POPPER AND HYPOTHETICO-DEDUCTIVISM

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Popper famously declared that induction is a myth. This thesis, if true, makes nonsense of the current volume. But is the thesis true? And, before we get to that, what precisely does it mean?

Popper is a deductivist. He thinks that whenever we reason, we reason deductively or are best reconstructed as reasoning deductively. Most philosophers disagree. Most philosophers think that most reasoning is non-deductive.

To understand why most philosophers think this, we have to look at the functions of reason or argument, and see that deduction seems quite unsuited to serve some of those functions. What are the functions of argument? Why do people reason or argue? One function of reason or argument is to form new beliefs or come up with new hypotheses. Another is to prove or justify or give reasons for the beliefs or hypotheses that we have formed. A third is to explore the consequences of our beliefs or hypotheses in order to try to criticise them. We need a logic of discovery, a logic of justification, and a logic of criticism.

It is usually accepted that deductive logic is fine so far as the logic of criticism goes. "Exploring the consequences of our hypotheses" means exploring the *deductive* consequences of our hypotheses. Criticism proceeds by deducing some conclusion, showing that it is not true (because it does not square with experience, experiment, or something else that we believe), and arguing that some premise must therefore be false as well. Criticism only works if the reasoning is deductively valid, if the conclusion is 'contained in' the premises, if the reasoning is not 'ampliative'. That is what entitles us to say that if the conclusion is false some premise must be false as well. If our argument were ampliative, criticism would not work. Showing that the conclusion is false would not entitle us to say that some premise must be false as well.

But deduction's strength so far as criticism is concerned seems to be a weakness as far as discovery and justification are concerned. In a valid deduction the conclusion is contained in the premises, does not 'amplify' them, says nothing new. If we want to come up with new beliefs or hypotheses, deduction obviously cannot help us. And if we want to justify a belief, deduction cannot help us once more. Deducing the belief we want to justify from another stronger belief is bound to be question-begging. The logics of discovery and justification must be non-deductive or ampliative. The conclusions of the arguments involved cannot be contained in the premises, but must 'amplify' them and say something new. Or so said the critics of deductive logic, down the ages.

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ENTHYMEMES AND THEIR DEDUCTIVIST RECONSTRUCTIONS

Deductivists deem ampliative reasoning invalid. If most reasoning is ampliative, then deductivists deem most reasoning invalid. That unpleasant consequence may seem reason enough to reject deductivism. To be sure, logic has a critical function. The task of the logician is not just to describe or 'model' how people do in fact reason, but also to prescribe how people *ought* to reason if they are to reason *well*. But if most reasoning is ampliative, then deductivists seem committed to the view that most of the time we do not reason well. Deductivism is a utopian ethic according to which most ordinary logical behaviour is thoroughly immoral!

Deductivists have a way of avoiding this unpleasant consequence. Folk seldom spell out all the premises of their arguments. Most reasoning, including most everyday reasoning, is in *enthymemes,* arguments with unstated or 'missing' premises. An argument which is invalid as stated can often be validated by spelling out its missing premise.

But deductivists must be careful here. Any invalid argument from premise(s) P to conclusion C can be validated if we count it an *enthymeme* and add the missing premise "If P then C". If deductivists are not careful, they will end up saying that people never argue invalidly at all! Deductive logic will be deprived of any critical function! Deductivists will now have an ethical gospel of relaxation — "Reason how you will, I will validate it".

There is an obvious deductivist response to this. Not every argument should be counted as having a missing premise. It must be clear from the context of production of an argument that it has a missing premise and what that is. Suppose somebody argues that it must be raining, because when it rains the streets get wet and the streets are wet. Deductivists do not validate this argument. They say it is a fallacy, an example of the fallacy of affirming the consequent. As to the uncertainty about what the missing premise of an argument is, if there is one, logic teachers have for generations asked students to supply the missing premises of arguments presented to them — and have marked their answers right or wrong. Still, this is not a logical issue, but a pragmatic one (for want of a better word). It must be admitted that in some cases it may not be clear from the context whether there is a missing premise and what it is. But then, if it matters, we can try to find out.

Most philosophers do not like deductivist reconstructions. Most philosophers say that ampliative reasoning is not to be validated in this way. Ampliative reasoning is deductively invalid, to be sure, but it is perfectly good reasoning nevertheless. Or at least, some of it is. All of it is invalid, but some of it is *cogent* and some of it is not. Thus the inductive logicians, or at least the best of them, try to work out when an ampliative argument is cogent and when not. Here they face the same problem as the deductivists. If it is not clear what the missing premise is, that would convert a real-life argument into a valid deduction, then it is equally unclear what the ampliative rule or principle is, that the real-life arguer is supposed to be employing. Unclarity about *enthymemes* cannot comfort inductivists.

Furthermore, inductivists do not think that all invalid arguments are ampliative. They agree with deductivists that you commit a fallacy if you argue that it must be raining because when it rains the streets get wet and the streets are wet. They do not say that this argument, although it is not deductively valid, is a perfectly cogent argument in some fancy ampliative logic. Yet when it comes to other deductively invalid arguments, this is precisely what inductivists do say. This is puzzling.

'AUTOMOBILE LOGIC'

To see how puzzling it is, suppose somebody produces the following argument:

> American cars are better than Japanese cars.
> Therefore, Cadillacs are better than Japanese cars.

What to say of this argument? You might quarrel with the premise, or with the conclusion, or with both. But what to say if you are a logician? Obviously, if you are a deductive logician, you will say that the argument is invalid, that the conclusion does not follow from the premise.

That verdict may seem a bit harsh. You might soften it by suspecting an *enthymeme*. Perhaps our arguer also has an unstated or missing premise to the effect that Cadillacs are American cars. If we spell that premise out, we get the perfectly valid argument:

> [Cadillacs are American cars.]
> American cars are better than Japanese cars.
> Therefore, Cadillacs are better than Japanese cars.

Having reconstructed the argument this way, we can turn to the more interesting question of whether the premises and conclusion of the argument are true. This is all trivial, and familiar to countless generations of logic students.

[By the way, in my deductivist reconstructions of *enthymemes*, I place the missing premise in square brackets. As we will see, these missing premises are often general claims. But it is not to be assumed, as it often is, that missing premises are always general. Ordinary reasoners often suppress particular premises as well as general ones. A real-life arguer might well say "We are all mortal — so one day George Bush will die" as well as "George Bush is only human — so one day he will die". In the former case the suppressed premise ("George Bush is human") is particular rather than general.]

There is another way to soften the harsh verdict that the argument with which we started is invalid. You might say that though the argument is deductively invalid, it is a perfectly cogent argument in a special non-deductive or inductive or ampliative logic that deals with arguments about automobiles. This *automobile logic* is characterised by special rules of inference such as "Cadillacs are American cars". (Do not object that "Cadillacs are American cars" is not a rule of inference,

but a sociological hypothesis about automobile manufacture. Recast it as a rule of inference: "From a premise of the form 'x is a Cadillac', infer a conclusion of the form 'x is an American car"'.) Unlike the formal or topic-neutral rules of deductive logic, the rules of automobile logic are material or topic-specific. The formally valid arguments of deductive logic are boring, for their conclusions can contain nothing new. But the 'materially valid' or cogent arguments of automobile logic are exciting, because their conclusions do contain something new.

So, the deductivist ploy regarding an invalid argument she wishes to appropriate is to reconstruct it as an *enthymeme* and supply its missing premise. And the inductivist ploy regarding a valid argument he wishes to appropriate is to reconstruct it (perhaps 'deconstruct it' would be better) as an ampliative argument where some necessary premise becomes a material rule of inference. Both ploys risk being applied trivially, as the example of automobile logic makes clear.

Enough! Automobile logic is silly. Has any serious thinker ever gone in for anything like automobile logic? Yes, they have, and on a massive scale. But before I document that claim, a complication needs to be considered.

FORMAL AND SEMANTIC VALIDITY

Suppose somebody produces the following argument:

> Herbert is a bachelor.
> Therefore, he is unhappy.

This argument is invalid. As before, deductivists will treat it as an *enthymeme* with a missing premise to the effect that bachelors are unhappy, to obtain:

> [Bachelors are unhappy.]
> Herbert is a bachelor.
> Therefore, he is unhappy.

