

KARL POPPER'S SOLUTION TO THE PROBLEM OF INDUCTION AND THE NON-JUSTIFICATIONIST CONCEPTION OF RATIONALITY

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Abstract. The article provides a detailed account and elucidation of Karl Popper's solution to Hume's problem of induction. It is pointed out that the solution has two major aspects. The first, explicitly described by Popper as his solution to the problem of induction, is the replacement of the inductivist account of the development of empirical knowledge, according to which cognition begins with observations of particular events and proceeds through inductive inferences to certainly true or highly probable theories-generalisations, with the hypothetico-deductivist account, according to which cognition begins with a problem and proceeds through conjecturing its possible solutions (advancing hypotheses) and attempts to falsify them by reproducible results of observations/experiments. The second aspect has to do with the problem of justification of the hypothetico-deductivist account (which replaces Hume's problem of the justification of induction). This problem is shown to be dealt with within Popper's-Bartley's general solution to the problem of justification, usually described as «non-justificationism», which admits the impossibility of absolute definitive justification (for any position) and replaces the search for such justification with the evaluation of relative advantages/disadvantages of competing approaches, which can provide us with reasons to prefer or tentatively accept one of them. The comparison is made between Popper's hypothetico-deductivist account and Charles Pierce's account based on abduction, or inference to the best explanation. It is shown that these accounts has similar logical structures, that with respect to empirical science they suggest mutual corrections and clarifications, and that inference to the best explanation can provide justification for the assumption of the existence of laws of nature, which is implicit in the hypothetico-deductivist account.

Keywords: induction, justification, hypothetical-deductive method, falsificationism, abduction, Popper, Bartley, Pierce.

РОЗВ'ЯЗАННЯ ПРОБЛЕМИ ІНДУКЦІЇ КАРЛОМ ПОПЕРОМ ТА НЕДЖАСТИФІКАЦІОНІСТСЬКА КОНЦЕПЦІЯ РАЦІОНАЛЬНОСТІ

Анотація. У статті докладно розглянуто та висвітлено розв'язання проблеми індукції Г'юма, запропоноване Карлом Поппером. Зауважено, що

це розв'язання має два головні аспекти. Перший, експліцитно описаний Поппером як його розв'язання проблеми індукції, полягає в заміщенні індуктивістської концепції розвитку емпіричного знання, згідно з якою пізнання починається зі спостережень і через індуктивні умовиводи має досягати гарантовано істинних або високоймовірних теорій-узагальнень, на гіпотетико-дедуктивістську концепцію, відповідно до якої пізнання починається з проблеми і здійснюється через висунення здогадок, гіпотез щодо можливого її розв'язання та через спроби спростування цих гіпотез відтворюваними результатами спостережень/експериментів. Другий аспект стосується проблеми джастифікації (обґрунтування-виправдання) гіпотетико-дедуктивістської концепції, що постає на місці Г'юмівської проблеми джастифікації індукції. З'ясовано, як ця проблема трактується в контексті загального підходу Поппера та Вільяма Бартлі до проблеми джастифікації, що зазвичай описується як «не-джастифікаціонізм», полягає у визнанні неможливості абсолютної, остаточної джастифікації (для будь-якої позиції) і замість пошуку такої джастифікації пропонує оцінювання відносних переваг і недоліків наявних альтернатив, що може надати нам підстави для пробного віддання переваги одній із них. Проведено порівняння між гіпотетико-дедуктивістською концепцією Поппера та концепцією абдукції, або висновування до найкращого пояснення, Чарльза Пірса. Показано, що ці концепції мають подібну логічну структуру, а також продемонстровано, що стосовно емпіричної науки вони доповнюють, уточнюють і прояснюють одна одну; зокрема, висновування до найкращого пояснення може слугувати обґрунтуванням припущення про існування законів природи, яке передбачене гіпотетико-дедуктивістською концепцією.

Ключові слова: індукція, джастифікація, гіпотетико-дедуктивний метод, фальсифікаціонізм, абдукція, Поппер, Бартлі, Пірс.

One of the most important achievements, if not *the* most important achievement, on which Karl Popper prided himself was his solution to the problem of induction, or Hume's problem. These two names, «the problem of induction» and «Hume's problem», are used as synonymous by Popper and other participants of the discussion. I suspect that this may be misleading for understanding Popper's solution or, rather, solutions. Really, Hume's problem seems to be the problem of the justification of *induction*, but there is more to it: it is the problem of the *justification* of induction as well as *of any possible alternative* with which induction may be replaced. Popper's explicitly claimed solution to the problem of *induction* is hypothetico-deductivism and falsificationism.¹ This leaves open the problem of *justification* — now of hypothetico-deductivism and falsificationism instead of induction. The solution to this problem can be found as implicit in Popper's expositions of his general conception of rationality designated as *critical rationalism*.² The explicit statement

¹ Popper K. The Logic of Scientific Discovery. New York: Basic Books, 1959; Popper K. Conjectures and Refutations: The Growth of Scientific Knowledge. Routledge, 1962; Popper K. Objective Knowledge: An Evolutionary Approach. Oxford University Press. 1972, 1979; Popper K. Realism and the Aim of Science. Routledge, 1985.

