Some Remarks on the Pragmatic Problem of Induction

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i. Scope of Inquiry

It is the most striking thing about scientific explanation the fact that we use induction without believing in it. Hume tried to find a way of proving that inductive inferences with true premises would yield true conclusions.¹ We now know he did fail in such a quest, but he didn’t do so because there wasn’t any justification at hand, he failed because it is the function of deduction to prove the truth of conclusions on the basis of true premises. Induction has a different function. Given true premises, the inductive inference establishes its conclusion as probable. Perhaps Hume failed because he was trying to make induction into deduction, and, as Wesley Salmon once said, he succeeded only in proving the platitude that induction is not deduction.²

Karl Popper provided the seminal account of a problem that has caught on rapidly the induction debate, and he did so by claiming that any intended solution to it was beyond the scope of his theory. Such problem could be illustrated with the next question: Are there any reasons for practical action we can rely on without appealing to some dreaded principle of induction at the kernel of Goodman’s paradox? Or, in other terms: given the fact that corroboration appraisals tell us nothing about future performance, why is it rational to rely one’s practical decisions upon the best-tested and corroborated theories, if there are no good

reasons for expecting that it will be a successful choice? In the next few pages I should devote myself to providing an answer to those questions. I will intend such a thing, by claiming that although the best corroborated theses provide us no reasons whatsoever with regards to its own truth, and thus we cannot properly ‘justify’ this or any theory, we may well still justify our preference for any of such theories. A theory being better corroborated doesn’t tell us that it is closer to the truth than its counterparts, but that it fulfils the optimum for science better than they do. As a way of entry into these issues I will begin with an a priori objection presented by Worrall to any attempted solution to the problem at hand. It may be put like this: If it is not to beg the question a solution must not bring in any inductive assumption; but if it is to succeed it will endorse some kind of inductive principle which it could not do without bringing in some inductive assumption. One would avoid this trap if one came up with an argument that, in terms of the above-mentioned distinction, provided no justification for any inductive principle in the sense of arguing for its truth, but did nevertheless provide the decision-maker with some positive reason for preferring an inductive to a counter-inductive strategy. The whole point of this paper is enquire how that meta-statement justifying a preference and not a theory itself, comes to be. The result should replace mixed talk about frequencies and probabilities by uniform talk about success-rates.

ii. A Fresh Look at the Problem

There is a sharp a distinction stressed by Popper between the problem which faces a theoretical scientist trying to select, out of several competing theories, the one that best fulfills the aim of science, and the pragmatic problem which faces an applied scientist or practical decision-maker trying to select, out of several competing hypotheses, the one that offers the best guidance. If Popperian philosophy of science provides an account of scientific rationality only at the theoretical level and not at this pragmatic level, then if I may paraphrase C.D. Broad’s famous verdict, it is bound to admit induction, although no longer the glory of science, still the scandal of philosophy.

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4 This is distinction was proposed by Popper, who expressed it by saying: ‘Although we cannot justify a theory ... we can sometimes justify our preference for one theory over another; for example if its degree of corroboration is greater’. Karl Popper, Unended Quest, Glasgow: Fontana/Collins, [1976], p. 104.

5 Popper describes the problem as follows: “Which theory should we prefer for practical action, from a rational point of view? (...) [W]e should prefer as basis for action the best-tested theory”. There are several controversial claims in Objective Knowledge that are anything but settled after 30 years, and deserve more discussion than they have yet received. One is the solution to the pragmatic problem of induction that is outlined in §9 of Chapter 1. At the time that Objective Knowledge was being assembled, in the summer of 1971, Popper was labouring over his extensive ‘Replies to My Critics’ (1974) in The Philosophy of Karl Popper, a volume in The Library of Living Philosophers edited by Paul Arthur Schilpp. In introducing Part III of these Replies, which was devoted to a handful of essays about the problem of induction, Popper took the opportunity to reproduce some parts of the present Chapters 1 and 2, including the section in question. See Karl Popper, Objective Knowledge: An Evolutionary Approach, Oxford: Clarendon Press, 1972, pp. 21-22.

