so that your principle to prefer the theory we have least reason to think false delivers the advice which you and everyone else apart from me seems to think is the only rational advice. But what *reason* can a Popperian give for barring the gruesome hypothesis in advance of any further evidence?

DAVID MILLER: Gruesome hypotheses like 'all freely falling bodies fall unitermly' are less falsifiable than 'natural' hypotheses like 'all freely falling bodies fall with constant (or roughly constant) acceleration'. The reason is that in order to falsify the second we need only an accepted basic statement which says that a body fell freely but not with even roughly uniform acceleration; while in order to falsify the first we would need *both* a description of a body's fall *and* a statement of the time at which that fall occurred. It is of course a basic (and *non*-inductive) principle of Popper's philosophy that more falsifiable theories are to be preferred to less falsifiable ones – unless, that is, the more falsifiable one is actually falsified. Until the more falsifiable Galileo generalisation gets refuted, its less falsifiable competitor should not even be considered.[29]

FLOATER: Goodman already showed, I believe, that any such suggestion is inevitably language-dependent. If we take his own example, if 'grue' and 'bleen' rather than 'green' and 'blue' are regarded as primitive predicates, then all we need in order to refute 'all emeralds are grue' is a non-grue-emerald, whereas in order to refute the weird hypothesis 'all emeralds are green' (equivalently: all emeralds are grue and observed before t_0) or bleen and observed after t_0, then we need both a statement of the emerald's 'colour' and a time at which it is observed.

But aside from this, it would anyway surely be right to spell out the quantificational structure of Galileo's law as "for any body *b* and for any time *t* if *b* is released above but close to the earth's surface at *t* then *b* falls to the earth with (roughly) uniform acceleration'. In order to falsify the law as thus formalised, we should need, of course, a description of a body *and* a time interval during which that body was allowed to fall freely, but in which it did not fall with constant (or roughly constant) acceleration.

Finally, and even ignoring these points, it does seem strange to appeal to the fact that we could forget information that we in fact can freely have (the motion if observed must have been observed at some time) as a justification for preferring one theory over another.

DAVID MILLER: Here is a different Popperian reason for banning the gruesome hypothesis rather than Galileo's. You see the problem only arises because so many philosophers are justificationists and *will* insist on talking in terms of 'empirical support' for theories. Since the gruesome hypothesis has been constructed precisely so that, on justificationist criteria, it will be supported by just as many facts as is Galileo's the justificationist is bound to think it mysterious that the gruesome hypothesis is not considered as standing on a par with Galileo's. But for the Popperian this problem simply does not arise. The aberrant hypothesis needs to be considered by a Popperian only if it promises to *solve some problem* not already solved by Galileo's law; and this it clearly fails to do.[30]

FLOATER: There are of course accounts of 'empirical support' which do not in fact yield Galileo's law and its gruesome rival as equally well supported.[31] However, my main reply to your argument is simply that it does no more than push the problem one stage back. As I already acknowledged several times, there is, of course, no doubt that science is against me: science does, *as a matter of fact*, sanction what everyone else seems to believe are the intuitively right predictions. As everyone knows, Hume himself accepted, indeed emphasised, this; but his question and therefore the question all along was not *whether* science sanctions the "intuitively right" actions as rational, but *why, on what grounds*, it does so.

You see I seem to lack the intuitions which everyone else shares; so you are not going to convince me by pointing out that scientists all have these intuitions – I knew that already. Galileo's law is no doubt an accepted part of science, perhaps even of "background knowledge". But we must now ask why Galileo's law *came* to occupy this privileged position on the basis of all this evidence about falling bodies, when exactly the same evidence also follows from the alternative hypothesis that I suggested. I don't suppose you would be happy just to regard this as an historical accident. After all, had my alternative been the one to be accepted initially, then your rule would have banned consideration of Galileo's law on the grounds that it solved no problems not already solved by an accepted theory. (Each theory basically solves the same set of 'problems' by describing with equivalent precision the motion of *every* freely falling body close to the earth's surface.) The problem is *not* do I have reason to take the lift rather than the air *if I accept science*, since obviously science predicts safety in the first case and disaster in the second. The problem is precisely *why* science accepts certain universal generalisations which therefore make predictions about the future when it can of necessity have only finite evidence from the past for these universal claims. Scientists do clearly intuitively prefer Galileo's law to any gruesome alternative, but what makes their intuitions better than my own? The only answer seems to be that it is the Galileo-style generalisations rather than the gruesome ones that have proved successful in the past. I don't accept this answer of course – precisely because it is inductive – but at least it's an answer: from a Popperian point of view the ban on gruesome hypotheses remains entirely arbitrary.

3. WATKINS' FAILURE TO SOLVE THE PROBLEM OF INDUCTION

JOHN WATKINS: Excuse me for butting in but I heard about your plight and immediately flew over to see if I could help. You see although I agree with you that Popper has completely failed to solve the problem, I have a 'neo-Popperian' solution which really does the trick.[32] Since you are clearly responsive to argument I think I may well be able to convince you that your new proposal to float tomorrow is irrational.

First of all let me say where I agree with Popper. My neo-Popperian, just like Popper himself, 'seeks to exclude all non-deductive inferences and insists that corroboration appraisals of hypotheses have no implications for their future performance'.[33] However, I agree with you (and of course with Hume) that the problem precisely is that deductive logic and observation results so far have no implications for future action. I also agree with you that if a philosophical system *is* going to advise a technologist to act on the prediction of the best corroborated theory, then it had better yield some *reason* for doing so.[34] Let's not pussyfoot around – to give such a reason we are going to have to incorporate certain extra *assumptions* into our theory of rational action, assumptions which, by the very nature of the problem, cannot be sanctioned either by observation or by deductive logic. In fact I propose two such assumptions which together solve the pragmatic problem of induction.

