some inductive inferences are as probable in relation to the premisses as non-sceptics think they are. But the premisses of my proofs were, principally, statements of logical probability; and propositions of this kind, or at least the published *systems* of propositions of this kind, lie under certain definite objections from philosophers, as well as under a less definite but even more damaging suspicion. So, although my purposes in this book are entirely polemical and critical, and I therefore neither have attempted nor will attempt anything systematic enough to be called a theory of logical probability, the arguments of Part One cannot possibly receive a fair hearing, unless I can show that the objections commonly entertained against the theory of logical probability are mistaken. This, and this alone, is the reason for the existence of Part Two.

The theory of logical probability is itself, however, only one form, though certainly the most ambitious form, that non-deductive logic has assumed; and some at least of the objections which have been made to it are equally objections to non-deductive logic in *any* form. In addition to that, even those objections which are peculiar to the theory of logical probability all stem, I believe, from mistakes about the relation between non-deductive and deductive logic, or from mistakes about deductive logic itself. For these two reasons, while my main object in this Part is to defend the arguments of Chapters V–VII, by defending the theory of logical probability, a considerable proportion of what follows is not about the theory of logical probability specifically, but about non-deductive logic in general; and this in turn required that considerable attention be devoted to deductive logic itself.

# IX

# THE MYTH OF FORMAL LOGIC

## (i)

WHEN I mention logic in the first four sections of this chapter, I mean deductive logic; and, expressed in graffito style, what I believe about deductive logic is this: cases rule. I do not mean that there are no general truths at all in logic. I think there are plenty, and I will mention some in a minute. What I mean is that hardly any of the true propositions of logic are purely formal. But I need to explain what I mean by 'purely formal', and before that, what I count as a proposition of logic.

That 'All swans are black and Abe is a swan' entails 'Abe is black', or that it does not; that the argument from the former proposition to the latter is valid, or that it is invalid; that the latter is a logical consequence of the former, or that it is not: these I take to be different ways of saying the same thing, and I count any proposition of this kind as a proposition of logic. That is usual enough. What is less usual is this: I do not count anything as a proposition of logic *unless* it entails a proposition of this kind. Thus what I call propositions of logic are only a proper subset of what would usually be called so, though as against that, they are the propositions which are the *raison d'être* of logic, on almost any view of logic.

I will call propositions of this kind 'judgements of validity or of invalidity'. I count something as a proposition of logic, then, if and only if it is a judgement of validity or of invalidity; that is, if and only if it says categorically, of some concrete argument or other, that it is valid or that it is invalid, or that the conclusion is or is not entailed by, or a logical consequence of, the premiss.

A judgement of validity or of invalidity may be *singular*, like the example I just gave: that is, it may be about just one argument. But we also make *general* judgements of validity or of invalidity. We pick out a certain *class* of arguments, and say that *every* member of that class is valid, or that every member of it is invalid. A general judgement of validity or of invalidity entails singular ones. For example, the singular judgement of validity mentioned above is entailed by many general ones, including: '"All $F$ are $G$ and $x$ is $F$" entails "$x$ is $G$", for all $x$, all $F$, all $G$.'

This last statement is not only a general judgement of validity, but is one which is purely formal in my sense. For I call a general judgement of validity or of invalidity 'purely formal', if and only if, in order to pick out the class of arguments in question, it employs at least one individual variable, or predicate variable, or propositional variable, and places no restriction on the values that that variable can take: that is, any propositional constant whatever can replace the variable if it is a propositional variable, any predicate constant can replace it if the variable is a predicate variable, and any individual constant can replace the variable if it is an individual variable.

Formal logic aspires to find judgements of validity or of invalidity which are not only true and general, but also purely formal in *at least* my sense. My thesis is that, above a low level of generality, there are few or no such things to be found.

Outside the purely formal, though, I admit that true general propositions of logic are common enough. For example, 'For any necessarily true $p$, and any contingent $q$, "$p$" does not entail "$q$"'; or again, 'For any contingent $p$ and any necessarily false $q$, "$p$" does not entail "$q$".' These are general judgements of invalidity, and I think that both of them are true. But they are not counter-examples to my thesis, of course, because they are not purely formal. They obviously do place restrictions on the values that can be taken by the propositional variables which they use to pick out the class of arguments in question.