6 I cannot resist quoting the amusing passage from which this is drawn in full: ‘[D]id Bacon provide any logical justification for the principles and methods which he elicited and which scientists assume and use? He did not, and he never saw that it was necessary to do so. There is a skeleton in the cupboard of Inductive Logic, which Bacon never suspected and Hume first exposed to view. Kant conducted the most elaborate funeral in history,
If one is to render a solution to the theoretician’s problem of rational theory-preference by any set of stratagems amounting to the truth of any of such theories, one has not thereby rendered a solution to the pragmatic problem. The fact that corroboration is non-inductive means that there is no carry-over from a solution of the theoretician’s problem to the problem under scrutiny here, because corroboration appraisals are no more than historical reports about the past performances of the theories in question, appraisals from which no inferences may be drawn about their future performance. What an agent wants of the factual hypotheses that guide his practical decision-making is not explanatory power, theoretical unity, probability or indeed verisimilitude, etc., but reliability; however, concerning reliability, the Popperian advice is clear enough: Abandon all hope. But this should not dishearten us an ounce. Of course no factual hypothesis can be absolutely reliable in the sense that it is guaranteed never to let you down, but we may suppose that our decision-maker asks only for comparative reliability: Given two competing hypotheses relevant to his practical problem, he wants to know which is the more reliable in the sense of being the more worthy of being relied upon. Such weakening of our position shouldn’t undershoot our mark.

While trying to solve any inductive problem at hand we might surely find ourselves between a rock and a hard place. There’s a dilemma in the centre of such enterprise vigorously pointed out by Worrall, and it consists in either failing or else begging the question. In order to see how that happens, the concepts under scrutiny in the face of our problem should be illustrated as in the following schema

\[ J \]
\[ \downarrow \]
\[ IP \]

and called Heaven and Earth and the Noumena under the Earth to witness that the skeleton was finally disposed of. But, when the dust of the funeral procession had subsided and the last strains of the Transcendental Organ had died away, the coffin was found to be empty and the skeleton in its old place. Mr Johnson and Mr Keynes may fairly be said to have reduced the skeleton to the dimensions of a mere skull. But that obstinate caput mortuum still awaits the undertaker who will give it Christian burial. May we venture to hope that when Bacon’s next centenary is celebrated the great work which he set going will be completed; and that Inductive Reasoning, which has long been the glory of Science, will have ceased to be the scandal of Philosophy? *Ethics and The History of Philosophy*, London: Routledge,1952, p. 142-3. These are the concluding words of his commemorative address on the occasion of The Bacon Tercentenary, 5 October 1926.

Where ‘IP’ stands for an inductive principle which, if it could be justified, would solve the pragmatic problem of induction by projecting any given evidence from the past to the future, ‘J’ a putative justification for IP;10 and ‘↓’ whatever support J provides for IP, which is a matter of degree, ranging from 0 to 1. ‘0’ amounts to no support, and ‘1’ signifies proof in the sense that, if J were known to be true then the truth of IP would be guaranteed. Suppose J entails IP and the degree of support is 1. In order to entail IP this J must contain all the content of IP, and therefore of course J will stand in at least as much need of justification as IP; here we completely beg the question. And I this is precisely what Miller did. In Miller’s argument, the hypothesis that has most successfully survived critical debate is our best source of information about the world, and there is little sense in ignoring this information and there are no reasons for supposing that an unfuted theory is not true. ‘You cannot do better’ he says ‘than act in the light of what is true’.11 Agents do not, he continues, act on the basis of scientific theories in practical affairs. Thinking that there is any theory that tells us how a bridge is to be built, ignores that universal statements proscribe but do not prescribe (they point out what cannot be achieved). We do not rely upon scientific hypothesis when we act, we exploit them; we take the scientific law as granted, and look for the best initial conditions. Nevertheless, he later admits, science does not provide an assemblage of initial conditions that will suit our goal. Miller suggests a rule of rational action: The rational thing to do is to follow that proposal that best survives the most searching criticism that can be directed against it. Though, he warns while following the Popperian caveat, we should not thereby believe that the hypothesis which best survives criticism is reliable. Miller’s is a kind of negative argument: The lack of reason to believe in its success does not mean that the proposal will not be successful, the rationality of decision-making is to favour the proposals that have best survived criticism and not those we expect to be successful. As I claimed before, this argument commits itself to question begging. By trying to insist on Popperian corroborlation in order to avoid rhetoric about success-rates, it incurs in an inductivist assumption, namely, that acting in the light of what was true in the past is a better guide for the future than what wasn’t.