This argument is valid, and has a dubious hypothesis about the virtues of matrimony as its missing premise. As with automobile logic, we would think ill of a philosopher who said that the original argument, though formally invalid, is a materially valid or cogent argument in matrimonial logic, which has "Bachelors are unhappy" as one of its interesting inference-licenses.

But now suppose somebody produces the argument:

> Herbert is a bachelor.
> Therefore, he is unmarried.

As before, deductivists might insist upon treating this as an *enthymeme* and supplying its missing premise to obtain:

> [Bachelors are unmarried.]
> Herbert is a bachelor.
> Therefore, Herbert is unmarried.

But it may be objected that the original argument is already valid: it is impossible for its premise to be true and its conclusion false. This is because its missing premise is analytically or necessarily true, true by virtue of the meaning of the word 'bachelor'. The argument is not formally valid, to be sure, but it is semantically valid. Deductivists, it may be objected, simply overlook the fact that there are semantically valid arguments as well as formally valid ones.

This objection raises the vexed question of whether the analytic/synthetic distinction is viable. Are there, as well as logical truths that are true in virtue of their logical form, also analytic truths that are true in virtue of the meanings of their non-logical terms? Quine said NO. He was a logical purist who insisted that so-called 'semantically valid' arguments are to be treated as *enthymemes* and converted into formally valid arguments by spelling out their missing premises. This purist policy can be defended by pointing to the notorious vagueness of the category of analytic truths. There are a few clear cases (like "Bachelors are unmarried"?), many clear non-cases (like "Bachelors are unhappy"?), and an enormous grey area in between. Or so most philosophers think. Quine thinks that the grey area is all encompassing, and that there is no analytic/synthetic distinction to be drawn, not even a vague one. Quine thinks the purist policy is the only game in town.

But this purist policy hides a deep problem. What are formal validity and logical truth? They are validity and truth in virtue of logical form. And what is logical form? You get at it by distinguishing logical words from non-logical or descriptive words. But formal validity and logical truth turn on the *meanings* of the logical words. Formal validity is just a species of semantic validity, and logical truth just a species of analytic truth. (As is well-known, formal validity of the argument "P, therefore C" is tantamount to logical truth of the conditional statement "If P, then C" corresponding to the argument.)

The deep problem is that there seems no sharp distinction between logical and descriptive words. There are a few clear cases of logical words, and the deductive logician's usual list gives them: propositional connectives, quantifiers, the 'is' of prediction, and the 'is' of identity. There are many clear cases of non-logical or descriptive words. But then there is a grey area, the most notorious examples being comparatives — 'taller than', older than', generally 'X-er than'. These seem to be mixed cases, with a descriptive component, the X, and a logical component, the '-er'. What are we to say of statements expressing the transitivity of comparatives, statements of the form "If a is X-er than b, and b is X-er than c, then a is X-er than c"? Do we count these logical truths, and the arguments corresponding to them formally valid? Or do we count them analytic truths, and the arguments corresponding to them semantically valid? Or do we count them as synthetic truths, and the arguments corresponding to them simply invalid unless we add the transitivity of 'X-er than' as a suppressed premise?

Quine favours the last option. His famous attack on the analytic/synthetic distinction proceeds for the most part by taking the notion of logical truth for granted. Logical truth is truth by virtue of the meanings of ... (here comes

the usual list of logical words). Quine then contrasts the precision of this notion with the vagueness of the notion of analytic truth. But then, all of a sudden and without much argument, we are told that *all* truths depend for their truth on the way the world is, that the *whole* of our knowledge faces the tribunal of experience as a corporate body, and that experience might teach us that even so-called 'logical truths' like "Either P or it is not the case that P" are false. Yet elsewhere Quine tells us that the deviant logician who wants to reject the law of excluded middle has a problem: he wants to convict us of an empirical error, but all he can really do is propose a new concept of negation. In short, the logical truths are restored: the law of excluded middle is true by virtue of the meanings of 'and' and 'not', and to reject it is to change the meaning of one of these terms.

Fortunately, these foundational issues are irrelevant to our main concern, which is with non-deductive or inductive logic. Recognition of the category of analytic but non-logical truth, and of semantic but non-logical validity, does not take us outside the realm of deduction. It does not get us to inductive or ampliative inferences. So let us return to that issue. (Later I will consider the view that inductive arguments are actually semantically valid deductive arguments.)

HISTORICAL INTERLUDE: MILL VERSUS ARISTOTLE

As we all know, deductive logic was founded by Aristotle, who worked out the logic of categorical propositions and the theory of the syllogism. Aristotle found out that there were 256 possible syllogisms that folk might use, and determined that only 24 of these were valid. So precious were these valid syllogisms that each was given its own name, and in the 13^{th} century Pope John XXI put all their names into a rhyme. Thereafter every educated person had to learn the rhyme and remember the valid syllogisms so that they might reason well.

Yet down the centuries people often complained that Aristotle's logic was trivial, uninformative or useless — precisely because valid syllogisms were not ampliative. The grumbling grew loud during the Scientific Revolution. The philosophers of the Scientific Revolution wanted to put Aristotle behind them. They dreamt of inferences that would not be trivial or uninformative or useless, ampliative inferences that would lead to something new. And they dreamt of a logic or method that might systematise such inferences, and tell us which of them were good ones, just as Aristotle had told us which syllogisms were good ones. Bacon and Descartes and Locke are but three examples of philosophers who criticised Aristotle's logic in this way. In the nineteenth century, John Stuart Mill was another.

Aristotle's critics had a point. Aristotelian logic is trivial in the sense that it deals only with relatively trivial arguments. There are lots of valid deductive arguments that Aristotelian logic cannot deal with. Perhaps the most famous everyday example is "All horses are animals. Therefore, all heads of horses are heads of animals". More important, it is hopeless to try to capture mathematical reasoning in syllogistic form. But beginning in about 1850 with Boole, deductive logic pro-

gressed way beyond Aristotelian logic. There is an irony of history here. While deductive logic was poor, one could forgive people for thinking that it needed supplementing with some non-deductive logic. But after deductive logic became rich, as it has in the last 150 years, one might suppose that anti-deductivist tendencies might have withered away. But nothing could be further from the truth. Which suggests that the belief in non-deductive or ampliative inference stems from a deeper source than Aristotle's inability to deal adequately with "All horses are animals. Therefore, all heads of horses are heads of animals". It does indeed stem from a deeper source - it stems from the idea that the logics of discovery and justification must be ampliative.

Mill argued (syllogistically, by the way!) that all genuine inferences lead to conclusions that are new, while syllogisms lead to nothing new, so that syllogisms are not genuine inferences at all. All genuine inferences, according to Mill, are inductive or ampliative inferences:

> All inference is from particulars to particulars. General propositions are merely registers of such inferences already made and short formulae for making more. The major premise of a syllogism, consequently, is a formula of this description; the conclusion is not an inference drawn *from* the formula, but an inference drawn *according to* the formula; the real logical antecedent or premise being the particular facts from which the general proposition was collected by induction. [Mill, 1843: II, iii, 4]

What did Mill mean by induction? He meant arguments from experience. He was an empiricist, who thought that knowledge came from experience. But knowledge transcends experience. So we need ampliative or inductive reasoning to get us from premises that experience supplies, to conclusions that transcend those premises.

The paradigmatic kind of inductive reasoning is *inductive generalisation*. Here is an example:

> All observed emeralds were green.
> Therefore, all emeralds are green.

This argument is invalid. But if that verdict seems harsh, deductivists might soften it by reflecting that people seldom state all of their premises. Perhaps this argument has a missing premise, to the effect that unobserved cases resemble observed cases. If we spell that premise out, we get the perfectly valid argument:

> [Unobserved cases resemble observed cases.]
> All observed emeralds were green.
> Therefore, all emeralds are green.

(By the way, if we change the conclusion from a generalisation about all emeralds to a prediction about the next case, we get so-called *singular predictive inductive inference*. That is what Mill had in mind when he said "All inference is from particulars to particulars".)

I said earlier that automobile logic is silly. But nearly everybody thinks that inductive logic is not silly. Why? The situation with the emeralds argument is symmetrical with the situation with the argument about Cadillacs. Yet most philosophers insist that they be treated differently. Most philosophers would not touch automobile logic with a barge-pole, yet insist that inductive logic must exist. Why? What is the difference between the two cases?