Then again, there are plenty of true propositions of logic which *are* purely formal, once the name 'propositions of logic' is not confined, as I confine it, to judgements of validity or of invalidity. For example: 'If $p$ entails $q$ and $q$ entails $r$ then $p$ entails $r$'; or, again, 'if $p$ entails $q$ then $p$-and-$r$ entails $q$, for any $r$'. Here I have left the quantification mainly tacit, but everyone will 'read it in', and will then see that these truths are purely formal in my sense. For they allow any proposition at all as a value of any of the propositional variables. But then, these things are not propositions of logic in my sense, since they do not pronounce any categorical judgement of validity or of invalidity. They are only truths of (what I will call) *meta*logic. All they say is, that certain arguments are valid *if certain other arguments are.*



## (ii)

I first consider purely formal judgements of invalidity.

Here is one which I think is true; and if it is, it will be obvious that there are many others like it. '"All swans are black and $x$ is black" does not entail "$x$ is a swan", for any $x$'.

This truth is, of course, of so low a degree of generality, and hence of formality, that most logicians would strenuously object to its being called a *purely formal* logical truth at all. But it *is* purely formal in my sense, obviously; and no apology is needed for that. It is a very humble fragment of formal logical truth, to be sure. Still, you might go farther and fare worse. In fact, if you go *much* farther, and allow *predicate* variables, or propositional ones, freely into your judgements of invalidity, you are *sure* to fare worse. For example, '"All $F$ are $G$ and $x$ is $G$" does not entail "$x$ is $F$", for any $x$, any $F$, any $G$', is false; and so is '"If $p$ then $q$, and $q$" does not entail "$p$", for any $p$, any $q$'.

The argument

(a) Hume is a father
―――――――――――
Hume is a male parent

is valid. It is a logical consequence of the supposition that Hume is a father, that he is a male parent. And since if $p$ entails $q$, then $p$-and-$r$ entails $q$, for any $r$, the following two arguments are also valid.

(b) All male parents are fathers
Hume is a father
―――――――――――
Hume is a male parent;

and

(c) If Hume is a male parent then Hume is a father
Hume is a father
―――――――――――
Hume is a male parent.

Arguments (b) and (c) are instances respectively of the 'fallacy of the undistributed middle term', and of the 'fallacy of affirming the consequent'. As these arguments are valid, it is not true that every argument which is an instance of either of those forms is invalid.

Of course, *subject to certain restrictions*, all instances of those forms are invalid. For example, the following statement is true:

'"All $F$ are $G$ and $x$ is $G$" does not entail "$x$ is $F$", for any $x$, any $F$, any $G$ *such that $F$ and $G$ are logically independent predicates*'. But then, this is not a purely formal judgement of invalidity, since it restricts the values admissible for the predicate variables. Indeed, because of the nature of the restriction here imposed, this statement happens not to be a judgement of invalidity at all. The logical independence of two predicates is simply the invalidity of certain arguments. Consequently this statement, although it looks like a judgement of invalidity, is no such thing: it does not pronounce *any* argument invalid. In fact it is only a metalogical truth. All it says is that certain arguments are invalid *if certain others are.*

It will be obvious that what has just been done for undistributed middle and affirming the consequent, can be done in the same way for all the other so-called formal fallacies. To pick out the 'form' called denying the antecedent, will require two propositional variables; to pick out the 'form' called illicit major will require three predicate variables, and so on. And then it will always be possible to choose values of these variables which yield a valid instance of the form in question. There is, in short, no such thing as a 'formal fallacy', as that phrase is usually understood.

As this fact is extremely obvious, it is to be presumed that all logicians know it. But if you publicize this fact, they regard you as not only a bore but a menace, and, for their own part, they certainly do not publicize it. I have seen a great many logic books, textbooks and other, and I have known of a great many logic courses, but never one which so much as mentioned the fact that all the so-called formal fallacies have valid cases.

In fact that is 'putting it mild'. In every logic textbook that I do know of, and every logic course, the opposite was either stated, or implied, or suggested. And that is *still* putting it mild. Countless thousands of students, over many generations, have in fact taken away from their logic courses *little except* the conviction that affirming the consequent, undistributed middle, etc., are invalid in every case. That, indeed, was the very point on which, it was supposed, their studies had raised them above the vulgar, and had armed them against unscrupulous rhetoricians. Even their teachers, I am ashamed to say, only a few decades ago regularly ridiculed the Un-American Activities Committee of the US Senate precisely on the ground of its supposed addiction to the fallacy of affirming the consequent or undistributed middle.



Entire consistency on the matter, it must be admitted, was hardly ever achieved by anyone: students, teachers, or textbook writers. The same process of affirming the consequent, which in an early chapter of the textbook had been duly exposed as betraying logical ignorance or unscrupulousness, had a habit of turning up again in the last chapter, on scientific method, but this time as nothing less than the logical mainstay of the entire structure of empirical science. But then, no one is perfect.