Let's examine next the second head of our Cerberus. Imagine $J$ is a tautology, and therefore none of the content of $IP$ is contained in $J$. Though not begging the question, here we completely fail to justify $IP$, because the degree of support provided by this $J$ has dropped to 0, and that amounts to, of course, no degree of support at all. Conversely, any $J$ that provides a non-zero degree of support for $IP$ will contain at least some of $IP$'s inductive content and will to that extent beg the question.

How can we avoid this dilemma—and consequently provide a basis for an argument that might solve the problem at hand? There is indeed an strategy, and we might like to look it up in Popper himself: 'Although we cannot justify a theory (...) we can sometimes justify our preference for one theory over another; for example if its degree of corroboration is greater'.\(^{12}\) We can make use of Popper's idea that considerations may justify a preference for a theory although they provide no justification, in the sense of having some tendency, however slight, to establish it as true, for the theory itself. Corroboration appraisals of a theory $T$ through non-inductive lenses, have no tendency to establish $T$ as true over its defeated rivals, but it does justify us in preferring $T$ to its rivals since it tells us (on that uncontroversial assumption mentioned earlier) that $T$ is the theory in its field at the present time that 'best fulfills the optimum aim for science'.\(^{13}\) This idea of justifying a preference for a proposition without justifying the proposition itself could be extended to our case where such proposition is an to the case where the proposition is an inductive principle which, explicitly or by implication, advises the decision-maker that well-corroborated hypotheses are his best way out of the dilemma. So, if one came up with an argument that, in terms of Popper's above-mentioned distinction, provided no justification for any inductive principle in the sense of arguing for its truth, but does nevertheless provide the decision-maker with some positive reason for preferring an inductive to a counter-inductive strategy, we would had then avoided the pragmatic trap.

This strategy is exposed to the next objection: could we possibly justify a preference for $IP$ without justifying $IP$ itself? If we are prone to say $IP$ is better to any other principle, because—in Watkins' terms—it provides the weaker forecast about the future, we are of course covertly justifying $IP$ itself by implying that it makes, on the face of some given evidence, the more probable forecast (involving an inductive assumption). Such an argument begs the question. Do we have any option at hand? According to John Stuart Mill, matters of fact (about the world) must be settled by 'direct appeal to the faculties which judge of fact—namely, our senses, and our internal consciousness.'\(^ {14}\) More precisely: one cannot prove by reasoning that first principles are true. This goes for scientific knowledge, and for ethics. Remember his rejection of intuitionism—one of the most famous, and disputed, statements to come from his pen:


[T]he only proof capable of being given that an object is visible, is that people actually see it. The only proof that a sound is audible, is that people hear it: and so of the other sources of our experience. In alike matter….the sole evidence it is possible to produce that anything is desirable, is that people do actually desire it.\textsuperscript{15}

He might have said in similar vein that the sole evidence it is possible to produce that X is preferable to Y, is that virtually everybody does actually prefer X to Y. Of course, this \textit{de facto} preferability does not make X \textit{de jure} preferable to Y. There is no doubt that IP \textit{de facto} preferable to CIP. But in drawing attention to this we are not providing any justification for IP itself, since it does not make X \textit{de jure} preferable to Y. Frank Ramsey argued well in the same line of thought, though perhaps too brisk for most philosophers—despite the subsequent work by Richard Braithwaite—,\textsuperscript{16} by saying as early as 1926 that ‘we are all convinced by inductive arguments, and our conviction is reasonable because the world is so constituted that inductive arguments lead on the whole to \textit{true opinions}’.\textsuperscript{17}

iii. \textit{Theory Content}

Until now, I’ve been arguing that a solution to the pragmatic problem is at least tractable—despite such a claim is not, as already shown, an altogether uncontested ground. Starting from the state of affairs in which this discussion was left after Watkins, Worrall, Musgrave, Howson, Goodman, Gower, Bamford, Nordin, Salmon and Miller, peered into it, I should like to essay out the best possible strategy to yield a solution for the problem at hand. Needless to say, my argumentation will indeed turn heterodoxical.