² Chapter 24, «Oracular Philosophy and The Revolt Against Reason», in Popper K. The Open Society and Its Enemies. Vol. 2. London: George Routledge & Sons, Ltd., 1945. P. 212-245.

and defence of this solution (presented as the solution to *the problem of the limits of rationality*) was advanced by Popper's pupil William Bartley.³ Bartley calls it *pancritical rationalism*, or *comprehensively critical rationalism*, to emphasize that it is not a matter of limiting the application of rationality (and leaving the most fundamental positions to irrational commitment) but a matter of the replacement of the traditional conception that identifies rationality with the requirement for justification with the weaker (non-justificationist, or minimalist) conception that identifies rationality with the openness to critical discussion in the search for truth. In this way, Bartley repairs some ambiguity in Popper's earlier expositions of *critical rationalism*, which can be construed as retaining the identification of rationality with the requirement for justification but making exceptions to this requirement, as unavoidable «concessions to irrationalism». Bartley's important point was that rationality as openness to critical discussion (unlike the requirement for justification) need not make any exceptions, and can be applied comprehensively. Popper's-Bartley's non-justificationism changes the focus from the question of whether a theory is justified to the question of whether there are reasons to prefer a theory over other, alternative theories.

This article purports to provide a detailed account and elucidation of Popper's solution to the problem of induction, Popper's-Bartley's solution to the problem of justification (or of the limits of rationality), as well as the relationship between these solutions, hypothetico-deductivism-*cum*-falsificationism and pancritical rationalism. In addition, it compares the hypothetico-deductive account with the Piercean abductive account, and reveals their similarity and complementarity.

1. What Popper held to be his solution to Hume's problem of induction

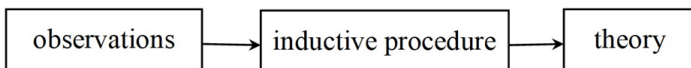
The specific problem of induction can be formulated as follows: given that we obtain our general theories by inductive generalization from experience, what inductive procedure, inductive method, or inductive logic ensures the truth or, at least, high probability of our theories? Popper's solution to this problem is:

³ Bartley W. *The Retreat to Commitment*. Alfred A. Knopf, 1962; Bartley W. *Rationality versus the Theory of Rationality* / Mario Bunge (Ed.), *The Critical Approach to Science and Philosophy*. New York: Free Press, 1964. P. 3-31.; Bartley W. *The Philosophy of Karl Popper. Part III. Rationality, Criticism, and Logic. Philosophia*. 1982. Vol. 11 (1-2). P. 121-221.

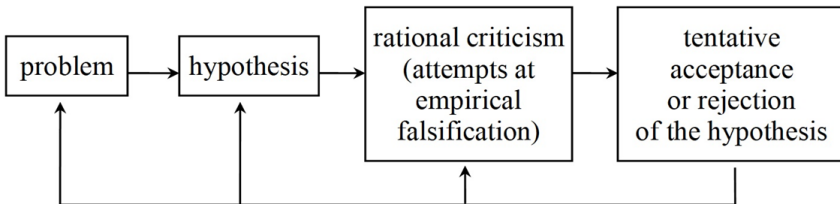
- (1) there is no inductive logic, no correct inductive procedure, no way to ensure the truth of our theories or even the high probability of them being true or generating true predictions;
- (2) the basic assumption of inductivism — the view that we obtain our general theories by inductive generalization from experience — is mistaken. In fact, we obtain our theories as conjectures made in attempts to solve our problems; after this, we should expose these hypotheses (tentative solutions to our problems) to critical discussion and empirical testing, which are attempts to *refute (falsify)* the theory at issue by demonstrating that its predictions contradict some results of properly made reproducible (usually, experimental) observations; hypotheses that withstand these ordeals are to be tentatively accepted as true. (The normative component, expressed by «should» and «are to be», defines favourable conditions for the development of our knowledge, the major factor of the progress in science.)

Some of Popper’s critics say that his hypothetico-deductive falsificati-
 onist theory is in fact a variety of induction. However, the two have very
 little in common, as we can see from the schemas below, which represent
 the structure of cognitive process according to the theory of induction and
 according to Popper’s theory.

Inductivist theory:



Popper’s theory:



To avoid a pretty usual misunderstanding, it should be noted that
 Popper’s rejection of induction does not mean the denial of the relevance
 of past experiences (observations) to the prediction of future events. It

would be absurd to deny such a relevance, and Popper didn't. What he denied is that this relevance is a matter of (inductive) *inference* from past observations to future events. He countered this with the claim that the relevance of past observations to prediction of future events is mediated by theories, which are, by their nature, hypotheses, conjectures that *are not inferable* from past observations. (Without such hypotheses we could have no idea as to *which* features of previously observed events are relevant for predictions of future events and *how* they are relevant). Consider, for example, Popper's explanation in the Appendix 2 of *Objective Knowledge*:

We never (least of all in science) draw inferences from mere observational experience to the prediction of future events. Rather, each such inference is based upon observational experience (formulated by statements of «initial conditions») *plus* some *universal theories*. The presence of these theories (such as Newton's theory of gravitation) is essential for arguing from the past to the future. But *these universal theories are not in their turn inferred* from past observational experience. They are, rather, *guesses*: they are *conjectures*.⁴

2. The residual problem of justification and Popper's-Bartley's solution

Now we turn to the more general problem — Hume's problem of the *justification*, either of induction, or of *whatever we put in the place of induction*. Hume contended that it is impossible to properly rationally justify induction; hence our reliance on it is irrational. Here, «proper rational justification» can be defined as a logically valid argument without unjustified premises. We obtain a proper rational justification if and only if the argument is logically valid and all its premises are justified. If (some of) the premises aren't justified, then the conclusion drawn from them is also unjustified. But such a proper rational justification is impossible, for obvious logical reasons! To justify a premise of an argument, you need another argument with other premises, and to justify those premises you need yet other arguments with yet other premises, and to justify those premises . . . , and so on *ad infinitum*. This chain has no end and so never achieves its required destination — justified premises that can serve as the foundation for justified conclusions. And this argument applies not only to induction but to any contender for its place in our general account of how we obtain and develop our theoretical knowledge!

⁴ Popper K. *Objective Knowledge: An Evolutionary Approach*. (Rev.ed.) Oxford University Press, 1979.