FLOATER: I am of course eager to hear your assumptions. But let me first make a general point about your whole enterprise here, since it may well turn out to clarify our later discussions. Of course it would be easy to develop a theory of 'rationality' which underwrote your and everyone else's insistence that my proposed float is irrational. For one thing you could simply *assert* that it is rational to act on the prediction of the best-corroborated theory. This 'solves' the problem 'at a stroke'. I am sure, however, that you would not expect me to be convinced by this – since it 'solves' the problem of induction simply by 'presupposing' that induction is rational, and hence simply presupposing the point at issue between us. Another, often tried way to 'solve' the problem is less obviously *ad hoc*, but equally unsatisfactory in the end. This involves invoking some assumption of the *uniformity of nature*. Now I readily concede that, suitably formulated, such a principle can sanction our crossing the deductive gap between past performance of a theory and its future predictions. But, as I understand has often been pointed out, if we ask what grounds we have for making the assumption that nature is 'uniform', then the answer can only be *inductive* – nature has seemed to be 'uniform' (in certain special respects) in the past, it seems reasonable to suppose that it *is (always)* uniform. The principle of uniformity of nature 'stands or falls' with induction itself, and cannot therefore provide a satisfactory rationale for induction. This criticism is, I believe, entirely standard, surely correct and I assume you accept it.

So, in claiming to provide a positive solution of the problem of induction, you must presumably believe that you can produce some principle or principles which (a) seem somehow plausible as general principles of rationality, (b) together sanction the step from 'best according to tests so far' to 'prefer its predictions', and (c) seem somehow *independent* of the straight inductive assumption, seem somehow *not* to 'stand or fall' with induction itself.

But even before you give me your specific proposals, it seems clear to me that you are skating on pretty thin ice. Conditions (b) and (c) give all the appearances of being contradictory. Whatever general assumptions about rationality you advocate, they cannot fulfill conditions (b) and still be *logically* independent of the inductive assumption. The only clear sense of 'presupposes' that I know of is 'entails' (that is, *A* presupposes *B* just in case *A* deductively entails *B*): so it is difficult for me to see how a set of general rationality assumptions can even be a candidate for solving the pragmatic problem *without* 'presupposing' (that is, entailing) that the best-corroborated hypothesis will be best corroborated in the future (or whatever your preferred inductive link is).

Another tack someone who proposed a positive solution of the problem of induction might take is to claim that he can produce principles of rationality which don't just entail the 'inductive link', but actually *explain* it. But I have problems with the whole idea of *philosophical* explanation. It's true, of course, that in the case of (deductive) explanations in science, it wouldn't be usual to complain that the explanans simply presupposes the explanandum, although it certainly does: if Newton's theory explains the planetary motions, then it must *entail* those motions, and hence must 'presuppose' them in the perfectly clear sense that Newton's theory itself cannot be true unless those motions occur. This, I suggest, does not worry us in science, because we only regard a general theory as an explanation of some fact or regularity if there is lots of *independent* evidence for it: we wouldn't regard Newton's theory as an explanation of the planetary motions if it weren't the case that it entails lots of other testable (and correct) claims besides. The notion of a 'philosophical explanation' on the other hand is altogether more nebulous:

certainly it hardly seems realistic to expect an explanation of induction to be independently testable in any very definite sense. That's why in the end I think you're just going to have to reach for your gun, like David Miller – that is, you're going to have to *assert* that I am irrational, and that's an end to it.

JOHN WATKINS: No, you have entirely misunderstood my whole approach to the problem. If my aim were to present rationality assumptions which entailed that the universe is and will be such as to make the success rate of predictions made by well-corroborated theories higher than that of predictions made by not so well-corrorobated theories then I would, of course, agree with you that I must implicitly presuppose an inductive principle. But all that I seek is some slight reason for preferring what I call corroborationism over any version of countercorroborationism – assuming that in order to survive we need to act on predictions using one or the other policy. I am merely trying to show that there is a reason which just tips the balance in favour of corroborationism; I am definitely *not* trying to develop a position which entails the truth (or likely truth) of the assumption about the world which underlies corroborationism. If I can give you a *reason*, no matter how marginal, for preferring corroborationism, and if no one has given any reason, no matter how marginal, for the opposite preference, then that's surely good enough. And if this reason is *not* that the world is such (or likely to be such) as to make corroborationism true, then I can surely advocate corroborationism *without* invoking any inductive assumption.

FLOATER: Oh dear I think I'm about to start to sound like a philosopher – but it does seem to me that a lot here depends on what counts as a 'reason'. If this just means something which seems intuitively relevant, is presented as an argument and makes no obvious appeal to emotion or whatever, and which perhaps as a matter of fact would persuade some people, then nothing is easier of course than to produce a reason which 'tips the balance' in favour of inductivism (or corroborationism). One such 'reason' is that following the inductivist (corroborationist) policy has always (or, at any rate, much more often than not) paid off in the past. This is indeed surely the 'reason' that most people would, if pressed, cite for their inductivist tendencies. But, it's clear that if it's considered good enough to cite *such* 'reasons' then Hume's problem need never have arisen in the first place. Suppose to make the problem sharp, I cited the previous *lack* of success of counterinductivism as a reason which tips the balance in *its* favour (after all, 'every dog has its day' and all that, and clearly inductivism really has pushed its luck so far). If you simply refused to countenance this as a reason then you are really no better off than David Miller, who in the end simply dismissed my counter-corroborationist conjecture as not serious. If you don't just dismiss it out of hand, then I can see no way out of simply asserting that while your balance tipping reason (whatever it is) really does make it more likely that inductivism (corroborationism) is true in the future, my balance tipping reason does not. But then we are back to making what you yourself admitted is an inductive assumption.

JOHN WATKINS: I propose that we postpone further consideration of this general point until after you have heard my specific proposal. You see your general point just can't be right: because my solution of the pragmatic problem of induction (as you'll see if you ever let me get it out), although it certainly uses the past success of the policy of following the best-corroborated theory, uses that past success in an entirely *non*-inductive way.