In the realm of rational choice under conditions of uncertainty, traditional Decision Theory has provided us with two contrasting approaches. First, the Bayesian approach, where the purpose is to maximize expected utility, by attaching probabilities to the possible outcomes of the various decisions opened.\textsuperscript{18} ‘This entails peering into the future, lackadaisical as it may sound. The second approach is brought to us by the Minimax theorem, where you seek to

\textsuperscript{15} Mill, \textit{op. cit.}, (I-3).
\textsuperscript{17} My italics. See Ramsey, Frank P., “Truth and Probability,” in Richard B. Braithwaite (ed.), \textit{Foundations of Mathematics and Other Logical Essay} (London: Routledge and Kegan Paul; Check on 1931 publication date), pp. 156-198. It seems to me that Ramsey is arguing in the same line as Mill did once, by saying: 'It is not necessary even to the perfection of a science that the corresponding art should possess universal, or even general rules. The phenomena of society might not only be completely dependent on known causes, but the mode of action of all those causes might be reducible to laws of considerable simplicity, and yet no two cases might admit of being treated in precisely the same manner. So great might be the variety of circumstances on which the results in different cases depend, that the art might not have a single general precept to give, except that of watching the circumstances of the particular case, and adapting our measures to the effects which, according to the principles of the science, result from those circumstances. But although, in so complicated a class of subjects, it is impossible to lay down practical maxims of universal application, it does not follow that the phenomena do not conform to universal laws'. See John Stuart Mill, \textit{A System of Logic: Ratiocinative and Inductive}, London: Longmans, 8th ed.,Book VI, Chapter VI, Section 1, 1843 (1961), p.572
minimize your losses, and instead of trying to peer into the future, you consider the worst possible outcome of each of the various courses of events open to you; therefore you choose the one with the best "worst".\footnote{John von Neumann: "Zur Theorie der Gesellschaftsspiele", \textit{Mathematische Annalen}, vol. 100, (1928), pp. 295–300} Nevertheless, Minimax is not entirely capable of eliminating dependence on assumptions about the future, for the "worsts" are not supposed to be mere logical possibilities and some inductive assumptions would be needed to assess real worst possibilities. But this shouldn’t discourage us; we may still say that the Minimax principle is tantamount to a maxim of prudence which reduces inductive assumptions without entirely dispensing with them.\footnote{John Watkins, 'The Pragmatic Problem of Induction', \textit{Analysis}, Vol. 48, No. 1 (Jan., 1988), p. 19} A maxim of prudence which dispenses with any inductive assumption and nevertheless favours IP against possible alternatives would provide a solution of the pragmatic problem of induction. Do we have such a maxim at hand?

Let’s see how this theory applies to our problem by bringing about a thought-experiment proposed by Watkins:

Suppose that you are planning a holiday outing for tomorrow. You are in a district where the weather is rather variable and unpredictable. You are considering two alternatives, A and B. For A to be a success, only one weather condition must be satisfied: it mustn’t be windy. For B to be a success, it must be neither windy nor rainy. Either outing will be equally enjoyable if not spoilt by the weather, and equally unenjoyable if so spoilt. (…) Now suppose that you have a choice between A and an equally enjoyable/unenjoyable alternative B’, and that for B’ to be a success it mustn’t rain but it must be windy (perhaps it’s a surfboarding expedition). In this district, with its highly unpredictable weather, windy days are about as frequent as non-windy ones.\footnote{Watkins, \textit{op. cit.}, p 18-19}

Minimax theorist will say it is rational, other things being equal, to choose that course whose success presupposes the weaker forecast about the future. For a weaker forecast is less likely, other things being equal, to turn out wrong than a stronger one. I will use ‘A-forecast’ to denote that statement about the future, which needs to be true if A is to be successful; likewise for ‘B-forecast’. The maxim, ‘Utilities being equal, prefer the course of action which presupposes the weaker forecast’ would enable you to settle the issue between A and B without forming any opinion about what the weather will in fact be on the day: you should obviously prefer A, since the A-forecast, namely ‘It will not be a windy day’, is strictly entailed by the B-forecast, namely ‘It will not be a windy or a rainy day’.

\begin{figure}[h]
\centering
\begin{tikzpicture}
  \node (A) at (-1,0) {A};
  \node (B) at (1,0) {B};
  \node (U) at (0,-1) {U};
  \path[set fill color=black!20] (A) -- (B) -- (U) -- cycle;
\end{tikzpicture}
\end{figure}
Watkins later considers a further possibility B’, whose utility, if successful, would equal the utility of A or B if successful. The B’-forecast is: ‘It will be windy and not rainy’. Suppose that the line between ‘windy’ and ‘not windy’ has been so drawn that ‘It will be a windy day’ and ‘It will not be a windy day’ are found equally informative, rather as ‘The switch is on’ and ‘The switch is off’ might be found equally informative. The claim that, although not entailed by it, the A-forecast is weaker than this B’-forecast might be argued along the following lines.