Are these things of the past? Not in the least. Students of elementary logic, with no exception that I have heard of, are still being taught, or at least encouraged to believe, that every instance of, say, affirming the consequent, is invalid. The terminology is sometimes different, but the substance is the same. For example, students are still being taught, or encouraged to believe, that any argument of that form *can be proved invalid by a truth table.*

I. M. Copi says, in his widely used and justly respected text (Copi, 1954): 'We can establish the invalidity of an argument by using a truth table to show that its form is invalid'. He was thinking, of course, not exactly of 'If $p$ then $q$, $q$, so $p$', say, but of '$p \supset q$, $q$, so $p$'. But that difference makes no difference here, and Copi's unmistakable teaching is, that any instance of the latter form can be proved invalid by a truth table; namely, of course, at that line in the table where $p$ is false and $q$ is true.

The sentence just quoted was from the first edition (1954) of Copi's textbook (p. 60). By the fourth edition of 1973, the sentence is interestingly different, and reads instead: 'We can establish the invalidity of an argument by using a truth table to show that *the specific form of that argument* is invalid' (p. 45, emphasis added). This suggests that, between those editions, someone had invited the author to try his skill, at proving invalidity by truth table, on a case like (c) above, or rather on its counterpart with the hook; and that he had, as a result, realized his mistake. But if he did realize it, he did not succeed in correcting it. Copi's notion of the specific form of an argument turns out to be signally unspecific; for 'the specific form' of any case of affirming the consequent, say, is none other than the familiar '$p \supset q$, $q$, so $p$'. And in the fourth edition, as in the first, the student is told that all he need do, to prove the invalidity of any argument of this form, is to prove the invalidity of this 'specific form'. So, although Copi must know perfectly well, for example, that the truth-functional counterpart of (c) above cannot be proved invalid by a truth table, or by anything else, for the simple reason that it is valid, he just cannot help himself: he must and will go on teaching students that it *can* be. In this way, and under a new terminology, the old illusion that there are invalid forms of argument is imparted to ever-new generations of students, apparently to be transmitted to the remotest posterity.

Perhaps I will be told that it is unfair to judge formal logic by its textbooks, even superior ones. It is not unfair, but I will let that pass. For the idea that every instance of an 'invalid form' is invalid has far better authority in its favour than textbooks of logic. It has, by implication at least, the unanimous endorsement of philosophers, in virtue of something that they do every day.

Philosophers proceed, and not just when they are teaching elementary logic, as though invalidity can be proved by means of a 'parallel argument': that is, as though it is enough to prove the invalidity of a given argument, if one can mention a second argument, which has the same logical form as the given one, and of which all the premisses are true and the conclusion false. Thus for example, the argument

All swans are black
Abe is black
Abe is a swan,

is supposed to be proved invalid by citing, for example, the argument

All persons now in this room are under 200 years old
Mr Hawke is under 200 years old
Mr Hawke is a person now in this room.

If there has been, in the entire history of the world, so much as one word published in criticism of this so-called method of proving invalidity, I have not had the good fortune to meet with it, though not for want of trying. On the contrary, this 'method' is constantly endorsed, as I have implied, by the practice of even good philosophers; and some philosophers of reputation (Popper for one)[1] have expressly endorsed it in print.

[1] See Popper (1968), p. 297.



It is illusory none the less; and in fact, of course, is essentially the same illusion as we have already met with in two other forms. The second argument's having true premisses and false conclusion proves, indeed, that *it* is invalid. But the invalidity of the given argument follows, from the invalidity of the second one, only if that is conjoined with the assumption that any argument which shares its logical form with an invalid argument is invalid itself. But that is precisely the assumption that all instances of a form which has invalid instances are invalid: the assumption which (b) and (c) suffice to show is false.

The mistakes I have been talking about, or rather the different forms of a single mistake, are made possible by not attending to arguments like (a), (b), and (c). (Or, of course, by not attending to the counterparts of (b) and (c) which contain '$\supset$': again, that difference makes no difference here.) Accordingly, when arguments like these *are* pressed on the attention of the formal logicians, their reaction is unfailingly instructive. This reaction is one of uneasy disapproval. And both the disapproval, and the unease, are very understandable.