Arguably, the most congenial Popperian solution to this problem is «biting the bullet». Yes, it is impossible to justify, in the way specified above, anything whatever (including, surely, hypothetico-deductivism and falsificationism). But, in the one very important meaning of the word «rationality», this does not mean that it is impossible to be rational (or that it is inevitable to be irrational) with respect to all, or most, or some of our ideas. This meaning — which I will call critical or minimalist or non-justificationist conception of rationality — is that we admit that we may be mistaken in our views, and we keep our views open to critical discussion and are willing to renounce or revise them if some arguments to this end happen to be convincing for us.

The basics of this conception of rationality can be found in Popper's expositions of *critical rationalism* as opposed to *uncritical* or *comprehensive rationalism*, in Chapter 24 of *The Open Society and Its Enemies*. However, that exposition allows two very different interpretations as to what the principal difference between critical and uncritical/comprehensive rationalism is supposed to be.

Popper describes uncritical/comprehensive rationalism as «the attitude of the person who says “I am not prepared to accept anything that cannot be defended by means of argument or experience”»; «the principle that any assumption which cannot be supported either by argument or by experience is to be discarded».⁵

There are two main objections to this view.

First, the acceptance of rationalism itself cannot be a matter of its being justified by argument or experience: «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».⁶

⁵ Popper K. *The Open Society and Its Enemies*. Vol. 2. 1945. P. 217.

⁶ *Ibid.* P. 217-218. Arguably, this objection is not irresistible. There are at least two ways for a comprehensive rationalist to meet it. First, she can point out that to be *sometimes* affected by arguments, to *sometimes* take the rational attitude, with respect to *some* issues, one does not need to be a rationalist. Certainly, a person who is not a rationalist (either an irrationalist or someone who does not take sides in the rationalism/irrationalism debate) can (and in fact, surely will) sometimes be rational. Rationalism is not a matter of *sometimes* being rational but of *principal, conscious allegiance* to rationality. To say more, it is mistaken to think of rationality and allegiance to the rational attitude as an either-or matter (a person is either rational or entirely devoid of rationality) — the mistake aptly criticised by Ray Percival as «The Myth of the Closed Mind» (Percival R. *The Myth of the Closed Mind*. Open Court, 2011). Rather, rationality and allegiance to it is a matter of degrees, more-or-less.

Second, «the principle that any assumption which cannot be supported either by

The second (much graver) objection to uncritical/comprehensive rationalism is that its requirement for justification cannot be satisfied because it generates infinite regress (as explained in the first paragraph of this section).

As for his preferred alternative, critical rationalism (CR), Popper presented it so that two approaches are distinguishable:

(1) CR is the view that makes important «concessions to irrationalism»: unlike comprehensive rationalism, it admits limitations on rationality — that unavoidably, some positions should be accepted irrationally;

(2) CR is the view that, unlike non-critical rationalism, identifies rationality not with the requirement of justification (that a position, to be accepted, should be supported by argument or by experience) but with openness to critical discussion.

This duality (ambiguity?) is contained already in the designation of the more traditional conception (that Popper criticises) as comprehensive/uncritical rationalism. What is wrong with this view? Is it that it takes the requirement of rationality as comprehensive, applicable to all beliefs? Or is it that it ineptly identifies rationality with justification? If the former, then its alternative, short of irrationalism, is non-comprehensive rationalism, (1); if the latter, then the relevant alternative is non-justificationist rationalism, (2). However, Popper didn't draw the distinction.

On the one hand, Popper takes the first objection against uncritical/comprehensive rationalism, that «a rationalist attitude must be first adopted if any argument or experience is to be effective, and it cannot therefore be based upon argument or experience»,⁷ as entailing the following conclusion:

[W]hoever adopts the rationalist attitude does so because without reasoning he has adopted some decision, or belief, or habit, or behaviour, which therefore in its turn must be called irrational. Whatever it may be, we can describe it as an irrational faith in reason. Rationalism is therefore far from comprehensive or self-contained.⁸

argument or by experience is to be discarded» (Popper K. *The Open Society and Its Enemies*. Vol. 2. 1945. P. 217) does not require that if the assumption is to be accepted, it should be accepted *because* it is supported either by argument or by experience. In fact, it states no condition for initial acceptance — only the condition for discarding. An uncritical rationalist can say that even if rationalism is accepted *not because it was supported* by an argument, once accepted, it *can be supported* by an argument, and so the principle at issue *does not* require that rationalism be discarded.

⁷ Ibid. P. 217-218.

⁸ Ibid. P. 218.

Accordingly, he describes «critical rationalism» as a «minimum concession to irrationalism»⁹, «one which frankly admits its limitations, and its basis in an irrational decision, and in so far, a certain priority of irrationalism».¹⁰ This strongly suggests the identification of critical rationalism with (1).

On the other hand, Popper describes the rationalism he espouses as «an attitude of readiness to listen to critical arguments and to learn from experience»; «fundamentally an attitude of admitting that “I may be wrong and you may be right, and by an effort, we may get nearer to the truth”».¹¹ This suggests the identification of critical rationalism with (2).

Popper, in *The Open Society and Its Enemies*, combined (1) and (2). Bartley, in *The Retreat to Commitment*, made a sharp distinction between (1) and (2); he took over (2) but criticised its conjunction with (1). Bartley argued that rationality as openness to critical discussion does not engender those problems that beset non-critical (justificationist) rationalism; hence, a critical rationalist, on his/her own (rather than on the uncritical rationalist's) conception of rationality need not and should not make any concessions to irrationalism, let alone admitting its basis in an *irrational* decision, or *irrational* faith in reason, or «a certain priority of irrationalism». So (1) is not needed and sits badly with (2): (1) implies the old notion of rationality, that of non-critical (justificationist) rationalism, rather than the notion introduced by (2). So Bartley considered (1) as «superfluous remnant of justificationism, out of line with the main thrust and intent of his [Popper's. — *D.S.*] methodology, empty baggage carried over from the dominant tradition»¹².