Suppose that the line between ‘windy’ and ‘not windy’ has been so drawn that ‘It will be a windy day’ and ‘It will not be a windy day’ are found equally informative, rather as ‘The switch is on’ and ‘The switch is off’ might be found equally informative. The claim that, although not entailed by it, the A-forecast is weaker than this B’-forecast might be argued along the following lines.

The A-forecast, ‘It will not be windy’ has a counterpart within the B’-forecast with which it can be paired off as equally informative, namely ‘It will be windy’. But the B’-forecast has a component, namely ‘It will not be rainy’, which has no counterpart in the A-forecast. It would again be rational for you to choose A, since the success of B’, like that of the previous B, requires the fulfilment of two conditions, not-C1 and C2, whereas A only requires that of C1, and you have no reason to regard C1 as less likely than not-C1.

Taken up to this point, we now face the problem of content comparison among incompatible and yet competing theories. Worrall is deeply pessimistic about the possibility of content-comparisons except in cases where strict entailment or containment obtains: ‘[P]artition the outcome-space one way and you get an ordering but one that can be reversed if you partition it another way’.22 While Ramsey, on the other hand, showed how parts of theories are not ‘strictly propositions by themselves’ since they contain variables, and their meaning ‘can only be given when we know to what stock of “propositions” (...) [they are] to be added.’23 Since this makes theoretical statements in rival theories incomparable, ‘the adherents of two such theories could quite well dispute, although neither affirmed anything the other denied.’24 This both explains the phenomenon of ‘incommensurability’,25 and limits its consequences, for example for deductive accounts of theoretical explanation;26 for as Ramsey remarks, it does not affect reasoning within the scope of a single theory’s quantifiers.27 But we are here

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22 Worrall, op. cit. p. 284
24 ibid p 133
27 For the sake of the argument, I should leave aside the following problem: Scientific theories apply new predicates to unobservable entities, like photons, to explain observable, for example optical phenomena.
concerned with comparing two or more theories which stand in a relation of counterparthood. Although such thing sounds like trying to fit a square peg in a round hole, the way of doing so is easily illustratable.

In order to compare incompatible theories for content, we might rely—following Watkins’ Science and Scepticism, hereinafter ‘SS’—in the following pattern, where $T_1$, has a counterpart $T_1'$ whose content is strictly contained in that of $T_2$, then $T_2$ has, in an objective and non-arbitrary sense, a greater content than $T_1$, where ‘$\approx$’ means ‘equal in amount’, ‘$\subset$’ means ‘strictly contained in’, and ‘$<$’ means ‘smaller than’:

\begin{enumerate}
  \item $\text{Ct}(T_1)\approx\text{Ct}(T_1')$
  \item $\text{Ct}(T_1')\subset\text{Ct}(T_2)$
  \item $\text{Ct}(T_1)<\text{Ct}(T_2)$
\end{enumerate}

This pattern doesn’t provide a guide for cases where a later scientific theory $T_2$ both revises and goes beyond its predecessor $T_1$. That can be shown by introducing to the debate the Kantian distinction between congruent and incongruent counterparts. Kant introduced it in connection with geometrical figures. In plane geometry any two triangles whose angles and sides are equal are congruent: one could be fitted over the other so that they coincide. But in spherical geometry there can be two triangles, one in the northern and one in the southern hemisphere, whose angles and sides are equal, so that they are counterparts of one another, but which could not be made to coincide, being mirror-images of one another. These Kant called incongruent counterparts.

In order to represent cases in which a later scientific theory $T_2$ both revises and goes beyond its predecessor $T_1$, we could let $T_1$ be ‘All computing machines are electric-wired’ and $T_2$ be

\begin{enumerate}
  \item do these predicates acquire empirical meaning? Ramsey’s drastic answer in ‘Theories’ (1929) is that there are no such predicates: we use ‘is a photon’, ‘has frequency $n$’, and so on not as predicates but as existentially bound variables. That is, a theory tacitly starts with quantifiers, ‘properties exist—call them “being a photon”, etc.—such that…’, followed by the explicit theory, in two parts. Its axioms link its predicate variables to each other, while its dictionary links them to observable predicates like ‘is red’. Thus if ‘$a$’, ‘$b$’ and ‘$g$’ are our theoretical predicates, ‘the best way to write our theory seems to be… ($\exists a,b,g$):dictionary.axioms’. This, which is now called the ‘Ramsey sentence’ of the theory, eliminates its problematic predicates while keeping its structure and observable consequences.