The formal logician is profoundly reluctant to call argument (a), for example, valid. To call an argument valid is to attribute to it the highest possible degree of logical value. Now, it is the fundamental article of the formal logician's creed that logical value in general, or at least validity in particular, is *essentially formal*. 'Logical *form*' is the subject of all his professional care and study, as well as the source of his livelihood. But (a) is an argument whose logical value owes nothing, obviously, to its logical form, while it is, equally obviously, an argument of the highest possible logical value. For a formal logician to be asked to call (a) valid, therefore, is a torment to him, even though he knows it is so: it is asking him to discard the fundamental article of his professional creed. And arguments (b) and (c), of course, offer an even more gratuitous affront to that creed than (a) does.

But, alas, the formal logician is even more profoundly reluctant to call (a) invalid; indeed, he dares not call it so. For (a) can, of course, be turned into an argument which he *does* call valid, by the mere addition of a necessarily true premiss, 'All fathers are male parents', or 'If Hume is a father then Hume is a male parent'; while the formal logician holds—at least the vast majority of them do, and for very good reasons—that an argument which is valid with a necessary true premiss is valid without it. The same goes, of course, for arguments (b) and (c) as well.

Thus, the formal logician cannot call (a), (b), or (c) valid, consistently with his professional creed: hence his disapproval of them. But he dares not call them invalid either: hence his unease.

A situation so painful as this one is bound to produce distress signals, even if only half-conscious ones. Some of the commonest of these signals sound as follows. 'Argument (c) is *invalid in propositional logic*'; '(b) is *not valid in predicate calculus*'; '(a) is *neither quantificationally valid nor truth-functionally valid*'. You can easily see how suitable such phraseology is to the distressed logician's situation. A phrase like 'invalid in propositional logic', for example, by including the word 'invalid', has the effect of setting the desired *tone*, the tone of disapproval; while at the same time it is admirably non-committal, because after all—as the formal logician himself will hasten to assure you—'invalid in propositional logic' no more entails 'invalid', than (say) 'suspected murderer' entails 'murderer'.

Still, these phrases betray their painful birth, by being nonsensical. You might as well say of an argument that it is invalid in the spring, or in the south, as say of it that it is invalid 'in' predicate logic, or whatever. Arguments are not 'in' predicate logic, or 'in' any other artefact that logicians may happen to make. Still less is their invalidity or validity 'in' anything at all, except the arguments themselves.

Such phraseology is, of course, of extremely recent origin, and I think I understand what the linguistic route was which made it available to logicians in recent times. I take it that 'not valid in propositional logic', say, is actually a contraction of 'not able to be *proved* valid by the axioms of rules of any system of propositional logic'. Similarly, I suppose, 'not valid in predicate calculus', is a contraction of 'not able to be proved valid by the axioms or rules of any system of predicate logic'.

It is perfectly true, of course, that the arguments (a), (b), (c) are not able to be proved valid by any logical system, or at least by any existing one. No person of sense thinks one atom the worse of these arguments on that account, of course, or would consider them any the better, if a system were devised which *did* enable them to be proved valid. But if my historical suggestion is right, then the phraseology I am speaking of, as well as being nonsensical, is evidence of a tendency among logicians which is deplorable, and of which there is only too much other evidence: I mean, the tendency to identify an argument's *being* valid, with its being able to be *proved* valid by means of some system or other which logicians have devised.



It would not be surprising if logicians were tempted to make this identification, magnifying greatly as it does the importance of their profession. But the identification is ridiculous all the same. The valid arguments would be valid, and the invalid ones would be invalid, even if it were never possible to prove any of them so, and if there never had been, and never was to be, such a thing as a system of logic, or a logician, in the world. Nothing could be more obvious than that.

Formal logic aims at *high* generality: at a degree of generality so high, at the least, as to forbid the employment, in order to pick out a class of arguments, of any propositional constant, or predicate constant, or individual constant. Generality of this high degree is present in the judgement that all cases of undistributed middle, or of affirming the consequent, are invalid. But these judgements of invalidity, we have seen, are false. So are all the others which correspond to the other supposed formal fallacies.

Of course this does not *prove* that all judgements of invalidity, possessing this degree of generality, are false. Still, it is a good reason to believe that; and that is what I believe. That is, that all purely formal judgements of invalidity, which employ *no* propositional constant, predicate constant, or individual constant to pick out the class of arguments in question, are false.

But my thesis is rather stronger than that. It is that all purely formal judgements of invalidity which are not of *low* generality are false.

To make this thesis as testable as one would wish it to be, I should, of course, state exactly *how* low is 'low': exactly what, according to me, that level of generality is, above which purely formal judgements of invalidity are *without exception* false. But this is a point which I have not been able to settle to my own satisfaction.