I see that this claim has stunned you into silence so let me give you my solution. It relies, as I admitted, on some assumptions – in fact just two of them. The *first* is that 'well corroborated hypotheses *have hitherto* proved better guides for practical decision makers than have hypotheses that were not well corroborated'.[35]

FLOATER: Well I am of course more than happy to grant you *that* assumption – indeed it just seems to me an obvious *fact* about the history of science and technology (very broadly construed). I would even say, as I indicated before, that if we restrict the issue, as I believe at any rate in the first place we should, to low-level empirical generalisations, then following the best-corroborated hypothesis has as a matter of fact been an infallible sure-fire method. Certainly if your second 'assumption' is as innocuous as this one, then my floating days are over.

JOHN WATKINS: Well my second assumption is simply this:

> it is rational, other things being equal, to choose that course [of action] whose success presupposes *the weaker forecast about the future*. For a weaker forecast is less likely, other thing being equal, to turn out wrong than a stronger one.[36]

So, for example, if you were on a family holiday and choosing between two ways to spend the day, and if plan *A* and plan *B* would each deliver the same number of utiles if successful and the same number of disutiles if unsuccessful, and if finally plan *A*'s success depended only on the weather not being windy, while plan *B*'s success required that it be *neither* windy *nor* rainy, then it is surely rational to choose plan *A*.

FLOATER: This certainly sounds like a very plausible (though also, I can't resist pointing out, very non-Popperian) general principle of rationality: in practical concerns be as timid, as non-bold, as you can possibly be. I would be very happy to accept it if restricted to cases of the sort you mention. After all, because of the deductive entailment relation between the two forecasts (neither windy nor rainy *implies* not windy), whenever plan *B* would have been successful so also would plan *A*, though not necessarily *vice versa* (and indeed in this case given what we know about the weather not in fact *vice versa*). However, I'm not at all clear how your second assumption is going to apply to the sort of cases over which we are in dispute. After all, the two forecasts which we make about future falling bodies, and in particular me, are logically inconsistent: I say that I shall quickly achieve a terminal velocity of 1″ per second if I jump, you say that I shall accelerate roughly uniformly. So, far from one entailing the other, *each* entails the other's negation. In what sense is your forecast about the future *weaker* than mine?

JOHN WATKINS: Well, since I want to help not just you but any others who suffer from these unfortunate irrational tendencies, let's consider the issue of which *general policy* to follow – corroborationism, which involves always preferring the prediction of the best-corroborated hypothesis, and some version of counter-corroborationism. The claim about the world which underlies corroborationism is that the overall superior success rate for well-corroborated theories is a 'constant feature' of 'the whole history (past and future) of mankind'.[37] As for the claims underlying countercorroborationism, it seems to me best to differentiate two versions. According to one version, the straight 'contrary' of corroborationism, the overall (past and future) success rate of actions guided by less well corroborated hypotheses is higher than the success rate of actions guided by the best corroborated hypotheses. The second, 'grueish' version of counter-corroborationism just says that the *future* success rate will be higher for actions guided by less well-corroborated hypotheses.

FLOATER: I'm not happy with your switch to talk of *general* policies here. One thing is that I don't hold one myself: I never claimed that there was some positive reason to prefer my proposed float to my friend's proposal of descent by lift, but only that there was nothing to choose between the two proposals from the point of view of rationality. As I said, I actually decided to float on the toss of a coin. Another difficulty is that I'm not clear exactly what your countercorroborationists say – do they say, in each instance, *which* particular non-best corroborated theory yields the rational prediction to follow? If not, they don't seem to say anything about getting down from the Eiffel Tower tomorrow. Countercorroborationism then would entail that one can't rely on all future falling bodies falling in the 'normal way', but it would *not* entail that I won't fall in the 'normal way' tomorrow. Finally it is presumably incoherent to assume that less well corroborated theories have a high success rate in the future: once they start to have any success they start to be corroborated. Presumably we are to take the available theories at some particular time, now say, divide them into best and less well corroborated *now*, and then just consider how they perform in the future, forgetting what this future performance might do to the original corroboration appraisals.

JOHN WATKINS: Well why not go along with the switch to general policies for the moment, and see where it leads? I accept your last clarification; and, as for your other point, I certainly want countercorroborationism to stand initially (that is, ahead of any evidence) entirely on a par with corroborationism, so let me clarify by assuming that countercorroborationism does pick out a *definite* prediction in each case (this may, for example, be the prediction of the fourteenth best corroborated theory in the field – the details won't in fact matter, I think). Indeed the whole idea of my solution is that the two principles should, in advance of the evidence about what has happened so far, stand entirely on a par, but that bringing in the evidence (the evidence, namely, that corroborationism has been more successful in the past) transforms the situation in favour of corroborationism: having made claims of exactly the same strength as its rival, it *now*, in the light of the evidence, makes the *weaker* prediction about the future.

FLOATER: This sounds like a classic inductivist assumption at the meta-level: the past success of inductivism makes its prediction (that it will continue to be successful in the future) weaker, 'that is, less likely...to turn out wrong'.

JOHN WATKINS: On the contrary, my claim is that the prediction that countercorroborationism is forced to make about the future, given the evidence E of the success of its rival in the past, is stronger than the corresponding prediction of corroborationism in a quite clear and *completely non-inductive sense.*

If we concentrate initially on my first version of countercorroborationism (which just says, remember, that its overall – past and future – success rate is higher than the overall success rate of corroborationism) then it entails, given E, 'not merely that the success-rate of actions guided by hypotheses that are not well corroborated will in the future be higher than that of actions guided by well corroborated hypotheses, but that it will be *sufficiently higher* to offset its lower success-rate in the past. No counterpart of this is yielded by the corroborationist principle when conjoined with E'.[38]

FLOATER: I'm not at all clear what 'counterpart' means here, but why isn't the statement that the success rate of countercorroborationism will *not* be sufficiently high to offset its past record the required counterpart? (Or equivalently the statement that the future success rate of corroborationism won't be sufficiently bad to wipe out its present advantage.)

JOHN WATKINS: Well, I agree that the formal theory of counterparthood that I developed in *Science and Scepticism* for a restricted range of cases does not straightforwardly apply here.[39] But certainly you can't just assume that the negation of a sentence can count as its counterpart. A leading intuitive idea behind my account was that counterparts should be of roughly equal logical weight. Well, if you take a statement like Newton's principle of universal gravitation, for instance, then it is clearly altogether stronger than its bare negation.