\footnotesize
30 “If two figures drawn on a plane surface are equal and similar, then they will coincide with each other. But the situation is often entirely different when one is dealing with corporeal [bodies] (…) They can be exactly equal and similar, and yet still be so different in themselves that the limits of the one cannot also be the limits of the other (…) [T]he most common and clearest example is furnished by the limbs of the human body (…) The right hand is similar and equal to the left hand. And if one looks at one of them on its own, examining the proportion and the position of its parts to each other, and scrutinising the magnitude of the whole, then a complete description of the one must apply in all respects to the other, as well. I shall call a body which is exactly equal and similar to another, but which cannot be enclosed in the same limits as that other, its incongruent counterpart”. Cfr. Kant, I. (1992), Concerning the ultimate ground of the differentiation of directions in space, in D. Walford & R. Meerbote, eds, ‘The Cambridge Edition of the Works of Immanuel Kant. Theoretical Philosophy, 1755–1770’, Cambridge University Press, Cambridge, pp. 369-70.
\end{enumerate}
‘All personal-computers are electric-wired if desktop and non-electric-wired if laptop’. The point to make here is that, although $T_2$ does not entail $T_1$, it seems intuitively to be the stronger hypothesis. Can we explicate this? Assume that ‘electric-wired’ and ‘non-electric-wired’ have been defined analogously to ‘windy’ and ‘not-windy’ above. We could re-write what our two theories say about computing machines as follows:

$T_1$: ‘All desktop computers are electric-wired and all laptop computers are electric-wired’

$T_2$: ‘All desktop computers are electric-wired and all laptop computers are non-electric-wired’.

We can now justify the intuition that $T_2$ has more content than $T_1$, on the ground that the content of $T_1$, has counterparts, congruent or incongruent, in $T_2$, while some of the content of $T_2$ (what it says about rodents other than mice) has no counterpart in $T_1$.

iv. Corroboration

Let us come back again to the pragmatic question. Consider a technologist who confronts a practical problem upon which several rival hypotheses bear, each indicating a different solution. One of these is better corroborated than its rivals. Can a Popperian, who seeks to exclude all non-deductive inferences and who insists that corroboration appraisals of hypotheses have no implications for their future performance, give him any reason why he should prefer the best corroborated hypothesis?

In SS Watkins presented a solution relying on the historical assumption that well corroborated hypotheses have hitherto proved better guides for practical decision-makers than have hypotheses that were not well corroborated. After all, the Humean problem of induction is not about the reliability of our knowledge of the past. Hume did not challenge the proposition that it has paid better to be guided in one’s actions by a principle of induction than by some alternative principle. His problem was: is there any reason to act on the supposition that it will continue to pay better in the future?

Watkins presented a corroborationist ‘IP’ to which he gave a statistical-cum-probabilistic character: It said that actions guided by hypotheses that are well corroborated have a better chance of being successful than actions guided by hypotheses that are not well corroborated. This was juxtaposed with various counter-inductivist principles which said that actions guided by hypotheses that are, say, chosen by lot, have a better chance of being successful than actions guided by hypotheses that are well corroborated. By doing so, Watkins actually restricts the discussion to a binary choice, by considering just the alternative general strategies: (i) rely on well-corroborated theories, and (ii) use a lottery to determine your choice of hypothesis about the consequences of performing particular actions. Let us not quarrel with this restriction of the problem, but see how it continues. He later introduces evidence, ‘E’ which said that, in the past, actions guided by hypotheses that are well corroborated have been successful more frequently than actions guided by hypotheses that
are not well corroborated. In order to take into account some well-pointed criticisms, Watkins introduced important variations in a later published rejoinder. Without introducing any unfairness, he simplifies the exposition this time by letting $E$ say that, in the past, well corroborated hypotheses have never let any agent down. $IP$ projects this into the future, while our counter-inductive principle $CIP$ projects the contrary into the future. Let $t_0$ denote now, and suppose that $n$ instances have been observed by $t_0$. Then we may represent,

(i) $E$ by ‘Yes (1,2, … $n$)’;
(ii) $IP$ by ‘Yes (n+1, n+2, …)’; and
(iii) $CIP$ by ‘No (n+1, n+2, …)’.