One obvious difference is that the missing premise of the Cadillac argument is true, while the missing premise of the emeralds argument is false. Another difference, connected with the first, is that "Unobserved cases resemble observed cases" is a much more general hypothesis than "Cadillacs are American cars". So what? It will hardly do to say that arguments with true missing premises are to be reconstructed as deductive, while arguments with false missing premises are not. Where, in the continuum of generality, will we draw a line below which we have empirical hypotheses, and above which we have material rules of inductive logic? And what do we gain by disguising a bit of false and human chauvinistic metaphysics like "Unobserved cases [always] resemble observed cases" as a principle of some fancy ampliative inductive logic?

We gain nothing, of course, and inductive logicians are smart enough to realise this. So they warm to the task of getting more plausible inductive rules than "Unobserved cases resemble observed cases". With characteristic clarity, Mill put his finger precisely on one big problem they face. Mill said that in some cases a single observation is "sufficient for a complete induction" (as he put it), while in other cases a great many observations are not sufficient. Why? Mill wrote:

> Whoever can answer this question knows more of the philosophy of logic than the wisest of the ancients, and has solved the problem of induction. [1843: III, iii, 3]

The answer to Mill's question is obvious. In the first kind of case, we are assuming that what goes for one instance goes for all, whereas in the second kind of case we are not. But philosophers do not like this obvious answer. Peter Achinstein discusses Mill's own example as follows:

> ... we may need only one observed instance of a chemical fact about a substance to validly generalise to all instances of that substance, whereas many observed instances of black crows are required to [validly?] generalise about all crows. Presumably this is due to the empirical fact that instances of chemical properties of substances tend to be uniform, whereas bird coloration, even in the same species, tend[s] not to be. [Achinstein, 2009: 8]

Quite so. And if we write these empirical facts (or rather, empirical assumptions or hypotheses) as explicit premises, then our arguments become deductions. In the first case we have a valid deduction from premises we think true, in the second case we have a valid deduction from premises one of which we think false (namely,

that what goes for the colour of one or many birds of a kind goes for all of them). Mill's question is answered, and his so-called 'problem of induction' is solved.

Other inductive logicians say that an inductive generalisation will only be 'cogent' if the observed cases are *typical* cases, or only if the observed cases are a *representative sample* of all the cases. But reflection on what 'typical' or 'representative' mean just yields a more plausible deductivist reconstruction of the emeralds argument. To say that the emeralds we have observed are 'typical' or 'representative' is just to say that their shared features are common to all emeralds. Spelling that out yields:

> [Observed emeralds are typical or representative emeralds: their shared features are common to all emeralds.]
> All observed emeralds were green.
> Therefore, all emeralds are green.

Or perhaps the generalizer about emeralds had something even more restricted in mind. Perhaps the hidden assumption was that emeralds form a 'natural kind', and that colour is one of the essential or 'defining' features of things of that kind. Spelling that out yields:

> [All emeralds have the same colour.]
> All observed emeralds were green.
> Therefore, all emeralds are green.

Of course, the second premise of this argument is now much stronger than it need be. Once we have assumed that all emeralds have the same colour, we need observe only one emerald and then can argue thus:

> [All emeralds have the same colour.]
> This emerald is green.
> Therefore, all emeralds are green.

This is an example of what old logic books called *demonstrative induction.* It is not induction, but (valid) deduction. Aristotle called it *epagoge.* He insisted that one observed case is enough for you to "intuit the general principle" *provided that the observation yields the essence of the thing* [*Prior Analytics*, 67a22]. Aristotle also described this as "a valid syllogism which springs out of induction [that is, observation]" [*Prior Analytics*, 68b15].

There is also what people call 'perfect' or 'complete' or 'enumerative' induction, where it is tacitly assumed that we have observed all the instances. Again, this is not induction but deduction. An example is:

> [The observed emeralds are all the emeralds.]
> All the observed emeralds were green.
> Therefore, all emeralds are green.

Popper mistakenly takes Aristotle's *epagoge* to be a complete induction [1963: 12, footnote 7].

Finally, there is so-called 'eliminative induction'. Once again, this is not induction, but just a special kind of deduction. An example is:

> [Either the wife or the mistress or the butler committed the murder.]
> The wife did not do it.
> The mistress did not do it.
> Therefore, the butler did it.

As the example indicates, this is a typical pattern of argument from detective stories. Sherlock Holmes argues that way all the time. So does your car mechanic to find out what is wrong with your car. So does your doctor to find out what is wrong with you. It is also the 'form of induction' advocated by Frances Bacon, High Priest of the experimental method of science, to find out the causes of things. Of course, an eliminative induction, though perfectly valid, is only as good as its major and often suppressed premise. You may get a false conclusion, if that premise does not enumerate all the 'suspects'.

As you can see, it is child's play, philosophically speaking, to reconstruct patterns of so-called 'inductive reasoning' as valid deductions. We have done it with inductive generalisation, singular predictive inference, enumerative induction, demonstrative induction, and eliminative induction. (I did not even mention *mathematical induction* — everybody agrees that that is deduction.) As I shall show later, one can also do it with abduction, and with its intellectual descendant, inference to the best explanation.

Yet most philosophers do not like these deductivist reconstructions of so-called inductive or ampliative arguments. They prefer to say, with Mill, that whether an 'inductive generalisation' is valid or cogent depends on the way the world is. This means that inductive logic, which sorts out which inductive arguments are cogent and which not, becomes an empirical science.

Deductive logic is not empirical. Empirical inquiry can tell you that the conclusion of a valid argument is false (and hence the premises as well). Empirical inquiry can tell you that the premises of a valid argument are false (though not necessarily, of course, its conclusion as well). But neither finding out that the premises are false nor finding out that the conclusion is false, shows that the argument is invalid. Empirical research can produce 'premise defeaters' and/or 'conclusion defeaters', but it cannot produce 'argument defeaters'. However, when it comes to inductive arguments, empirical research can provide 'argument defeaters' as well. An argument defeater casts doubt on the cogency of the argument, without necessarily impugning the truth of either the premises or the conclusion. The error of 'psychologism' was to suppose that logic describes how people think and is a part of empirical psychology. Despite its invocation of 'cogency' to parallel the notion of validity, inductive logic is also descriptive, part of empirical science in general, since whether an inductive argument is cogent depends on the way the world is.

Deductive validity is an all-or-nothing business, it does not come in degrees. If you have a valid argument, you cannot make it more valid by adding premises. Inductive logic is different. Inductive cogency does come in degrees. You can make

an inductive argument more cogent or less cogent by adding premises. If I have observed ten green emeralds, I can pretty cogently conclude that all emeralds are green. But my inference will be more cogent if I add a further premise about having observed ten more green emeralds. However, if I add instead the premise that I observed my ten green emeralds in the collections of a friend of mine who has a fetish about collecting green things (green bottles, green postage stamps, green gemstones, and so forth), then my argument becomes less cogent or perhaps not cogent at all.

Deductive logic, as well as being non-empirical, is *monotonic.* You cannot make a valid argument invalid by adding a premise. (Here I ignore the *relevance logicians*, who think that any valid argument can be invalidated by adding the negation of a premise as another premise.) Inductive logic is *non-monotonic.* An 'inductively valid' or cogent argument can be made invalid or non-cogent by adding a premise.

Deductivists prefer to keep logic and empirical science separate. They stick to deductive logic, monotonous and monotonic though it may be. Are they just stick-in-the-mud - or worse, closet logical positivists? Why not go in for inductive logic, which is exciting rather than monotonous and monotonic? Yet as we have seen, it is child's play to do without induction, and to reconstruct so-called inductive arguments as hypothetico-deductive ones. So why is everybody except Karl Popper reluctant to do that? Why does everybody believe in induction, and in ampliative reasoning?

The answer lies in the fact that our deductivist reconstructions of so-called inductive or ampliative arguments turn them into *hypothetico*-deductive arguments, whose missing premises are hypotheses of one kind or another — Cadillacs are American cars, Bachelors are unhappy, Unobserved cases resemble observed cases, All emeralds have the same colour, Instances of chemical properties of substances are uniform, Observed Xs are typical or representative Xs, and so forth. Hypothetico-deductive reasoning is no use to us if we want to *justify* the conclusions we reach. (It is perfectly good, however, if we want just to arrive at interesting new hypothetical conclusions, if we want a logic of discovery. I shall return to this.) If our interest is justification, then why not render hypotheses invisible by resisting deductivist reconstructions? But this is just to hide the problem of induction, not to solve it.