Bartley argued that *if we identify rationality with the openness to critical discussion and readiness to revise our beliefs and attitudes in the light of this discussion*, then rationality can be comprehensive — it is not the case that something should necessarily be left outside its domain: «there are no limits to rationality in the sense that one *must* postulate dogmas or presuppositions that must be held exempt from review»¹³; «it is not necessary to mark off a special class of statements, the justifiers, which *do* the justifying and criticizing but are not open to criticism».¹⁴

⁹ Ibid. P. 219.

¹⁰ Ibid. P. 218.

¹¹ Ibid. P. 213.

¹² Bartley W. *Unfathomed Knowledge, Unmeasured Wealth*. Open Court, 1990. P. 237.

¹³ Bartley W. *The Retreat to Commitment*. (2nd ed., rev. and enl.) Open Court, 1984. P. 221.

¹⁴ Ibid. P. 223.

Accordingly, the name he proposed for (2) is «pancritical rationalism», or «comprehensively critical rationalism».

Popper retained the name «critical rationalism» (without «pan» or «comprehensively») but assimilated main points of Bartley's conception; in particular, he stated his approval to «the principle that *nothing is exempt from criticism*», «the principle that *everything is open to criticism* (from which this principle itself is not exempt)»¹⁵, and characterised his conception of rationality as non-justificationist.¹⁶

There is a pretty common (and detrimental) misunderstanding about Popper's-Bartley's non-justificationism that should be warned against. David Miller, who was a friend and one of the most reputed followers of Popper, wrote an influential book *Critical Rationalism: A Restatement and Defence*, in which he construed — or rather advanced his own version of — critical rationalism as the view that rationality has nothing to do with reasons and needs nothing but deductions of false consequences.¹⁷ Quite a few Popperians took Miller's view as a genuine representation (and clarification) of Popper's-Bartley's views. However, I think that it is a mistake: arguably, Miller's «critical rationalism» is in conflict with Popper's views and is not entailed by Bartley's case for pancritical rationalism (as the solution to the problem of the limits of rationality).¹⁸ And arguably, Miller's «critical rationalism» can't work as a general account of rationality (applicable to all reasoning, whether inside or outside empirical sciences) and leads to absurd and clearly false consequences.¹⁹

The point of Popper's critical rationalism is that rationality is a matter of our being open to rational discussion and ready to revise our theories, beliefs, attitudes etc. in the light of such discussion, and the point of Bartley's pancritical rationalism is that such openness/readiness (and, hence, rationality) does not need any principal exceptions, and so can be comprehensive. And a rational discussion is, as Popper explained, one

¹⁵ Popper K. *The Open Society and Its Enemies*. (4th rev. ed.) Vol. 2. Routledge & Kegan Paul, 1962. P. 408-409.

¹⁶ See especially Popper K. *Realism and the Aim of Science*.

¹⁷ Miller D. *Critical Rationalism: A Restatement and Defence*. Open Court, 1994.

¹⁸ See Sepetyi D. Non-justificationism and the negativist legend about Karl Popper's philosophy. *Actual Problems of Mind*. 2020. Vol. 21. P. 24-45 for an analysis of Popper's and Bartley's texts to this point.

¹⁹ See Berkson W. In Defence of Good Reasons. *Philosophy of the Social Sciences*. 1990. Vol. 20 (1). P. 84-91; Berkson W. Methodology Is Pragmatic. *Philosophy of the Social Sciences*. 1990. Vol. 20 (1). P. 95-98; Sepetyi D. Non-justificationism and the negativist legend about Karl Popper's philosophy. *Actual Problems of Mind*. 2020. Vol. 21. P. 24-45.

that satisfies three basic principles:

1. The principle of fallibility: perhaps I am wrong and perhaps you are right. But we could easily both be wrong.
2. The principle of rational discussion: we want to try, as impersonally as possible, to weight up our reasons for and against a theory; a theory that is definite and criticizable.
3. The principle of approximation to the truth: we can nearly always come closer to the truth in a discussion which avoids personal attacks. It can help us to achieve a better understanding; even in those cases where we do not reach an agreement.²⁰

Popper's formulation of «the principle of rational discussion» is unambiguous about rationality being a matter of dealing with reasons (both *pro* and *contra*), and trying to estimate their relative «weightiness».

3. Back to «justification» of the preference for Popper's hypothetico-deductive account of science over the inductivist account

This view of rationality and «justification» dispenses with the question of whether a theory (or a statement, or a position) is justified in an «absolute» sense, and replaces it with the question of *relative* justification — the reasons for preferring a theory (a statement, a position) over other, alternative ones.²¹ In our case, the relevant question is: why is Popper's hypothetico-deductive account of science preferable to the inductivist account and other alternatives (if there are any)? What reasons «justify» this preference?

One major deficiency of the inductive account is concerned with inductive inferences, which are taken to be necessary for acquiring any useful knowledge about the world beyond a mere collection of records of particular observations. Hume's problem of induction is that such inferences are invalid, and not only in cases where the conclusion is «deterministic» (such as «All swans are white» or «The Sun will rise tomorrow morning») but also if conclusions are probabilistic (as with

²⁰ Popper K. In *Search of a Better World*. Routledge, 1992. P. 199.

²¹ Here, again, Popper's views about reasons and «justification» were (*pace* Miller) that «we can often give reasons for regarding one theory as preferable to another» by «pointing out that, and how, one theory has hitherto withstood criticism better than another», and «giving reasons for one's preference can of course be called a justification (in ordinary language)» (Popper K. *Realism and the Aim of Science*. P. 19-20).

inductive conclusions like «More than $x\%$ of swans are white» or «It is more probable that the Sun will rise tomorrow morning than not»). In cases of open (potentially infinite) multitudes, from the fact that all As *so far observed* (no matter how many) had a property B it *does not follow* that all As have B, or that a certain percentage of As have B, or that there is such-and-such probability that the next A that we will observe has B.