Putting this all together at work, he offers the next thought experiment. At $t_0$, suppose you confront a choice, if you accept ‘Yes (n+1)’ you should act in one way whereas if you accept ‘No (n+1)’ you should act in a quite different way. $IP$ and $CIP$, considered by themselves, are incongruent counterparts with equal amounts of content. But now conjoin $E$, which both sides accept, with each of them. Conjoining it with $IP$ yields ‘Yes (1, 2, … n, n+1, n+2, …)’. Conjoining it with $CIP$ yields ‘Yes (1, 2, … n), No (n+ 1, n+2, …)’.

There is a difference when it comes to conjoining $CIP$ with $E$ and $IP$ with $E$, this difference resides in the fact such correlation implies that (i) a discontinuity will occur, and (ii) it will occur in the $n+1$th instance. Watkins adds this also implies that in the $n+1$th instance a new era will be inaugurated.

$IP$ does imply something about this instance, but what it implies, namely that it will not inaugurate a new era, does not single it out from any other instance. In singling out this particular instance $CIP$ does something that has no counterpart in $IP$. Therefore, it is possible to claim that a counter-inductivist principle in the presence of $E$ makes a claim about the future that is stronger than that made by a corroborationalist $IP$ in the presence of $E$. The above-mentioned maxim of prudence says that we should prefer, other things being equal, the course of action which presupposes the weaker forecast. Whether or not other things are equal in the present case, they surely do not favour the counter-inductive side of the dispute. So, we can conclude, there is a consideration which justifies a preference for $CP$ without justifying $CP$ itself. The trick is to make use of $E$ in a non-inductive manner.

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31 The original account went: ‘Now consider two rival principles. One says that, during the whole history (past and future) of mankind, the overall success-rate of actions guided by well corroborated hypotheses is higher than that of actions guided by hypotheses that are not well corroborated. We may call this a corroborationalist principle. The other, which we may call a counter-corroborationalist principle, has two versions. In the first, it says that, during the whole history of mankind, the overall success-rate of actions guided by hypotheses that are not well corroborated is higher than that of actions guided by well corroborated hypotheses. This version is simply the contrary of the corroborationalist principle. In the second version, it says that during the future history of mankind the overall success-rate of actions guided by hypotheses that are not well corroborated will be higher than that of actions guided by well corroborated hypotheses. This version is a ‘grue-ish’ variant of the corroborationalist principle.’ See John Watkins, ‘The Pragmatic Problem of Induction’, *op. cit.* p. 19. The new account of the problem is in ‘Scientific Rationality’, *op. cit.*
v.  **Concluding Remarks**

I have tried to argue that there is a non-inductive way out of the pragmatic problem of induction, within the grounds of Critical Rationalism. As Popper once said, although the best corroborated theses provide us no reasons whatsoever with regards to its own truth, and thus we cannot properly ’justify’ this or any theory, we may well still justify our *preference* for any of such theories. The decision maker seeks for reliability, and of course no factual hypothesis can be absolutely reliable in the sense that it is guaranteed never to let you down, but we may suppose that our decision-maker asks only for comparative reliability: Given two competing hypotheses relevant to his practical problem, he wants to know which is the more reliable in the sense of being the more worthy of being relied upon. Such weakening of our position may still provide the decision maker with some *positive reason*, which is at the same time non-inductive, in order to yield a solution to the problem at hand. Once the debate resigns mixed talk about probabilities and frequencies, and replaces it by uniform talk about success-rates, the problem can thus be solved.

vi.  **Coda: Of Mice and Men**

There is a line of argument which I couldn’t essay in the length of this work. Such a line recurs to epistemology instead of Decision Theory, and as such, poses the pragmatic question in a rather different way: it isn’t about how should we best warrant (non-inductively) our beliefs, but rather what is it about a belief that deserves to be warranted. There seems to be an enormous asymmetry between some of our daily beliefs, the fact that the ceiling under which I am writing these lines will not fall in the next few hours doesn’t to be as present in my actual state of mind, as the doubt whether Higgs boson will continue to show up in further scientific inquiries. Such stratagem requires very careful examination of quintessential epistemological issues, such as knowledge being justified-true-belief, the Gettier-problem, issues of evidence, and the problem of externalism vs. internalism. Only then, only when we arrive to the point in which we can integrate epistemology and philosophy of science in a proper way tantamount to yield a satisfactory solution to this problem, perhaps we might finally claim that the best-laid plans of mice and men *shan’t* go astray.
vii. References


_________ ‘On Vindicating Induction,’ Philosophy of Science, 30(3). 1963.