WITTGENSTEINIAN INSTRUMENTALISM

Mill inaugurated the view that general hypotheses are not premises of our arguments, but rules by which we infer particulars from particulars. The logical positivists said the same thing. They read in Wittgenstein's *Tractatus* (1921):

> Suppose I am given *all* elementary propositions: then I can simply ask what propositions I can construct out of them. And then I have *all* propositions, and that fixes their limits. (4.51)

> A proposition is a truth-function of elementary propositions. (5)
>
> All propositions are the results of truth-operations on elementary propositions. (5.3)
>
> All truth-functions are the results of successive applications to elementary propositions of a finite number of truth-operations. (5.12)

Schlick took the 'elementary propositions' to be particular observation statements. As usual, there is some dispute whether this reading was correct. But given that reading, general propositions are not genuine propositions at all. They are not (finite) truth-functions of particular observation statements, and so are not verifiable by observation. Given the verifiability theory of meaning, general propositions are meaningless. Thus Schlick on the general laws of science:

> It has often been remarked that, strictly, we can never speak of the absolute verification of a law... the above-mentioned fact means that a natural law, in principle, does not have the logical character of a statement, but is, rather, a prescription for the formation of statements.
>
> The problem of induction consists in asking for a logical justification of universal statements about reality ... We recognise, with Hume, that there is no logical justification: there can be none, simply because they are not genuine statements. (Schlick, as translated by Popper 1959: 37, note 7)

And Ramsay:

> Variable hypotheticals are not judgements but rules for judging. ... when we assert a causal law we are asserting not a fact, nor an infinite conjunction, nor a connection of universals, but a variable hypothetical which is not strictly a proposition at all but a formula from which we derive propositions. [Ramsey 1931: 241, 251]

If general statements, including general principles and (putative) laws of science, are not true or false propositions, what are they? What exactly are "prescriptions for the formation of statements" (Schlick) or "rules for judging" (Ramsey)? Wittgenstein's *Tractatus* did not help much, with its vaguely Kantian suggestions:

> Newtonian mechanics, for example, imposes a unified form on the description of the world. ... Mechanics determines one form of description of the world ... (6.341)
>
> ... the possibility of describing the world by means of Newtonian mechanics tells us nothing about the world: but what it does tell us something about is the precise way in which it is possible to describe it by those means. (6.342)
>
> The whole modern conception of the world is founded on the illusion that the so-called laws of nature are the explanations of natural phenomena. (6.371)

W. H. Watson, a physicist who sat at the master's feet, put it thus:

> It should be clear that the laws of mechanics are the laws of our method of representing mechanical phenomena, and since we actually choose a method of representation when we describe the world, it cannot be that the laws of mechanics say anything about the world. [Watson 1938: 52]; this is parroted by [Hanson, 1969: 325]

Toulmin and Hanson attempt to clarify this Kantian view by saying that theories are like maps, and that general laws are like the conventions of map-making or 'laws of projection':

> Our rules of projection control what lines it is permissible to draw on the [map]. Our rules of mechanics control what formulae it is permissible to construct as representing phenomena ...
>
> Perhaps what we have called "the laws of nature" are only the laws of our method of representing nature. Perhaps laws show nothing about the natural world. But it does show something about the world that we have found by experience how accurate pictures of the world ... can be made with the methods we have learned to use. [Hanson, 1969: 325-6]; this parrots [Toulmin, 1953: 108-9]

As well as flirting with these vaguely Kantian suggestions, the Wittgensteinians revert (without knowing it) to Mill's idea that general statements, including the general principles or laws of science, are 'material' rules of non-deductive inference. Ryle insisted that "the most 'meaty' and determinate hypothetical statements" like "If today is Monday, then tomorrow is Tuesday" or "Ravens are black" are not premises of arguments but material rules of inference or 'inference-licences' [Ryle, 1950, 328]. Harre said the same of empirical generalisations:

> The natural process of prediction of an instance is to state the instance as a consequence of another instance, for example, that a creature is herbivorous follows from the fact that it's a rabbit. The justification of this move ... takes us back to the generalization or its corresponding conditional ... These are not premises since they validate but do not belong in the argument that expresses the deduction. It is natural to call them the rules of the deduction. We infer a particular not from a generalization but in accordance with it. [Harre, 1960: 79-80]

Toulmin and Hanson say the same of the hypotheses or principles or (putative) laws of science:

> ... the role of deduction in physics is not to take us from the more abstract levels of theory to the more concrete ... Where we make strict, rule-guided inferences in physics is in working out, for instance, where a planet will be next week from a knowledge of its present position, velocity, and so on: this inference is not deduced from the laws of

> motion, but drawn in accordance with them, that is, as an application of them. [Toulmin, 1953, 84-5]; see also [Hanson, 1969, 337-8]

The idea that universal statements about reality are not genuine statements at all enabled Schlick to solve, or rather sidestep, the problem of induction. Watson agreed:

> It seems that the expression 'the correct law of nature' is not a proper grammatical expression because, not knowing how to establish the truth of a statement employing this form of speech, we have not given it a meaning. [Watson, 1938, 51]; see also [Hanson, 1969, 324]

But this just hides the problem — it does not solve it. If observation cannot establish the truth of a universal statement, then neither can observation establish the soundness of a material rule of inference. Humean sceptical questions about the certainty of general hypotheses or the reliability of predictions drawn from them, can simply be rephrased as sceptical questions about the usefulness of inference-licenses or the reliability of predictions drawn according to them. If answering Hume was the aim, it has not been achieved. The other arguments or motivations for the inference-licence view are equally broken-backed (as I show in my [1980]).

I said earlier that automobile logic is silly, and asked whether any serious philosopher has gone in for anything like it. Well, as we have seen, Wittgensteinian and his followers went in for it, on a massive scale. (I leave the reader to judge whether they count as serious philosophers.) I also said earlier that the widespread belief in non-deductive logic stems from the view that that deductive logic is useless either as a logic of discovery or as a logic of justification. Well, let us see.

'LOGIC OF DISCOVERY' — DEDUCTIVE OR INDUCTIVE?

The distinction between the contexts of discovery and justification is due to the logical positivists and Popper. They were sceptical about there being any *logic* of discovery. They regarded the 'context of discovery' as belonging to the province of psychology rather than logic. Popper famously declared (1959: 31): "The initial stage, the act of conceiving or inventing a theory, seems to me to neither call for logical analysis nor to be susceptible of it". That this statement occurs in a book called *The Logic of Scientific Discovery* has astonished many readers. The oddity can be partially relieved. 'Discover' is a success-word. One cannot discover that the moon is made of green cheese, because it isn't. To discover that p one must come up with the hypothesis that p, or guess that p, and then show that p is true. It is consistent to maintain that the initial 'guessing' stage is not susceptible of logical analysis, and that logic only plays a role in the second stage, where we show that p is true, prove it or justify it. This only partially relieves the oddity of Popper's claim, because he famously claims that there is no proving or justifying

our hypotheses either. All he gives us in *The Logic of Scientific Discovery* is a logical analysis of the process of empirical testing.

Because 'discover' is a success word, it is odd to speak of discovering a false hypothesis. It would be better to speak, not of the context of discovery, but of the *context of invention*. Then we can separate the question of inventing a hypothesis from the question of justifying it. But 'context of justification' is not a happy phrase either, at least in Popper's case. He thinks that while scientists can rationally evaluate or appraise hypotheses, they can never justify or prove them. So as not to beg the question against that view, it would be better to speak of the *context of appraisal*. These terminological suggestions are due to Robert McLaughlin [1982, p. 71].

Were the positivists and Popper right that there is no logic of invention, no logical analysis of the initial stage of inventing a hypothesis? No. People do not typically invent hypotheses at random or through flashes of mystical intuition or in their dreams. People typically invent new hypotheses by reason or argument. But, the pervasive thought is, these reasonings or arguments cannot be deductive, for the conclusion of a valid deduction contains nothing new. Hence we need an inductive or ampliative logic of invention (discovery).