FLOATER: We may need to come back to consideration of 'equal logical strength' later, but let's now be clear: your initial attempt to specify what it means for one prediction to be stronger than another has failed. You pointed out that countercorroborationism, together with E, has this consequence, let's call it C, about its future success rate being *sufficiently* high to offset its present disadvantage, and you said that this made the counter-corroborationist's claim about the future stronger, because no counterpart of C follows from corroborationism together with E. You refuse to allow the negation of C as the required counterpart. But since not-C clearly does follow from corroborationism plus E, and since the negation of the negation – C itself – cannot count as the counterpart within courtercorroborationism for not-C, we are back to a situation of complete symmetry between the two principles so far as consequences and counterparts go. Each has a consequence for which there appears to be no counterpart in the other.

JOHN WATKINS: Oh, but surely it is clear intuitively that the future success rate of countercorroborationism will be, not just higher than that of its rival, but *sufficiently* higher to offset its present bad record is a much stronger assertion than its negation.

FLOATER: O.K., but then you accept that your attempt to explicate 'strength' in terms of counterparts has (so far at least) failed, and you are thrown back on what are, on your account so far, *primitive* judgments of greater and less 'logical strength'. I would like to investigate the *grounds* for these judgments (I assume that you don't think they are primitive in some absolute sense.) Since they are explicitly *not* deductive judgments, what else can they rely on but *inductive* judgments about what is more or less likely, given what has happened in the past?

JOHN WATKINS: I don't really see why you have such trouble with this idea, but perhaps an analogy will help you.[40] Suppose two runners, A and B, are to race over a distance of 10 miles. You have no information in advance of the race – certainly none about the outcome of previous races. Initially then the two claims – that A will win, and that B will – surely stand on a par: they have equal logical strengths. They then start to race and at a certain stage you look to see how things are going: in fact A has covered 5 miles, B only 4. Given this evidence, the claim that B will win now surely has what is, other things being equal, a *stronger* consequence about the future than the claim that A will win. *Not* in some inductive sense, that in lots and lots of 10 mile races in the past anyone who has been 1 mile ahead at the halfway stage has won much more often than not, but in the simple non-inductive sense that, if you like, the prediction that B will win is, given the evidence, incompatible with more possibilities than the prediction that A will. The B-prediction now rules out, for example, more winning margins (counting this as the distance the loser is behind when the winner finishes): given the evidence the maximum winning margin for A is, of course, 6 miles (B would need to find himself unable to take another step), while the maximum winning margin for B is 5 miles.

FLOATER: Well, another of the books I was reading before Sir Karl arrived concerned difficulties with the 'classical theory of probability', difficulties with how to count up 'equal possibilities'. You may begin by describing the outcomes of some repeatable experiment using one set of basic events, which it seems reasonable to say are 'equally likely'. But then some one produces another description of physically the same set-up using quite different basic events, which however again seem intuitively equally likely. Following the classical account in the two cases and assigning in each case a uniform probability distribution over the 'basic events' leads to incoherence: the very same event (under of course two different descriptions) receives different probabilities in the two cases. Essentially the same problem has repeatedly come up in the recent history of inductive logic (and the recent history of the idea of verisimilitude). Now it's true of course that you yourself are not in the business of assigning probabilities (at any rate not officially). But you clearly *do* rely on partitioning the whole outcome space into some finite number of 'possibilities' each of which we are supposed to take as counting equally: so that if one prediction rules out more of these possibilities, as thus individuated, than does a rival prediction, then the first is 'stronger' than the second. But this suggestion runs into the same old problem: namely the existence of other ways of partitioning the outcome space into 'possibilities', other ways which yield different strength judgments. Why, for example, in your race case, should I not simply count the possibilities as the following: *A* wins having been ahead after 5 miles, *A* wins having been behind after 5 miles, *B* wins having been ahead after 5 miles, *B* wins having been behind after 5 miles? Given *that* partitioning into 'possibilities' (and who's to say that they aren't equally likely?) then the prediction that *A* will win and the prediction that *B* will are both consistent with two possibilities initially and the evidence rules out *one* possibility in each case. Thus on this partitioning, the two predictions will, on your account, remain equally 'strong' after the evidence is brought in.

Indeed, it seems to me that if, as you do, you rule out inductive considerations then there is no way of distinguishing between various different ways in which the race outcome space might be partitioned (even ones in which the prediction that the front runner will win turns out to rule out *more* possibilities[41]). And this means that the strengths of the two predictions should just on your account be regarded as incomparable. I think most people's intuitions (which, as I keep saying, do seem unfortunately to be inductivist) would tell them that (a) if they have seen lots of races of this or similar type in which someone so far ahead at half way eventually wins, and few, if any, in which he loses, then the prediction that *A* wins is indeed, given *E*, 'weaker', that is, more likely (inductively) to be correct; (b) if, on the contrary, they've seen lots of races in which someone so far ahead eventually 'blows up' and loses and few, if any, in which he wins then the prediction that A wins would, given E, actually be 'stronger' – less likely to be correct; and (c) if, as in your case, all inductive considerations are excluded so that no previous race outcomes can be taken into account, then they would (or at any rate ought to) say that the evidence fails to tell one way or the other (who knows, if he never saw a race before, whether a halfway lead is an advantage or a disadvantage?). It might seem natural to translate this failure to tell either way into the assertion that the two post-evidence predictions are equally strong – but this would be the analogue of the classical probability mistake: the only safe assertion seems to be that they are of non-comparable strength.