However, some inductivists²² object to this argument by pointing out that it appeals to *deductive validity*: by the rules of deductive logic, the (universal or probabilistic) conclusion does not follow from the conjunction of the (singular) premises. However, that only shows that induction is not (reducible to) deduction. But on the inductivist view, it should not be; rather it is as fundamental as deduction. Induction is to be taken as complementary to deduction, and many inductivists hold that there is some such thing as inductive logic that justifies inductive inferences. So perhaps inductive inferences are justified, although deductively invalid. Now suppose an opponent appeals to Hume's arguments to the point that we cannot justify the belief that inductive inferences are likely to result in true conclusions without reliance on induction; so any purported justification of induction will be viciously circular. To this, an inductivist can object that although such a justification will be circular, this circularity is not vicious, because anyway, even deductive logic cannot do better than justify its rules by its own means. If we try to justify the rules of deductive logic by advancing some argument in their favour, that argument inevitably relies on the rules of deductive logic. If this circularity does not discredit deductive logic, then the impossibility of non-circular justification of inductive logic is not discrediting for the latter as well.

Against this, an opponent of inductivism can argue that the supposed parallelism between (justification of) inductive and deductive logic is spurious. To begin with, despite centuries of efforts of inductivists, there is no clear and agreed upon account of what the rules of inductive logic are supposed to be. Compare: the main rules of deductive logic were formulated in the 4th century BC by Aristotle and are universally agreed upon. This

²² See, for example: Black M. The justification of induction // Black M. *Language and philosophy*: Studies in method. Cornell University Press, 1949. P. 59-88; Black M. *Models and Metaphors*. Cornell University Press, 1962; Jones G., Perry C. Popper, Induction and Falsification. *Erkenntnis*. 1982. Vol. 18. P. 97-104; Sanford D.H. The Inductive Support of Inductive Rules: Themes from Max Black. *Dialectica*. 1990. Vol. 44. P. 23-41; Papineau D. Reliabilism, Induction and Scepticism. *The Philosophical Quarterly*. 1992. Vol. 42 (166). P. 1-20; Lange M. Hume and the Problem of induction / D. Gabbay, S. Hartmann and J. Woods (eds.). *Inductive Logic*. Amsterdam: Elsevier, 2011. P. 43-92.

is not a merely accidental historical fact. Rather, it is a reflection of the fact that deductive inferences (patterns of which are expressed by the rules of deductive logic) are *self-obviously* valid. If you think of the matter *and if you have some basic logical grasp*, you will just «see», for example, that if all men are mortal, and if Socrates is a man, then Socrates is mortal; obviously it cannot be otherwise. And, if you consider things on a more abstract level, you will «see» that the same goes for all other similar inferences, of the same form (All As are B. X is A. Hence, X is B).²³ That is exactly why deductive logic does not require justification. On the other hand, this is clearly not the case with purported inductive inferences. It is clear that if we observed many swans and they all happened to be white, *it is possible* that the next swan will be of some other colour (and this is the same as to say that from the fact that all swans so far observed happened to be white it *does not follow* that the next one will be white too). Also, if you think of it a bit, it is clear that if we observed many swans and they all, or a certain percentage of them, happened to be white, it is *possible* that with other swans (those we haven't observed so far) some other percentage is white, and that percentage can be any — from 0 to 100. That is why deductive inferences are valid, and inductive inferences are not. Their validity/invalidity is not a matter of arbitrary postulation of some set of rules, but a matter of fact. Deductive inferences are in fact valid, and inductive inferences are in fact invalid.²⁴

²³ See an illuminating discussion by John Searle of the validity of a *modus ponens* argument *vis-à-vis* the Lewis Carroll paradox (Carroll L. What Achilles Said to the Tortoise. *Mind*. 1895. Vol. 4. P. 278-280). The conclusion is as follows:

The derivation does not get its validity from the rule of modus ponens; rather, the inference is perfectly valid as it stands without any outside help. It would be more accurate to say that the rule of modus ponens gets its validity from the fact that it expresses a pattern of an infinite number of inferences that are independently valid. The actual argument does not get its validity from any external source: if it is valid, it can be valid only because the premises entail the conclusion. Because the meanings of the words themselves are sufficient to guarantee the validity of the inference, we can formalize a pattern that describes an infinite number of such inferences. But the inference does not derive its validity from the pattern. [...] What goes for this argument goes for any valid deductive argument. Logical validity does not derive from the rules of logic. (Searle J. *Rationality in Action*. The MIT Press, 2001. P. 19)

²⁴ Popper's own reply to the question «Why should we take deductive inferences to be valid?» and so principally different from inductive inferences appeals to the non-existence of counter-examples: «[W]e have a method of objective critical testing at our disposal: to any proposed rule of deduction, we can try to construct a counterexample.

If so, we would certainly do better if we can do only with one logic, which represents well known and agreed upon valid rules, (that is, deductive logic) rather than seek to complement it with another set of rules (purported to constitute «inductive logic») with respect to which there is nothing nearing universal agreement and which are invalid. And Popper's hypothetico-deductive account of science is an account that shows how we can do it, and how scientists in fact do it. That alone is enough to make it very much preferable over the inductivist account.

There is also another very good reason for such a preference. It is that the inductivist account of the right method to acquire knowledge, *as one that begins with unprejudiced collection of observations and proceeds to their classification and generalization*, is crudely wrong both as the description of the best scientific practice and as a recommendation for the efficient way to develop knowledge. In fact, there are very few scientific achievements that even approximately fit this model, and arguably if all scientists would seriously try to follow it, they would achieve very little. Let me follow James Ladyman (2002) for some prominent historical examples.