But that, too, is wrong. We already saw that it is child's play to reconstruct inductive generalisation, singular predictive inference, enumerative induction, demonstrative induction, and eliminative induction as valid deductions. And when we did that, we did not say whether we were reconstructing 'discovery arguments' or 'justification arguments'. Let us suppose the former, and revisit one trivial example. Suppose you want to know what colour emeralds are. Do you lie on your couch, close your eyes, and somehow dream up conjectures that you will then subject to test? No. You observe an emerald and perform a trivial 'demonstrative induction' (deduction):

> [Emeralds share a colour.]
> This emerald is green.
> Therefore, all emeralds are green.

Your major premise, perhaps left un-stated, is a presupposition of the question "What colour are emeralds?".

Here is another trivial example of the same thing. Suppose you want to know what the relationship is between two measurable quantities P and Q. You have the hunch that it might be linear, or decide to try a linear relationship first. Do you lie on your couch, think up some linear equations between P and Q (there are infinitely many of them!), and then put them to the test? No. You make a couple of measurements and perform a trivial deduction:

> [$P = aQ + b$, for some a and b.]
> When $Q = 0, P = 3$, so that $b = 3$.
> When $Q = 1, P = 10$, so that $a = 7$.
> Therefore, $P = 7Q + 3$.

This is called 'curve-fitting'. It is supposed to be induction. But of course, it is really deduction.

These are trivial examples of the 'logic of invention (discovery)'. Other examples are less trivial. Newton spoke of arriving at scientific theories by *deduction from the phenomena*. Newton was right to speak of deduction here, not of induction, abduction, or anything like that. He was wrong to speak of deduction from phenomena alone. The premises of his arguments do not just contain statements of the observed phenomena. They also contain general metaphysical principles, heuristic principles, hunches. Newton first called them 'Hypotheses'. Then, anxious to make it seem that there was nothing hypothetical in his work, he rechristened them 'Rules of Reasoning in Philosophy'. (As we can see, disguising hypothetical premises as rules of ampliative reasoning, as in automobile logic, has a fine pedigree - it goes all the way back to Newton!) Newton had four 'Rules of Reasoning':

RULE I

We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances.

To this purpose the philosophers say that Nature does nothing in vain, and more is vain, when less will serve; for Nature is pleased with simplicity, and affects not the pomp of superfluous causes.

RULE II

Therefore to the same natural effects we must, as far as possible, assign the same causes.

RULE III

The qualities of bodies, which neither admit intensification nor remission of degrees, and which are found to belong to all bodies within the reach of our experiments, are to be esteemed the universal qualities of all bodies whatsoever.

RULE IV

In experimental philosophy we are to look upon propositions inferred by general induction from phenomena as accurately or very nearly true, notwithstanding any contrary hypotheses that may be imagined, till such time as other phenomena occur, by which they may be made either more accurate, or liable to exceptions.

This rule we must follow, that the argument of induction may not be evaded by hypotheses.

(*Principia*, Book III; Newton 1934, 398-400.)

Rule III enables Newton to arrive at the law of universal gravitation:

> Lastly, if it universally appears, by experiments and astronomical observations, that all bodies about the earth gravitate towards the earth,

> and that in proportion to the quantity of matter that they severally contain; that the moon likewise, according to the quantity of its matter, gravitates toward the earth; that, on the other hand, our sea gravitates toward the moon; and all the planets one toward another; and the comets in like manner toward the sun: we must, in consequence of this rule, universally allow that all bodies whatsoever are endowed with a principle of mutual gravitation ... [Newton, 1934, 399]

Here Newton lists the 'phenomena' that experiment and astronomical observation have revealed to him. (These 'phenomena' are highly theory-laden, of course, but that is not the issue here.) He then applies Rule III "The qualities ... which are found to belong to all bodies within the reach of our experiments, are ... the universal qualities of all bodies whatsoever" and deduces "all bodies whatsoever are endowed with a principle of mutual gravitation".

So, Newton did deduce things, but not just from 'phenomena', also from general metaphysical principles disguised as 'Rules of Reasoning'. Or so it seems — as we will see shortly, careful reading reveals another interpretation, in which the 'Rules of Reasoning' are not metaphysical principles at all but rather *epistemic* principles. Here what matters is that once we spell out Newton's so-called 'Rules of Reasoning' as explicit premises, whether metaphysical or epistemic, his arguments all become deductive.

Newtonian deduction from the phenomena is ubiquitous in science. There is now quite a body of literature analysing real episodes from the history of science and demonstrating the fact. Examples include Cavendish's deduction of the electrostatic inverse square law (see [Dorling, 1973a; 1973b]), Einstein's deduction of the photon hypothesis (see [Dorling, 1971]), Rutherford's deduction of the Rutherford model of the atom (see [McLaughlin, 1982; Musgrave, 1989]), and Einstein's deductions of the special and general theories of relativity (see [Zahar, 1973; 1983]). Sometimes the major and often 'missing' premises of these deductions are general metaphysical principles like Newton's. Sometimes they are more specific hypotheses that make up the 'hard core' of a particular scientific research programme. Imre Lakatos and his followers have produced many case-studies of the latter kind (see the papers collected in [Howson, 1973; Latsis, 1976]). What all the examples show is that there is a logic of discovery, despite positivist/Popperian orthodoxy, and that it is deductive logic, despite philosophic orthodoxy in general.

What of the argument that logic of discovery must be non-deductive or ampliative because discovery is by definition coming up with something new and the conclusion of a valid deduction contains nothing new? Here we must distinguish logical novelty from psychological novelty. True, the conclusion of a valid deduction is not 'logically new', which is just a fancy way of saying that it is logically contained in the premises. But the conclusion of a valid deduction can be psychologically new. We can be surprised to discover the consequences of our assumptions. When Wittgenstein said that in logic there are no surprises, he was just wrong: Hobbes was astonished that Pythagoras's Theorem could be deduced from Euclid's axioms. Moreover, the conclusion of a valid deduction can have

interesting new properties not possessed by any of the premises taken singly — being empirically falsifiable, for example. Nor, finally, do deductivist reconstructions of inventive arguments take all the inventiveness out of them and render hypothesis-generation a matter of dull routine. The originality or inventiveness lies in assembling a new combination of premises that may yield a surprising conclusion. It also lies in obtaining that conclusion, which in interesting cases is no trivial or routine task.

The positivists and Popper were wrong. There is a logic of invention (discovery). And it is deductive logic, or is best reconstructed as such. It will be objected that hypothetico-deductive inventive arguments are only as good as their premises. And further, that the 'missing' premises of inventive arguments, the general metaphysical or heuritstic principles that lie behind them, are very often false. It is not true that unobserved cases always resemble observed cases, that the relationship between two measurable quantities is always linear, that Nature is simple, that like causes have like effects, and like effects like causes, and so forth. Moreover, the objector might continue, scientists know this. Is it plausible to think that scientists argue from premises that they know to be false?

There is no evading this objection by viewing the arguments as non-deductive arguments which proceed according to rules of inductive reasoning. It is equally implausible to think that scientists reason according to rules that they know to be unsound.

Of course, that a hypothetico-deductive inventive argument contains a premise that is false, or at least not known to be true, is fatal to the idea that the argument proves or establishes its conclusion. But we should not mix up discovery and proof, or the logic of invention and the logic of justification. There is nothing wrong with getting new hypotheses from general heuristic principles that are not known to be true. There is not even anything wrong with getting new hypotheses from general principles that are known to be false, though they have some true cases.

This may fail to convince. If so, we can if we wish recast hypothetico-deductive inventive arguments so that they become arguments that are not only sound but known to be so (at least so far as their general heuristic principles are concerned). To see how, let us return to Newton. So far we have had Newton deducing things, not just from 'phenomena', but also from general metaphysical principles disguised as 'Rules of Reasoning'. But careful reading reveals another interpretation, in which the 'Rules of Reasoning' are not metaphysical principles at all but rather *epistemic* principles, about what we ought to *admit, assign, esteem,* o*r look upon* to be the case. (It was, so far as I know, John Fox who first drew attention to this reading in his 1999.) On this reading, Newton does not deduce the law of universal gravitation (G) — what he deduces is that we must "allow that" or "esteem that" or "look upon it that" G is the case. In short, Newton's conclusion is that *it is reasonable for us to conjecture that* G. And his 'Rules of Reasoning' are general epistemic or heuristic principles like "*It is reasonable for us to conjecture that* the qualities ... which are found to belong to all bodies within the reach of our experiments, are ... the universal qualities of all bodies whatsoever". This

epistemic principle is, I submit, true and known to be true. Its truth is not impugned by the fact that a conjecture reached by employing it might subsequently get refuted. If we reasonably conjecture something and later find it to be false, we find out that our conjecture is wrong, not that we were wrong to have conjectured it. This epistemic interpretation makes sense of Newton's Rule IV, in which Newton admits that any conclusion licensed by or reached from his first three Rules might be refuted. The purpose of Rule IV is to deny that sceptical proliferation of alternative hypotheses counts as genuine criticism ("This rule we must follow, that the argument of induction may not be evaded by hypotheses"). This is not trivial. It is important to see that the sceptical proliferation of alternative hypotheses is no criticism of any hypothesis we might have. It is only an excellent criticism of the claim that the hypothesis we have is proved or established by the data which led us to it.