JOHN WATKINS: I'm still not sure that you have grasped the main point of my position; but, while I'm thinking about that, let me hear what you think about my treatment of the 'grueish' variant of countercorroborationism. This says, remember, that although following the best-corroborated theory in the past was more successful, following less well corroborated theories will be more successful in the future. Again it seemed to me that, in view of corroborationism's past success, this grueish view makes a stronger forecast about the future than any made by the corroborationist. It 'implies that a time will come when success rates change over, and, moreover, that *this time has come now*'. Corroborationism, on the other hand, 'says nothing corresponding to this'.[42]

FLOATER: Well, Goodman, of course, showed that this idea of a 'change' in the course of nature is language-dependent: native grue/bleen speakers would spot a 'change' tomorrow (or whenever) if emeralds, in our terms, continued to be green (for them, emeralds would have changed from grue to bleen). But laying this point aside for the moment, my reply is essentially the same as before. The corroborationist *does* say something corresponding to this change-over prediction, namely that the time has *not* now come for any change-over.

JOHN WATKINS: But, as I told you before, my notion of counterparthood explicitly excludes automatically counting the negation of a sentence as its counterpart. Surely you must admit that the statement that the time has *now* come for the change-over is much stronger intuitively than its negation. The negation is of course a very weak statement compatible not only with a change-over occurring at *any other* time than now, but also compatible with no change-over ever occurring.

FLOATER: Well again your original explication of 'strength of prediction' won't in fact deliver the judgment you want. Given that you disallow the negation from counting as counterpart to the 'time is now' prediction, then the corroborationist also makes a prediction for which there is no counterpart in countercorroborationism: the prediction that the time has *not* now come for a 'change-over' in success-rates. So far as your counterpart argument goes the two principles remain on a par after E is brought in: each entailing a consequence for which there is no counterpart entailed by the other.

So again you are forced back on intuitive, for you primitive, judgments about the relative logical strength of two contradictory propositions. You just take it as obvious that 'the time has *not* now come over for a change-over' is 'weaker' than 'the time *has* now come for a change-over'. You will not be surprised to learn that I don't share this judgment and that I shall argue that, although most people might well accept it, that acceptance is informed precisely their inductive preconceptions.

However, in order to develop my reply in detail I need to raise a few formal niggles so as to get clearer on what exactly your 'grueish' counter-corroborationist is saying. The talk of changing success rates is unclear. The success rate of countercorroborationism relative to corroborationism can't just change in an instant – in the clear cut case where we assume corroborationism has been uniformly successful so far, then even if in the very next case of a prediction we had a 'change' in that finally the best-corroborated theory got it wrong, and some hitherto badly corroborated theory got it right, then countercorroborationism would still have a long way to go to catch up. Moreover, assuming that we are not in this clear cut case – which your talk of success *rates* seems to imply – so that we have already some variability of outcomes, then we can't even talk of the success rates *starting* to change, since the success rates then change every single time a trial is performed. If we are just talking straight *percentages* of success (and therefore assuming, as seems plausible, that only finitely many predictions will ever be made in the whole past and future history of mankind) then although it may be true that the success rate for corroborationism, say, in the subpopulation of *past* predictions is different from the success rate in the subpopulation of *future* predictions, you can see that it is still not possible to talk of success rates 'changing' just now. Almost certainly in such a case the success rate for corroborationism in the subpopulation of predictions made earlier than 1985, say, will be different from the success rate in the subpopulation of predictions from 1985 onwards up till now *and* into the future. Why say that the success rates changed *now* rather than in 1985?

If you really want to have the countercorroborationist say that there will be a 'change' *now* then, so far as I can see, you have two options. First you could talk in *probability* terms rather than success rate terms; second, you could take it that we are talking only about well-corroborated low level empirical generalisations and take advantage of my concession that the corroborationist policy would in these cases have paid off in the past, not just more often than not, but 100% of the time – in that case you could have the grueish countercorroborationist say that the very next case was going to be the first exception.

So, consider the first possibility where the two rival principles are making different claims about the probability that a randomly selected prediction of a so far best-corroborated theory will turn out true. Let's simplify by assuming that there are only two possible outcomes – the best-corroborated theory's prediction turning out false, just in case the prediction of whatever theory the countercorroborationist favours is true. The 'grueish' countercorroborationist then allows that the probability *in the past* that the well-corroborated theory would be successful was at any rate greater than a half, but claims that a change is occurring exactly now in the probabilistic set-up so as to favour the less well-corroborated theory. (Of course how these, essentially *theoretical*, claims about probabilities are to be established is a tricky matter – all that we can 'observe' are your success rates. But let's not worry about that now.) The corroborationist, on the other hand, predicts no change in the probabilities.

JOHN WATKINS: Suppose I accept this account, then surely my case is made. You must agree that the claim that there will be a change in probabilities exactly now is stronger – other things being equal – than the claim that they will continue unchanged.

FLOATER: Well the same old point comes up again: the corroborationist principle *does* imply something about the present time, namely that the same probabilities will obtain and hence, if you like, that no change will occur in these probabilities. I just don't see how, in the explicit absence of any inductive considerations, you can say that one of these claims is 'stronger' than the other. If we look at the situation overall, each of the two positions entails equally precise claims about the probability of a certain outcome of a certain 'experiment' when that experiment is performed *at any given time*. Whether or not one of the claims seems 'stronger', that is, less likely to be true, can only be judged from the evidence: do we seem to live in a chancy, inconstant world or in a more regular world of seemingly fixed probabilities?

I need hardly say that this is an inductive consideration.

JOHN WATKINS: Suppose then I took your second sense in which the grueish countercorroborationist might be predicting a change just now. He allows that the success-rate for corroborationism so far is 100% but that the first exception is going to be the next instance (perhaps from there on in there are only 'exceptions'). Surely the uniformity claim is weaker than this claim that something different will happen in the very next instance.

FLOATER: The first part of my reply is that, as Goodman showed in his original paper and as I mentioned before, the talk of a 'change' occurring here is language-dependent (speakers of a language with 'grue' and 'bleen' as primitive predicates see a change if the next emerald is green). But secondly and more centrally, any talk of a 'change', even with respect to a given language, is derivative. Basically the two positions describe in equivalent detail what will happen in each observational trial. Say that the next action to depend on the prediction of some theory is my proposed descent from the top of the Eiffel Tower. The corroborationist and countercorroborationist make entirely analogous, but different predictions about what will happen: the corroborationist predicts roughly uniform acceleration, the countercorroborationist predicts, let's say, a gentle float. Since it is this latter prediction which, given E, entails the change claim, it must be at least as 'strong' as that change claim (given E). But then in the end you're going to have to say that the statement that 'the next freely falling body will achieve terminal velocity of '1″ per second' is, given the evidence, 'stronger' than the statement 'the next freely falling body will fall with roughly constant acceleration'. And the fact that *that* judgment of comparative strength has exclusively inductive credentials is surely patent.