Let us begin with Newton's system of physics. Although Newton himself «famously claimed not to make hypotheses, but to have inductively inferred his laws from the phenomena»²⁵, this does not seem really to be the case. There is no evidence that he did what Bacon's account of inductive method prescribes: begin with collecting observations made without prejudice or preconception under a wide variety of conditions, record the results, put the data in tables of Essence and Presence (a list of all things having a certain feature), of Deviation and Absence by Proximity (things that are as close as possible to those in the first table but not having that feature), of Degrees and Comparisons (in which things with the feature are quantified and ranked according to the amount of the feature they involve), and then make an inductive generalisation by «studying all the information displayed in the tables and finding something that is present in all instances of the phenomenon in question, and absent when the phenomenon is absent, and furthermore, which increases and decreases in amount in proportion with the increases and decrease of

If we succeed, then the inference, or the rule of inference, is invalid. . . » (Popper K. *Unended Quest: An Intellectual Autobiography*. Routledge, 2002. P. 169). However, this testing itself involves a sort of logical or modal «seeing», for example, that if all men are mortal and Socrates is a man, then Socrates cannot be immortal. No empirical check with the real Socrates is required or relevant, for if it turns out that Socrates is immortal, this clearly means (as a matter of logical-modal «seeing») that either Socrates is not a man, or not all men are mortal, and the inference at issue is valid anyway.

²⁵ Ladyman J. *Understanding Philosophy of Science*. Routledge, 2002. P. 54.

the phenomenon».²⁶ Instead, «[i]n his celebrated *Principia* [...], Newton presented his three laws of motion and his law of universal gravitation, and went on to use them to explain Kepler's laws of planetary motion, the behaviour of the tides, the paths of projectiles (such as a cannon ball) fired from the surface of the Earth, and many other phenomena».²⁷ This presentation moves not from observations or less fundamental laws to Newton's laws but in the opposite direction: rather than Newton's laws being «induced» from «Kepler's laws of planetary motion, the behaviour of the tides, the paths of projectiles, etc.», the latter are explained by being deduced (approximately) from Newton's laws. (Note that this is how it should be according to Popper's hypothetico-deductive account.) Moreover, as Pierre Duhem pointed out, Newton's laws could not be «induced» from Kepler's laws, because the former *contradict* the latter: Kepler's laws «say that the planets move in perfect ellipses around the Sun», whereas Newton's laws entail that the paths of the planets will never be perfect ellipses (because each planet exerts a gravitational force on all the others and the Sun itself).²⁸ Besides, Newton's law of gravitation involves new quantitative theoretical concepts (introduced by Newton), of mass and force, that are entirely absent in Kepler's laws (which «relate positions, distances, areas, time intervals and velocities and make no mention of forces and masses».²⁹ The idea that Newton's laws could be induced from observations is also untenable. For example, Newton's first law «states that every body will, unless acted upon by an external force, maintain its state of uniform motion (if it is already moving) or will remain at rest (if it is not)»; in fact, however, «we have never been able to observe a body that is not acted upon by some external force or other, so again this law cannot have been inferred directly from the observational data».³⁰ And the later refutation of Newtonian mechanics (its failure to withstand testing in situations where velocities near the speed of light) and its replacement with Einstein's theory is another proof that Newtonian mechanics was not a result of inductive inference. (The observational basis from which Newtonian mechanics was claimed to be induced is equally consistent with Einstein's theory.)

The situation with Kepler's discovery of his laws of planetary motions may look more in line with the inductivist account. In fact, Kepler had at

²⁶ Ibid. P. 24.

²⁷ Ibid. P. 54-55.

²⁸ Ibid. P. 55.

²⁹ Ibid. P. 55-56.

³⁰ Ibid. P. 55.

his disposal thousands of observations of the planets made earlier by the astronomer Tycho Brahe. However, as Ladymen points out, «Kepler was unable just to read off his laws from the data, rather he was motivated to search for a reasonably simple pattern to planetary motion by his somewhat mystical (Pythagorean) belief in a mathematically elegant form to the motion of the planets, which he thought of as the harmony of the spheres».³¹

For later developments in physics, the application of the inductivist account becomes ever more implausible. One reason is that Bacon's recommendation that scientific research should start from scratch, from observations free of all preconceptions, is impracticable and misleading. Many 20th century philosophers highlighted the point that observations are always «theory laden»; what we can observe depends on our concepts and background knowledge. This should be obvious at least for modern science that uses complex devices to make observations, devices that embody preceding scientific developments:

It may have seemed okay to start from scratch in Bacon's time in order to avoid being misled by the received Aristotelian wisdom that had become dogmatic and unproductive, but nineteenth and twentieth century scientists were building upon well-established and complex theories. They wanted to consolidate and extend that success and not ignore it when investigating new domains. So they needed to use the theories of optics to help build telescopes to study stars and microscopes to study cells. Modern science is so complex and developed it is absurd to suggest that a practising scientist has no preconceptions when undertaking research. Scientists need specialised knowledge to calibrate instruments and design experiments. We cannot just begin with the data, we need guidance as to what data are relevant and what to observe, as well as what known causal factors to take into account and what can safely be ignored.³²

[O]nce a science has matured, the idea that further observations should be presuppositionless is undesirable, because it means starting from scratch instead of building on previous success.³³

It should also be noted that the theories of modern «matured» science involve many concepts of entities that are not directly observable, and so these theories cannot be inferred as a matter of generalisations of the results of observations. Take, for example, such physical entities as electrons, photons, quarks, electromagnetic fields, etc. The justification of modern physical theories that involve such unobservable entities is that

³¹ Ibid. P. 56.