Return to so-called 'inductive generalisation', for example:

> All observed emeralds were green.
> Therefore, all emeralds are green.

We validated this argument by spelling out its missing premise, to obtain:

> [Unobserved cases resemble observed cases.]
> All observed emeralds were green.
> Therefore, all emeralds are green.

We then objected that this missing premise is a piece of false and human chauvinistic metaphysics, and that nothing is gained by replacing obvious invalidity by equally obvious unsoundness. But here is a better deductivist reconstruction of the argument:

> [*It is reasonable to conjecture that* unobserved cases resemble observed cases.]
> All observed emeralds were green.
> Therefore, *it is reasonable to conjecture that* all emeralds are green.

This argument does not, of course, establish that emeralds are all green. But our interest here is invention (discovery), not proof.

Or consider *analogical reasoning*, another alleged pattern of inductive inference which in its simplest form goes like this:

> a and b share property P.
> a also has property Q.
> Therefore, b also has property Q.

We might validate this by adding an obviously false metaphysical missing premise:

> [If a and b share property P, and a also has property Q, then b also has property Q.]
> a and b share property P.

> a also has property Q.
> Therefore, b also has property Q.

But if our interest is invention rather than proof, we can reconstruct analogical reasoning as a sound deductive argument:

> [If a and b share property P, and a also has property Q, then *it is reasonable to conjecture that* b also has property Q.]
> a and b share property P.
> a also has property Q.
> Therefore, *it is reasonable to conjecture that* b also has property Q.

I submit that the missing premises of these deductivist reconstructions, premises about what it is reasonable for us to conjecture, are true and known to be true. That might be disputed. These missing premises simply acknowledge that scientists are inveterate 'generalisers from experience'. The same applies to ordinary folk in the common affairs of life. Small children who have once burned themselves on a hot radiator do not repeat the experiment — they jump to conclusions and avoid touching the radiator again. The same applies to animals, as well. Popper tells the nice story of the anti-smoking puppy who had a lighted cigarette held under his nose. He did not like it, and after that one nasty experience he always ran away sneezing from anything that looked remotely like a cigarette.

It seems that 'jumping to conclusions' is 'hard-wired' into us, part of the hypothesis-generating 'software' that Mother Nature (a.k.a. Natural Selection) has provided us with. Sometimes the 'hard-wiring' runs deep, being built into the perceptual system, and may be quite specific. A famous example concerns the visual system of the frog, which contains special mechanisms for detecting flies. A fly gets too close to a frog and triggers the mechanism, whereupon the frog catches and eats the fly. The frog's eye is specially designed (by Natural Selection) for detecting flies. Similar discoveries have been made about the eyes of monkeys. Monkey eyes have special cells or visual pathways that are triggered by monkey hands. Of course, all this is unconscious. But if we adopt Dennett's 'intentional stance' and attribute 'as if' beliefs to frogs and monkeys, we can see that they form beliefs on the basis of visual stimuli combined with general principles that are hard-wired into their visual systems. The beliefs that are formed in this way may be false. Experimenters can fool the frog into trying to eat a small metal object introduced into the visual field and moved around jerkily as a fly might move. Experimenters can fool baby monkeys into reaching out for a cardboard cut-out shaped roughly like a monkey-hand.

But is it *reasonable* for us to proceed in this way? Philosophers have long been aware of inbuilt generalising tendencies. Frances Bacon said: "The human understanding is of its own nature prone to suppose the existence of more order and regularity in the world than it finds" (*Novum Organum*, Book I, Aphorism xlv). Bacon deplored this and tried to get rid of it. Hume deplored it, thought it could not be got rid of, and deemed us all irrational. But in deploring our generalising tendencies, Bacon and Hume mixed up invention (discovery) and proof (appraisal).

Hypotheses arrived at by 'jumping to conclusions' are not thereby shown to be true. But we need to navigate ourselves around the world, and forming beliefs in this way is a perfectly reasonable way to begin.

Does this mean that positivist-Popperian orthodoxy was basically correct? Is the context of invention (wrongly ascribed to the province of psychology and deemed incapable of logical analysis, but no matter) irrelevant to the context of appraisal? No. To describe a conjecture as a reasonable conjecture is to make a minimal appraisal of it. And such minimal appraisals are important. Philosophers tediously and correctly point out that infinitely many possible hypotheses are consistent with any finite body of data. Infinitely many curves can be drawn through any finite number of data-points. We observe nothing but green emeralds and hypothesize that all emeralds are green — why not hypothesise that they are grue or grack or grurple? Scientists, not to mention plain folk, are unimpressed with the philosopher's point, and do not even consider the gruesome hypotheses produced in support of it. What enables them to narrow their intellectual horizons in this way, and it is reasonable for them to do so? What enables them to do it are epistemic principles about reasonable conjecturing, which are, so far as we know, true. These principles are not necessarily true. We can imagine possible worlds in which they would deliver more false hypotheses than true ones, and thus be unreliable. But just as it may be reasonable to persist in a false belief until it is shown to be false, so also it may be reasonable to persist in an unreliable belief-producing mechanism until it is shown to be unreliable. And nobody has shown that the belief-producing mechanisms I have been discussing are unreliable.

So much for the context of invention (discovery). I have resisted the idea that the logic of invention (discovery) must be inductive or ampliative. But what about the context of justification? Surely justification requires ampliative reasoning. Which brings me to the last, and most important, reason for the widespread belief in inductive logic.

'LOGIC OF JUSTIFICATION' — DEDUCTIVE OR INDUCTIVE?

People reason or argue not just to arrive at new beliefs, or to invent new hypotheses. People also argue for what they believe, reason in order to give reasons for what they believe. In short, people reason or argue to show that they know stuff. Knowledge is not the same as belief, not even the same as true belief — knowledge is justified true belief. People reason or argue to justify their beliefs.

Seen from this perspective, deductive arguments are sadly lacking. To be sure, in a valid deductive argument the premises are a conclusive reason for the conclusion — if the premises are true, the conclusion must be true as well. But if we want to justify a belief, producing a valid deductive argument for that belief is always question-begging. The argument "C, therefore C" is as rigorously valid as an argument can be. But it is circular, and obviously question-begging. Non-circular valid deductive arguments for C simply beg the question in a less obvious way. Moreover, the premises of a non-circular valid argument for C might be false,

even if C is true. Such is the case with some of our deductivist reconstructions of arguments from experience. An inveterate generaliser observes a few ravens, notices that they are all black, and jumps to the conclusion that all ravens are black. His argument is invalid. But it does not help to validate it by adding the false inductive principle that unobserved cases always resemble observed cases. He might have tacitly assumed that to arrive at his new belief — remember, he is an inveterate generaliser. But if our interest is justification, there is no point replacing obvious invalidity by equally obvious unsoundness. And even where it is not obvious that the heuristic principle is false, spelling it out in a deductivist reconstruction does no good if we want justification. It does no good because it is not *known* to be true. We want a reason for our conclusion C, and produce a valid deductive argument "P, therefore C" to obtain one. But now we need a reason for the stronger claim P which logically contains C. And so on, *ad infinitum,* as sceptics tirelessly and rightly point out.

But what if we start from premises for which no further reason is required, premises whose truth we know directly from observation or experience? And what if there are inductive or ampliative arguments from our observational premises to our conclusions? These arguments are not deductively valid, to be sure. Induction is not deduction. But inductive arguments might be cogent, they might give us good though defeasible reasons for their conclusions. We do not need ampliative arguments in the logic of criticism. We might not even need them in the logic of invention (discovery). But we surely do need them in the logic of justification.