JOHN WATKINS: It is important for me to speak about *overall* success and failure of the two policies and not to concentrate simply on the next instance. Let me have the assumption that the future will be finite so that I can avoid the difficulties with probabilities and stick to success-rates. I accept that I can't legitimately talk about success-rates 'changing now'. So let me amend the position of my grueish countercorroborationist: he now says simply that the success-rate of countercorroborationism within the (finite) set of future predictions will be higher than that of corroborationism within the same set.

FLOATER: Fine: but then – since you have dropped your idea that the grueish countercorroborationist claims that a change occurs now – I don't see even a *prima facie* case for his prediction about the future being any 'stronger', given the past evidence, than that of his corroborationist rival. Unlike the 'straight' countercorroborationist, he makes no claim about his future success rate being sufficiently high to offset his present bad record, he simply discounts the past. He and the corroborationist simply make equally precise but conflicting predictions about success rates in the future. Given that we are banned from making any inductive use of the past evidence to ground claims that one prediction is more likely than another, the two surely stand on a par.

JOHN WATKINS: Well, not quite. My 'grueish' chap says, remember, that his policy will definitely pay off in the future – he says that the success rate of countercorroborationism will be, not just greater than *or equal to* the success rate of corroborationism, but strictly greater than it. The past evidence allows the corroborationist, on the other hand, to rest on his laurels: he claims that the *overall* success rate of his policy is strictly greater than that of his rival, but his past success means that he need only claim that his success rate in the future will be *no worse* than that of his rival, he need not claim that his future success-rate will actually be higher. I agree that this means that the corroborationist's prediction is weaker only by a whisker: but then I always told you that my promise was only just to tip the balance in his favour.

FLOATER: Well in fact, if we go along with the finite universe assumption, your corroborationist – since he claims only that corroborationism will be ahead 'at the end of time' – could in fact allow that his success rate in the future will actually be *less* than that of the countercorroborationst, so long as it is only marginally less so that the big crunch will come before corroborationism's present advantage is finally eroded. But I always felt that your 'first past the post' formulation of the two positions didn't come to grips with the real problem.[43] I want to know why my proposed float is 'irrational'. Now even if you convinced me that there is some reason to think that the general corroborationist policy will have outscored its rival at the end of time, would this be enough to categorise my proposal to float as irrational? Surely not.

If corroborationism's prediction about the future really is compatible with less than 50% of *future* predictions from best-corroborated theories being correct, then how could it deliver the required irrationality verdict? If the corroborationist allows that it *may* be true that I have a slightly better than 50% chance of getting down safely by floating and that it *may* be true that there is a slightly worse than 50% chance that the predictions underlying the construction of the lift will hold in future instances, then how can it possibly deliver the verdict that everyone else seems so sure is the right one? Even if we turn back to probabilities and have your corroborationist say that there is at least a 50% chance of any future prediction of any best-corroborated theory being correct, then my proposal to float down can hardly be regarded as 'irrational' – remember that I am quite happy on every occasion to float or not, depending on the toss of a coin. In general, the intuitions of my opponents would surely be that, where an action's success depends on the outcome of a random process with equally likely outcomes, then it would not be irrational to base one's action in a given case on the prediction of *either* outcome. If in the future, playing 'corroborationist' or 'counter-corroborationist' is equally likely to pay off on average, then what seems to be indicated as 'most rational' is the 'mixed strategy' that I proposed, Certainly always playing 'corroborationist' is not indicated as the uniquely rational strategy (and indeed would surely stand on a par rationalitywise with the strategy of always playing 'countercorroborationist'). What you make the corroborationist say now seems to me so 'weak' that it fails to solve the problem even if established. To deliver the verdict you want, you need to make some positive (and not merely non-negative) claim about the future. Your corroborationist needs to assert *at least* that there is a better than 50% chance of his favoured predictions turning out correct in the future. But then we are back to complete parity between him and the grueish counter-corroborationist.

JOHN WATKINS: What you say would be correct, of course, if we *knew* that corroborationism would be correct only 50% (or marginally less than 50%) of the time in the future. But the whole problem was that we *don't* know what the future success rates will be. Any claim to know them, or even any claim that one set of future rates is more rationally credible (or whatever) than another, would be an acknowledgedly inductive assumption. And my position, remember, is 'neo-Popperian': I hold that *at most* we know accepted observation statements and deductive logic. Corroborationism's future success rate may, for all we know, be much higher than 50%, perhaps even 100% or close to it; but it is the (now known) fact that this future success rate *could* be only 50% (or slightly less) and the policy still be overall champion which, I claim, just tips the balance in favour of acting on the corroborationist principle.

FLOATER: Well, it *might* tip the balance in favour of the claim that corroborationism will be overall champion. (Although even this relies, as I said before, on how you count up possibilities.) But I just don't see at all how it tips the balance in favour of *applying corroborationism in future instances*, and this after all was the problem all along. If we go back to your race analogy, the analogue of the question about whether to take the lift or to the air is *not* the question of who will eventually win the race, but something like who will cover the next yard, say, the faster. And even if I accepted (which I don't) that his 1 mile lead at halfway tips the balance in favour of the prediction that A will win, this lead is, as I am sure you will accept, entirely irrelevant to the question of whether he covers the next yard faster.