³² Ibid. P. 57.

³³ Ibid. P. 110.

these theories are fruitful of predictions about observable phenomena, and these predictions fit the results of observations (usually, in experimental situations), that is, pass the observational (experimental) tests that were likely to refute (falsify) them, if the theories at issue are false.

Generally, as we have seen from the examples of Newton's and Kepler's theories, scientific theories are not generalisations that can be simply «read off» or induced from the data:

If there is one thing that has been learned from the twentieth century debates about scientific method it is that the generation of scientific theories is not, in general, a mechanical procedure, but a creative activity.³⁴

This is just what Popper's hypothetico-deductive account tells: the development of science is not a matter of first collecting observations and then inducing theories from the collected data; it is a matter of problem-solving by inventing hypotheses and subjecting them to severe empirical testing.

4. Hypothetico-deductive method *vis-à-vis* inference to the best explanation

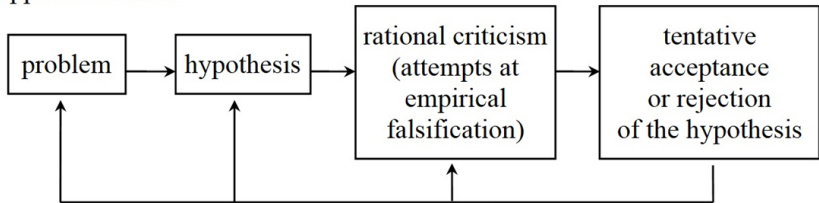
It is appropriate here also to compare Popper's hypothetico-deductivism-*cum*-falsificationism with another account of science that gained popularity in 20th century — the account based on abduction, or inference to the best explanation, henceforth to be referred to as the abductive account. The founder of this account of science was the prominent American philosopher of the 19th–20th centuries Charles Sanders Peirce, and the basic method, abduction or inference to the best explanation, is the same that was described by Arthur Conan Doyle in his stories about Sherlock Holmes (mistakenly designated there as «deductive method»). The point of the abductive account is that scientists look for the best explanation of the data that they seek to explain. Roughly, the theory is accepted as true if it is found to give the best explanation (as compared with alternative theories available at present) to some phenomena that require explanation.

The logical structure of the abductive account is similar to that of the hypothetico-deductive account. *Prima facie*, it may seem that the abductive account is like the inductive account in taking data of observations as the starting point of research. However, on the abductive account, these observational data are not things to be collected, classified

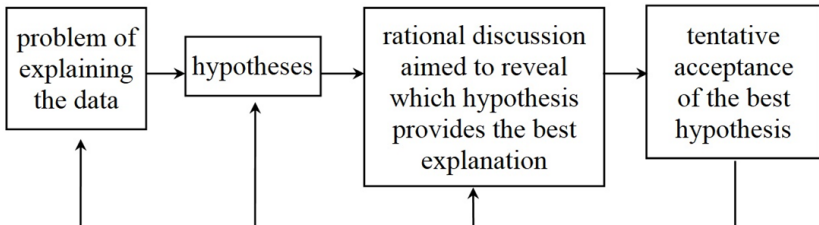
³⁴ Ibid. P. 74.

and inductively generalized upon; rather they enter into the research only insofar as scientists find that they require explanation. That is, the starting point is *the problem of explaining* some data, observed facts. Scientists invent potential explanations, that is, advance explanatory hypotheses, and then try to adjudicate which of these hypotheses is the best explanation of the explanandum. The decision should be taken as tentative rather than final, because in future, someone can devise some new, even better explanation.

Popper's account:



The abductive account:



Comparing these accounts, let us note the following. The abductive account well reflects the point that theories are advanced primarily for the purpose of explaining something. Therefore, their success in providing such explanation is the primary requirement. If a theory fails in this, it fails full stop. However, there are several problems with the abductive account.

First, it does not explain how we are to adjudicate between the candidates: what are the criteria for determining that an explanation is better than other explanations?

Second, if at present there is only one candidate, and it turns out to be pretty bad at explaining what it was required to explain, it is still the best one. Should it be accepted for true?

Third, a theory that is very successful in explaining the data that was its initial target can conflict with other data or with other theories we

accept. (Newton's theory was hugely successful, and in a sense it is still the best explanation for the data within its realm of success — if only because it is much simpler for understanding and use than Einstein's theory.)

Fourth, a theory that is successful in explaining the data that was its initial target and is consistent with other known data and other accepted theories can conflict with facts about the world that are not as yet known (were not so far observed). The abductive account does not enjoin to look for such facts (devise and execute experiments that can perhaps refute the theory).

In all these respects, hypothetico-deductive account provides clarifications and improvements.

First, it provides a deductive account of what counts as successful explanation: a theory supplemented with initial conditions explains a phenomenon if the theory and the initial conditions logically entail the occurrence of the phenomenon, or a high probability of its occurrence.

Second, it discourages the acceptance of a theory that fails to properly explain what it was required to explain, even if at present we have no better alternative candidate.

Third, it requires that a theory, to be accepted as true, should be not only successful in explaining the data that was its initial target but be also consistent with other known data and other accepted theories.

Fourth, it enjoins scientists not to be content with that but to subject the theory to further severe testing that can conceivably refute it.

On the other hand, in certain respects, the hypothetico-deductive account can be seen as supported by abduction, and in certain other respects it can be improved by integrating some basic insights of the abductive account. The preference for the hypothetico-deductive account can be regarded as a matter of its providing the best explanation for the development of science and its most successful practices.