Or so everybody except Popper and me thinks. We are here confronted with the problem of induction. I think Popper has solved this problem. Let me briefly explain how. (What follows is a controversial reading of Popper, which is rejected by many self-styled 'Popperians'. For more details, see my [2004] and [2007].)

The key to Popper's solution is to reject *justificationism*. What is that? As everybody knows, the term 'belief' is ambiguous between the *content* of a belief, what is believed, the proposition or hypothesis in question, and the *act* of believing that content. I shall call a belief-content just a 'belief', and a belief-act a 'believing'. Talk of 'justifying beliefs' inherits this ambiguity. Do we seek to justify the belief or the believing of it? It is obvious, I think, that we seek to justify believings, not beliefs. One person can be justified in believing what another person is not. I can be justified in believing today what I was not justified in believing yesterday. The ancients were justified in believing that the earth does not move, though of course we are not. Despite these platitudes, justificationism is the view that a justification for a believing must be a justification for the belief.

Given justificationism, we must provide some sort of inductive or ampliative logic leading us from evidential premises to evidence-transcending conclusions. At least, we must provide this if any evidence-transcending believings are to be justified believings. But if we reject justificationism, we need no inductive or ampliative reasoning. Our evidence-transcending believings might be justified even though our evidence-transcending beliefs cannot be. Of course, we need a theory of *when* an evidence-transcending believing is justified. Popper's general story is

that an evidence-transcending believing is justified if the belief in question has withstood criticism. As we saw, the logic of criticism is entirely deductive.

Popper's critics object that he smuggles in inductive reasoning after all. In saying that having withstood criticism is a reason for believing, Popper must be assuming that it is a reason for belief as well. But these critics smuggle in precisely the justificationist assumption that Popper rejects.

This is all terribly abstract. To make it concrete, let us consider abduction, and its intellectual descendant, inference to the best explanation (IBE). Abduction is generally regarded as the second main type of ampliative reasoning (the other being induction). Abduction was first set forth by Charles Sanders Peirce, as follows:

> The surprising fact, C, is observed.
> But if A were true, C would be a matter of course.
> Hence, ... A is true.
> [C.S. Peirce 1931-1958, Vol. 5, p. 159]

Here the second premise is a fancy way of saying "A explains C".

By the way, abduction was originally touted, chiefly by Hanson, as a long neglected contribution to the 'logic of discovery'. It is no such thing. The explanatory hypothesis A figures in the second premise as well as the conclusion. The argument as a whole does not generate this hypothesis. Rather, it seeks to justify it. The same applies, despite its name, to 'inference *TO* the best explanation' (IBE). Abduction and IBE both belong in the context of appraisal (justification) rather than in the context of invention (discovery).

Abduction is invalid. We can validate it by viewing it as an *enthymeme* and supplying its missing premise "Any explanation of a surprising fact is true". But this is no use — it merely trades obvious invalidity for equally obvious unsoundness. The missing premise is obviously false. Nor is any comfort to be derived from weakening it to "Any explanation of a surprising fact is *probably* true" or to "Any explanation of a surprising fact is *approximately* true". (Philosophers have cottage-industries devoted to both of these!) It is a surprising fact that marine fossils are found on mountain-tops. One explanation of this is that Martians came and put them there to surprise us. But this explanation is not true, or probably true, or approximately true.

IBE attempts to improve upon abduction by requiring that the explanation is the best explanation that we have. It goes like this:

> F is a fact.
> Hypothesis H explains F.
> No available competing hypothesis explains F as well as H does.
> Therefore, H is true.
> [William Lycan, 1985, p. 138]

This is better than abduction, but not much better. It is also invalid. We can validate it by viewing it as an *enthymeme* and supplying its missing premise "The

best available explanation of a (surprising) fact is true". But this missing premise is also obviously false. Nor, again, will going for probable truth or approximate truth help matters.

But wait! Peirce's original abductive scheme was not quite what we have considered so far. Peirce's original scheme went like this:

> The surprising fact, C, is observed.
> But if A were true, C would be a matter of course.
> Hence, *there is reason to suspect that* A is true.

This is also invalid. But to validate it the missing premise we need is "*There is reason to suspect that* any explanation of a surprising fact is true". This missing premise is, I suggest, true. After all, the epistemic modifier "There is reason to suspect that ..." weakens the claim considerably. In particular, "There is reason to suspect that A is true" can be true even though A is false. So we have not traded obvious invalidity for equally obvious unsoundness. Peirce's original scheme may be reconstructed so as to be both valid and sound. Why does everybody misread Peirce's scheme and miss this obvious point? Because everybody accepts justificationism, and assumes that a reason for suspecting that something is true must be a reason for its truth.

IBE can be rescued in a similar way. I even suggest a stronger epistemic modifier than "There is reason to suspect that ...", namely "There is reason to believe (tentatively) that ..." or equivalently, "It is reasonable to believe (tentatively) that ...". What results when this missing premise is spelled out is:

> [*It is reasonable to believe that* the best available explanation of a fact is true.]
> F is a fact.
> Hypothesis H explains F.
> No available competing hypothesis explains F as well as H does.
> Therefore, *it is reasonable to believe that* H is true.

This is valid and instances of it might well be sound. Inferences of this are employed in the common affairs of life, in detective stories, and in the sciences. Why does everybody misread IBE and miss this obvious point? Because everybody accepts justificationism, and assumes that a reason for believing that something is true must be a reason for its truth. (The cottage industries devoted to probable truth and approximate truth stem from the same source.)

All the criticisms of IBE presuppose justificationism. People object that the best available explanation might be false. Quite so — and so what? It goes without saying that any explanation might be false, in the sense that it is not necessarily true. But it is absurd to suppose that the only things we can reasonably believe are necessary truths. People object that being the best available explanation of a fact does not show that something is true (or probably true or approximately true). Quite so — and again, so what? This assumes the justificationist principle that a reason for believing something must be a reason for what is believed. People

object that the best available explanation might be the "best of a bad lot" and actually be false. Quite so — and again, so what? It can be reasonable to believe a falsehood. Of course, if we subsequently *find out* that that the best available explanation is false, it is no longer reasonable for us to believe it. But what we find out is that what we believed was wrong, not that it was wrong or unreasonable for us to have believed it.

What goes for IBE goes for so-called 'inductive arguments' in general. They can be turned into sound deductive *enthymemes* with epistemic principles among their premises and epistemic modifiers prefacing their conclusions. Let us confine ourselves to inductive generalisation or singular predictive inference. In the context of justification we require a stronger epistemic modifier than "It is reasonable to conjecture that ...". We need "It is reasonable to believe that ...". For singular predictive inference we obtain:

> [*It is reasonable to believe that* unobserved cases resemble observed cases.]
> All observed emeralds have been green.
> Therefore, *it is reasonable to believe that* the next observed emerald will be green.

Robert Pargetter and John Bigelow (1997) suggest an improved version of this, in which a tacit total evidence assumption is made explicit:

> All observed emeralds have been green.
> This is all the relevant evidence available.
> Therefore, *it is reasonable to believe that* the next observed emerald will be green.

However, as in the above formulation, Pargetter and Bigelow do not spell out or make explicit the general epistemic principle involved here — "If all observed As have been B, and if this is all the relevant evidence available, then *it is reasonable to believe* that the next observed A will be B". They do not spell this principle out because they regard it is analytically or necessarily true, true by virtue of the meaning of the term 'reasonable', so that the argument as it stands is semantically though not logically valid. They say, of arguments like the emeralds argument as set out above:

> They are, of course, not *formally* valid ... They are valid just in the sense that it is not possible for their premises to be true while their conclusions are false. They are valid in the way that arguments like these are valid: 'This is red, so it is coloured', 'This is square, so it is extended', and so on. The validity of the emeralds argument rests not just on its logical form but on the nature of *rationality*. [Pargetter and Bigelow, 1997, p. 70]

Now I do not want to quarrel about whether 'Anything red is coloured' or 'Anything square is extended' are analytic or necessary truths, as Pargetter and Bigelow

evidently think. But I do wonder whether it is analytic that "If all observed As have been B, and if this is all the relevant evidence available, then *it is reasonable to believe* that the next observed A will be B". This principle conflicts with the following Humean justificationist principle: "It is reasonable to believe a conclusion only if your premises establish that it is true or probably true (more likely true than not)". I do not think this Humean principle is 'conceptually confused'. So neither do I think the anti-Humean principle an analytic truth. But this is a family quarrel among deductivists, so I shall say no more about it (there is more in my [1999]).