This really brings me back to the general question of 'balance tipping'. As I said before, it is easy to cite 'balance tipping' factors in favour of inductivism or corroborationism. Almost everyone believes it, for example. Or why not come right out and say that what tips the balance in its favour is its past success (what you do say seems to amount in the end to not much more than this anyway)? But then the whole problem always was *why* this past success should tip the balance. In general I don't believe that you can really claim to have solved the problem of induction by citing a 'balance tipping' factor, unless that reason tells in favour of inductivism or corroborationism being correct at least more often than not *in the future*. Otherwise Popper would already have solved the problem which you yourself accept that he did not. Let's take it that Popper's system of corroboration contains no inductive element. What tips the balance in favour of the lift rather than my float? Well: the fact that Galileo's law is best-corroborated. This 'solution' does not presuppose induction, since the corroboration appraisal contains no inductive element. But Salmon's point (and others') was exactly that if this is meant to deliver the verdict that it is rational to apply one theory rather than others in the future, then Popper must presuppose that the so far best corroborated will continue to be better corroborated than its known rivals in the future. That is, he must presuppose the very point at issue. You yourself quote Salmon's argument at one point, I believe, and comment 'Game, set and match to Salmon'.[44] It seems that in the end he scores the same victory over you.

JOHN WATKINS:[45] Perhaps you're right: I've been banging my head against the same intellectual brick wall as Popper. Once it has been accepted that there is a deductive gap between past performance of a theory and its future performance, it has to be further accepted that, if we are going to deliver the verdict that your proposed float is irrational, we need to beef up our theory of rationality. We need, that is, to accept that neo-Popperianism or deductive rationality (observation statements plus deductive logic) is too weak to do the job. But if we are forced to incorporate substantive assumptions into our theory of rationality, we are not likely to find any more intuitively convincing or more plausible than the straight inductive assumption that, in certain restricted circumstances, it is reasonable to suppose that the future will be like the past. I just solve the problem by fiat and declare you irrational. By the way it strikes me that, although I've all along just taken *deductive* logic as given, if someone (I hear you have a *really* strange brother) took a similar line on deductive rationality, he would in the end force me into a similar apparently dictatorial 'irrational' position.[46] So maybe I shouldn't feel too bad about it.

But what are we going to do about you? I'm absolutely amazed that you have survived so far and quite convinced that you won't for much longer if you don't mend your ways. Pyrrho, the founder of one version of scepticism, is alleged to have actually lived the life of a sceptic as well as defended the philosophical doctrine. But I notice that he did take the precaution of surrounding himself with lots of disciples. My conjecture has always been that he would have made sure that he did not fully convince these disciples, so that whenever he headed for a cliff edge or whatever, claiming that there was no reason to think that he would come to any harm, there were always a few acolytes around whose 'emotions' got in the way of their 'reason' and who therefore pulled him back. Perhaps we can get a few apprentice philosophers to look after you – they can have fun arguing with you in the meanwhile.

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NOTES

[1] See Miller (1982) both for references to Popper's early and late critics, and for Miller's attempt to argue that these criticisms are invalid.

[2] Since this volume was to be kept as a surprise, I faced the prospect of publishing the present paper without the benefit of John Watkins's critical comments. Since this would have been both an unwelcome and entirely novel experience for me – he has been kind enough to read and helpfully criticise drafts of all my other work prior to publication – I resorted to subterfuge: I am glad to say that this paper too has been (substantially) modified as a result of his comments on an earlier version of Sections 1

and 2 delivered at a conference on Popper's philosophy and on an earlier version of Section 3 delivered at his seminar. Did anyone ever really need to be assured that a critic of an earlier draft is 'not responsible for the mistakes that remain'? If so, it is hardly likely to be in this case – I have no doubt that John will have at least as many disagreements with the new version as he did with the old. As always, though, I thank him for his comments (and for the help and encouragement he has given me over the years). Thanks for comments on an earlier version are also due to Colin Howson and Elie Zahar.

[3] Popper (1972), p. 1.

[4] Miller (1982), p. 18.

[5] I don't, of course, deny the possibility of the lift cable breaking or some other disaster. But should such a disaster occur it would not be put down to our having an incorrect appreciation of the laws of nature but instead to some 'natural' (and independently checkable) cause such as metal fatigue. This means that, had the equipment been checked just in advance of the disaster, then disaster could (at any rate in principle) have been predicted and hence averted. This is, then, quite different from our friend the floater's proposal which, if it led, as we all expect, to disaster would indeed be attributed to his incorrect appreciation of the laws of nature. Let's suppose, then, that some important dignitary is due to ascend the Eiffel Tower tomorrow, and that therefore the lift equipment has been exhaustively checked. Those who just can't get the fallibility of lifts out of their head should substitute slowly walking down the emergency stairs in thick rubber-soled shoes with various safety-harnesses attached.

[6] Russell (1948), p. 9: 'Scepticism, while logically impeccable, is psychologically impossible, and there is an element of frivolous insincerity in any philosophy which pretends to accept it'.

[7] Popper, *op. cit.*, p. 21.

[8] *Op. cit.*, p. 22.

[9] Salmon (1981), p. 122.

[10] Although I have given some of my characters real names, they are of course my own 'rational reconstructions'. Except where they have been deliberately caricatured for 'dramatic effect' (in places that should be fairly obvious), my reconstructions are intended to be accurate, as indicated by the quotations and references. But readers must judge for themselves how far my intentions have been realised; and should certainly remember that, except when explicitly quoting, I am indulging in the dangerous business of putting words in other people's mouths.

[11] Miller, *op. cit.*, p. 40.

[12] *Ibid.*

[13] *Op. cit.*, p. 38–40.

[14] The question of whether there is some cogent Popperian reason to discount gruesome hypotheses is the subject of Section 2.

[15] *Op. cit.*, p. 39.