Abduction can also provide the answer to the question: why it is reasonable to (tentatively) accept a theory that survives many various attempts at falsification, if the fact that it survives so does not logically entail that it is true, nor even that its truth is highly probable (in the sense of the probability calculus). The answer may be that the theory's survival is best explained by the supposition that the theory is either true or pretty near the truth (at least, in the domain where it was well tested). Otherwise, the fact that the theory survived many attempted various trials although it was very likely to be falsified by each of them would be a hugely improbable coincidence, sort of a miracle.

Also, on the hypothetico-deductive account, the research in empirical

sciences implies the assumption of the existence of laws of nature — universal, time-space invariant properties of the world that are not directly observable. If we ask about credentials («justification») for this assumption, the most plausible answer is that this assumption is necessary for explaining the regularities we find in the world and for justifying our (practically necessary) expectations that these regularities are not merely matter of accidental coincidences in the data we happen to have from our observations to-date but are expectable to hold in future (for the observations we haven't yet happened to make).³⁵ The alternative to the assumption that there are laws of nature that explain the most important regularities or patterns found in the available data is that these regularities are merely a matter of superhuge coincidences. Such an explanation does not count as satisfactory on the deductive account of satisfactory explanation — because nothing is entailed by the hypothesis that there are no laws of nature and everything that happens is entirely a matter of coincidences. And it means that for any such regularity discoverable in the available data, there are no reasons at all to expect that it will hold for future events (or future observations). Hence, no predictions are reasonable (or more reasonable than any other). In particular, predictions made in conformity with what is considered as the best scientific theories are not a bit more reasonable or more reliable than any other, entirely arbitrary, predictions.

Also, by focusing on the *relative* advantages of theories to be evaluated (as candidates for acceptance for truth), the abductive account invites reasonable correction for a too strong literal interpretation of falsificationism. The point is that if a theory is by-and-large successful at explaining a large scope of observable phenomena (and in withstanding experimental testing), but there seem to be phenomena that contradict its predictions, and there is no better alternative, then *it is not advisable for scientists to take this conflict as a falsification* of the theory. As Pierre Duhem³⁶ famously argued, we never know for certain whether the failed prediction is due to the falsity of the theory or to some mistaken complementary assumptions (the specification of «initial conditions»), and sometimes it is reasonable to think that the latter rather than the former are to blame. At the same time, such a conflict should not be ignored but taken as a problem

³⁵ Cf.: «And our guesses are guided by the unscientific, the metaphysical (though biologically explicable) faith in laws, in regularities which we can uncover — discover.» (Popper K. *The Logic of Scientific Discovery*. P. 278.)

³⁶ Duhem P. *The Aim and Structure of Physical Theory*. Princeton University Press, 1954.

for the theory, its *possible* falsification, and scientists should make efforts to resolve the problem, in one way or the other — either by demonstrating that the apparent falsification is not a genuine one, or by finding a better theory and discarding the old one as falsified.

To make this point clear, consider the famous case with Newton's mechanics and the orbit of Uranus. The actually observed orbit of Uranus turned out to be somewhat different from that predicted by Newton's mechanics, given all known facts about masses, positions and motions of the heavenly bodies. However, this was not taken as the falsification of Newton's mechanics. Instead, scientists advanced the auxiliary hypothesis that there is another planet, so far unknown, that is responsible for the divergences. Popper's response to this case in *The Logic of Scientific Discovery* was that scientists should avoid introducing auxiliary hypotheses in order to «immunize» their theories, *except if such hypotheses are empirically testable*.³⁷ Now, in the case of Newton's theory and Uranus, the auxiliary hypothesis about another planet was empirically testable, was tested, and successfully passed the test. However, this should not always be the case. It is clear that scientists should not advance *ad hoc* hypotheses that are untestable *in principle* (never can be made testable), for example, that invisible and otherwise unobservable angels push the planet. However, scientists may be justified in holding that a theory like Newton's mechanics in such a situation is true even if they don't have a testable auxiliary hypothesis at present. It may be that they have an auxiliary hypothesis that, although testable in principle, *cannot be tested with available technologies* (so that its testing must await the appearance of new technologies or specific circumstances that will enable the testing). And it may be that for some spell of time, scientists *don't have any particular auxiliary hypothesis* but just suppose that something must be wrong with the specification of the relevant initial conditions. In both such cases, it may be advisable for scientists to take the apparent conflict between the theory and some observations not as a falsification of the theory but as a problem that requires further research.

Popper's later reply to criticisms of his falsificationism includes the following important admissions:

Often it takes a long time before a falsification is accepted. It is usually not accepted until the falsified theory is replaced by a proposal for a new and better theory.³⁸

³⁷ Popper K. *The Logic of Scientific Discovery*. P. 82-83.

³⁸ Popper K. *Realism and the Aim of Science*. P. xxiv.

I do not contend that falsifications are usually accepted at once... — not even that they are immediately recognised as *potential* falsifications.³⁹

I think that that these admissions are not quite sufficient, insofar as they are merely factual: yes, scientists usually do that; so what? We have the fact that scientists usually are not hasty to recognise *prima facie* conflicts between a generally successful theory and some observations as falsifications, and usually wait for the appearance of a new and better theory. Is that commendable? Is that generally beneficent or maleficent for science? I think that examples like those of Newton's mechanics and the orbit of Uranus strongly suggest that it is beneficent and so commendable. Obviously, much bad and nothing good would come of scientists' discarding generally successful theories without replacing them with better ones whenever there is an appearance of refutation. If so, the corresponding correction to the methodological rules proposed in *The Logic of Scientific Discovery* is appropriate.⁴⁰

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³⁹ Popper K. Realism and the Aim of Science. P. xxviii.

⁴⁰ Note that this is in line with what Popper said about «the need for some dogmatism» in science: «the dogmatic scientist has an important role to play. If we give in to criticism too easily, we shall never find out where the real power of our theories lies» (Popper K. Normal Science and its Dangers / I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge*. Cambridge University Press, 1970. P. 55).

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