Deductivists have a different family quarrel with John Fox. Fox is generally sympathetic to deductivist reconstructions of so-called inductive arguments as deductive arguments with epistemic principles among their premises. He says that one can be a "deductivist without being an extreme inductive sceptic, by holding that the best analysis of why inductive beliefs are rational when they are displays no *inferences* but deductively valid ones as acceptable", where the inferences "conclude not to predictions or generalisations, that is, not to inductive beliefs, but to judgements about their reasonableness" [Fox, 1999, pp. 449, 456]. But Fox thinks that this is not enough:

> ... real-life arguers conclude to something further, which is *not* a deductive consequence of their premises: to the generalisations or predictions *themselves.* ... In his primary concern to establish how surprisingly much can be reached simply by deduction, Musgrave seems simply to have overlooked both this further step and its non-deductive character. [Fox, 1999, 456]

Fox says that all we need to get from a conclusion of the form "It is reasonable to believe that P" to P itself is a further very simple non-deductive inference which he calls an *epistemic syllogism.* Examples are:

> It is reasonable to believe that P.
> Therefore, P.
>
> One should accept that P.
> Therefore, P.

Epistemic syllogisms are obviously invalid, and could only be validated by invoking absurd metaphysical principles like "Anything that it is reasonable to believe is true" or "Anything that one should accept as true is true". But here Fox makes an ingenious suggestion. He does not try to validate epistemic syllogisms but he does think that they can be trivially 'vindicated'. To vindicate an argument is to show that, given its premise(s), it is reasonable to accept its conclusion. The premise of the epistemic syllogism is that it is reasonable to believe that P. If this is correct, then trivially it is reasonable to conclude, further, that P. Which vindicates epistemic syllogisms: "Indeed, precisely if these *deductively* drawn conclusions are *correct*, it is *reasonable* so to conclude" [Fox, 1999, p. 456].

This is clever — but is it correct? The matter turns on the word 'conclude', which is ambiguous between inferring and believing. Fox distinguishes a weak sense of 'infer' whereby one infers a conclusion from some premises without coming to believe it, from a strong sense of 'infer' whereby "to infer a conclusion from premises is to *come to accept it on their basis*" [Fox, 1999, p. 451]. I say that you infer in the strong sense if you first infer in the weak sense and then, as a result of having made that inference, come to accept or believe the conclusion. This can happen. But being caused to believe a conclusion, by inferring it from premise(s) that you believe, is not some special 'strong' kind of inferring. Making the inference is one mental act, coming to believe its conclusion is another. The former can cause the latter. But coming to believe something is not the conclusion of the inference, it is the effect of making it.

Aristotle's so-called 'practical syllogism', whose premises are statements and whose conclusion is an action, is an oxymoron. Fox agrees, but thinks his epistemic syllogisms are different:

> Aristotle's 'practical syllogism' was not an inference at all. Its 'premise' was a proposition, to the effect that one should do x; its conclusion was the action of doing x. When the premise is that one should accept p, coming to accept p is *doing* just what the premise says one should, the 'conclusion' of an Aristotelian practical syllogism. But doing this is precisely (strongly) inferring in accordance with the pattern I vindicated above. Because here inference is involved, the term 'syllogism' is more apt than in most practical syllogisms. [Fox, 1999, p. 451]

I can see no difference between Aristotle's practical syllogism and Fox's epistemic syllogism. Both involve or are preceded by inferences. In Aristotle's case, you infer that you should do x from some premise(s). In the epistemic case, you infer that you should accept or believe P from some premise(s). The further steps, actually doing x or accepting P, are actions rather than the conclusions of arguments.

Fox's 'vindication' of his epistemic syllogisms seems trivial: from the premise "It is reasonable to believe that P" the conclusion "It is reasonable to believe that P" trivially follows. But "It is reasonable to believe that P" does not say that any way of arguing for P is reasonable. It says nothing about any way of arguing for P — it speaks only of P. In particular, it does not say that "It is reasonable to believe that P. Therefore, P" is a reasonable way to argue for P.

Why does Fox think his obviously invalid epistemic syllogisms are necessary? Why does he think that "real life arguers" need to argue, not just that it is reasonable to believe some evidence-transcending hypothesis, but also for that hypothesis itself? Well, if you assume that a reason for believing P must be a reason for P itself, then you will need to invoke epistemic syllogisms to get you (invalidly) from a reason for believing P to a reason for P. But we should get rid of that justificationist assumption.

GETTING STARTED — 'FOUNDATIONAL BELIEFS'

In discussing induction, I talked of evidence and evidence-transcending hypotheses. And in discussing abduction and IBE, I talked of having 'facts' that require explanation. What is the source of this evidence or of these facts? There are two main sources, sense-experience and testimony. Justificationism bedevils discussion of these matters, too.

My nose itches and I scratch it. The itch causes (or helps cause) the scratching. The itch is also a reason for the scratching (or part of the reason). In cases like this, we are happy with the thought that causes of actions are reasons for them. The experience (the itch) is both a cause and a reason for the action (the scratching).

I see a tree and I form the belief that there is a tree in front of me. The tree-experience causes (or helps cause) the believing. Is the tree experience also a reason (or part of the reason) for the believing? The two cases seem symmetrical. Yet many philosophers treat them differently. Many philosophers are unhappy with the thought that the tree-experience is both a cause and a reason for the tree-believing. Why the asymmetry? Justificationism lies behind it. Justificationism says that a reason for believing something must be a reason for what is believed. What is believed is a statement or proposition. Only another proposition can be a reason for a proposition. But perceptual experiences are not propositions, any more than itches or tickles are. So my tree-experience cannot be a reason for my tree-belief, and cannot be a reason for my tree-believing either.

If we reject justificationism, we can allow that perceptual experiences are reasons as well as causes of perceptual believings (though not, of course, for the perceptual beliefs, the propositions believed). We can even allow that they are good reasons. They are not conclusive reasons, of course, but defeasible ones. There is the ever-present possibility of illusion or hallucination. The tree-belief transcends the tree-experience, and future experiences may indicate that it is false. Still, it is reasonable to "trust your senses" unless you have a specific reason not to.

In support of this, we can regard perceptual belief as a case of IBE. A simple example, formulated in the usual way, is: "I see a cat in the corner of the room. The best explanation of this is that there is a cat in the corner of the room. Therefore, there is a cat in the corner of the room". But this formulation is wrong. The question was not "Why is there a cat in the corner of the room?", but rather "Why do you believe that there is a cat in the corner of the room?". What we are trying to justify or give a reason for is not the statement that there is a cat in the corner of the room, but rather my coming to believe this. So the conclusion ought to be "It is reasonable to believe that there is a cat in the corner of the room". And the missing premise required to convert the argument into a perfectly valid deduction is "It is reasonable to believe the best explanation of any fact". Of course, my reasonable perceptual belief might turn out to be false. If evidence comes in of hallucination or some less radical kind of perceptual error, I may concede that my perceptual belief was wrong — but that does not mean that I was wrong to have believed it.

Much the same applies to testimony. Somebody tells me something and I come to believe it. Is the testimony a reason as well as a cause for my believing? Many philosophers are unhappy with the thought that it is. Again, justificationism lies behind this. My hearing the testimony is not a proposition, any more than an itch or a tickle is. So my hearing the testimony cannot be a reason for my belief, and cannot if justificationism is right be a reason for my believing either.

If we reject justificationism, we can allow that testimony is a reason as well as a cause of believing (though not, of course, for what is believed). We can even allow that it is a good reason. It is not a conclusive reason, of course, but a defeasible one. There is the ever-present possibility that my informant is misinformed or even lying to me. Future experience may indicate that the belief I acquired from testimony is false. Still, it is reasonable to "trust what other folk tell you" unless you have a specific reason not to.

These reflections on the role of sense-experience and testimony are really no more than common sense. These 'sources of knowledge' — or rather, sources of reasonable believings - are simply ways of getting started. Sense-experience and testimony yield foundational beliefs, 'foundational' not in the sense that they are certain and incorrigible but only in the sense that they do not arise by inference from other beliefs. (For more on all this, see my [2009].)

And so I conclude. We do not need inductive or ampliative logic anywhere — not in the context of criticism, not in the context of invention, and not in the context of appraisal either.

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