[16] *Op. cit.*, p. 41: 'if we are sufficiently finicky, and *insist boringly* on the eternal possibility that nature will entirely change its course just before our action is performed, then no proposal [for action] whatever will get rejected [in the course of critical debate].' In case this seems to give the game away, I should add that Miller goes on to make it clear that one should be less 'finicky' and less 'boring' and

therefore reject all proposals *except for* the intuitively rational ones.
[17] Miller, quoting Larry Briskman, *op. cit.*, p. 11.
[18] This is, of course, one of the points made by Goodman (see below); for Miller on verisimilitude, see his (1974) and (1975).
[19] '[Popper's] answer manifestly involves no recourse to any principle of induction. There is no suggestion that the proposal that best survives criticism is one that we have any reason to expect to be successful; but this hardly means that it will not be successful' (Miller (1982), p. 40). Cf: 'The crux of the matter is that in order to provide genuinely interesting knowledge of the world inductivism needs to *assume* that there is some order and regularity in the world, whilst falsificationism *requires only* that there is some order and regularity in the world – but it does not need to make any sort of assumption to this effect'. (*Op. cit.*, p. 33; emphasis added.) I cannot for the life of me see what credit accrues from abstaining from an assumption, when one's theory requires the world to be such as to make the assumption true.
[20] In fact the only defensible thesis here, I believe, is that the (mathematical) structure of a rejected theory (though not its ontology) standardly lives on as a limiting case. For at any rate a partial articulation and defence of 'structural realism', see my (1989).
[21] See Feyerabend (1975), and for criticism (and elaboration of the distinction between 'crude' and 'scientific facts') my (1978) and (1981).
[22] For an instructive case, concerning the pressure of light, see my (1982).
[23] See, for example, Popper (1963), Chapter 10.
[24] 'The rationalist attitude is characterized by the importance it attaches to argument and experience. But neither logical argument nor experience can establish the rationalist attitude; for only those who are ready to consider argument or experience, and who have therefore adopted this attitude already, will be impressed by them...We have to conclude from this that no rational argument will have a rational effect on a man who does not want to adopt a rational attitude...But this means that whoever adopts the rationalist attitude does so because he has adopted, consciously or unconsciously, some proposal, or decision, or belief, or behaviour: an adoption which may be called irrational ["non-rational" would surely have been better].' (Popper (1945) II, pp. 230–1). Bartley's Comprehensively Critical Rationalism was for a while regarded by Popperians as providing an escape from having to admit that their rationality, like other forms, is based on a non-rational decision. But what exactly would it mean to have a criticism of the critical method? No one ever said – at any rate not coherently. It is surely clear that Popper was right that: 'a comprehensive rationalism is untenable' (*ibid.*)
[25] It will be clear that this is what I think should be said about the problem of induction – though it is not at all clear that it constitutes a 'solution' of the problem as opposed to an honest recognition of its insolubility. Again the position is hardly new; it was held for example by Russell:

What [Hume's] arguments prove – and I do think the proof can be controverted – is, that induction is an independent logical principle, incapable of being inferred either from experience or from other logical principles, and that without this principle science is impossible. (1946), p. 647.

[26] I need hardly, perhaps, say that at this point the amount of reconstruction (rational or irrational) becomes extreme!

[27] Miller, *op. cit.*, p. 39.
[28] *Ibid.*
[29] See Miller, *op. cit.*, p. 38 and note 85: 'Actually I think that this claim [that the 'natural' and 'gruesome' alternatives are equally corroborated by the evidence on Popperian criteria] is false, since a test for grueness involves not only a color test but also a date test'.
[30] See Miller, *op. cit.*, pp. 39–40. Miller refers to a similar position adopted earlier by Bartley (1981).
[31] For example, the 'Zahar–Worrall account' (for details see my (1985)).
[32] See Watkins (1984), pp. 342–8; and the 'simpler...and better version' in Watkins (1988).
[33] Watkins (1988), p. 18.
[34] 'Can a neo-Popperian like myself...give [a technologist] any reason why he should prefer the best-corroborated hypothesis?' *Ibid.*
[35] Watkins (1988), p. 18.
[36] *Op. cit.*, p. 19.
[37] *Ibid.* There are difficulties with Watkins' use of 'success rates' which, except in extreme cases, cannot of course be constant (see *below*). But laying these aside, John Watkins' claim here is ambiguous between (a) both the success rate in the past and the success rate in the future (assuming this notion is well-defined (*below*)) are higher for corroborationism and (b) taking both past and future successes into account corroborationism has the higher rate. John Watkins is, I think, inclined to plump for the latter construal. As Mr Floater points out (*below*), this means that, even if it were accepted that there is a reason to prefer corroborationism as thus construed, then the problem of induction would still not in fact be solved. The whole question, as normally understood, is about what it is rational to do *in the future* (or less grandly, in the next instance).
[38] *Ibid.*
[39] (1984), pp. 171–7. Watkins distinguishes 'congruent' and 'incongruent counterparts'. The former are simply pairs of *identical* consequences of two different theories and raise no problem. Our concern is of course with *incongruent* counterparts.
[40] Watkins, personal communication.
[41] Thus, for example, suppose the 'possibilities' are taken to be: (i) A wins having been ahead at halfway; (ii) A wins having been, at halfway, both behind and whistling 'Dixie'; (iii) A wins having been, at halfway, both behind and not whistling 'Dixie'; (iv) B wins having been ahead at halfway; (v) B wins having been, at halfway, both behind and whistling 'Dixie'; (vi) B wins having been, at halfway, both behind and not whistling 'Dixie'; (vii) deadheat. The prediction that A will win starts off entirely on a par with the prediction that B will – both excluding four possibilities. But bringing in E, which is just, remember, the information that A is 1 mile ahead at halfway (if one is allowed to bring in extra evidence then it is even easier to make the A prediction 'stronger' given the evidence), now makes the prediction that A will win 'stronger' on the Watkins account. This is because the 'A wins' prediction together with E is, of course, compatible with only one of the seven original 'possibilities' (possibility (i)), while the 'B wins' prediction, together with E, is compatible both with possibility (v) and with possibility (vi). (The dependence of the measures of logical strength developed in Watkins' [1985] on how the 'possibilities' are parti-

tioned was pointed out at several LSE seminars by Colin Howson.)

[42] Watkins (1988), p. 19.

[43] The ambiguity in the formulation of the corroborationist position, pointed to above n. 37, becomes significant here.

[44] Watkins (1984), p. 341.

[45] Once again the amount of reconstruction becomes extreme at this point.

[46] See Lewis Carroll's famous (1895) dialogue between Achilles and the Tortoise.

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