Fortunately, however, my thesis is perfectly testable as it is. For, in the field of logic, philosophers and logicians possess a large fund of strong and virtually unanimous intuitions about generality. There is very little danger, consequently, of any test of my thesis petering out in disagreements as to whether a given judgement of invalidity is, or is not, of low generality.

## (iii)

I turn now to purely formal judgements of validity.

As in the case of invalidity, there are plenty of these which are true, but of 'villainous low' generality. '"All swans are black and $x$ is a swan" entails "$x$ is black", for all $x$'; and so on.

But above that level, and in particular, again, once we allow predicate variables or propositional variables into our purely formal judgements of validity, falsity very soon sets in, just as it does with our judgements of invalidity. Purely formal judgements of validity, if they employ a predicate variable or a propositional variable to pick out the class of arguments in question, are all or almost all exposed to counter-examples, direct or indirect, or they generate paradoxes.

The class of invalid argument is so heterogeneous, and so vast—since after all, $p$ does not entail $q$, for almost any $p$ and almost any $q$—that no one should ever expect to be able to reduce all of *it* to a few simple types. But it seems, at least, to be different with the class of valid arguments. There, it does seem possible to bring nearly all cases under a few simple, very general, and even purely formal types. It seems possible, because it seems as though logicians have in fact done it.

But I think that this is only the official and daylight face, as it were, of logic. It is a different story outside, at night, and especially in the oral tradition. Why, the very same man who in print appeals, with apparently the most perfect confidence, to (say) hypothetical syllogism as a valid form, will in conversation with you cheerfully allow himself to cast the most scandalous aspersions on it. In fact there is nowadays scarcely a philosopher who cannot show you, in private and between consenting logic teachers, a collection of logical dirty pictures. Perverse counter-examples, paradoxes at once disgusting and tedious, dog our footsteps whenever we attempt to frame purely formal judgements of validity which are of high generality while being true. With whatever eugenic care we select the parents, monstrous offspring sooner or later ensue. The elephant man is not in it by



comparison: this is a whole museum of pathology, or of pornology as you might say.

My own collection of these things is not a very large one. (A very large collection of them is a rather bad sign, humanly speaking; just as is the opposite but of course connected thing, the obsession with maintaining formal purity. Formal logic, Jowett said, is neither a science nor an art but a dodge; I say that, whatever else it is, it is a character defect.) But, strictly in the interests of science, I must now expose a few of my specimens to view.

First, transposition: '"If $p$ then $q$" entails "If not-$q$ then not-$p$", for all $p$, all $q$.' That is a purely formal judgement of validity, and one which has as good a chance of being true as most. But 'If Baby cries then we beat him', does not entail 'If we do not beat Baby then he does not cry'. It may well be doubted whether parental severity is even a biological guarantee of a stoical infant; but a *logical* guarantee it certainly is not. (I owe this example to Mr Vic Dudman, of Macquarie University.)

Second, a syllogistic rule: '"All $F$ are $G$ and $x$ is $F$" entails "$x$ is $G$", for all $x$, all $F$, all $G$.' That is a purely formal judgement of validity with as good a claim on our belief as any. Here I offer, not a counter-example, but a counter-example-or-paradox, the paradox being an obvious relative of the Liar. (I owe this example to a first-year student of many years ago, Mr Peter Kintominas.)

(d) All arguments with true premisses and false conclusion are invalid
(d) is an argument with true premisses and false conclusion
———
(d) is invalid.

If (d) is invalid then our syllogistic rule is false straight off. If (d) is valid, then its conclusion is false, and so one of its premisses must be false. Then the problem is to find the false premiss. The first premiss is true. So is the second part (we are supposing) of the second premiss. The falsity must therefore be in the first part of the second premiss: but where? Indeed, since the conclusion, if false, is necessarily false, and since the first premiss is necessarily true, and since the second part of the second premiss is necessarily true: please find the *necessary* falsity which is asserted by the first part of the second premiss, (the part which says that both premisses are true).

Third, hypothetical syllogism, plus one kind of universal instantiation. (This example is due to Dr Paul Hyland.)

'All men are mortal' entails 'If Socrates is a man then Socrates is mortal'; which in turn entails 'If Socrates is a man then it is not the case that Socrates is an immortal man'. So, entailment being transitive, 'All men are mortal' entails 'If Socrates is a man then it is not the case that Socrates is an immortal man'. Conjoin that conditional with the necessarily true conditional, 'If Socrates is an immoral man then Socrates is a man'. Hypothetical syllogism then gives you, 'If Socrates is an immoral man then it is not the case that Socrates is an immortal man'. Entailment being transitive, and necessarily true premisses being dispensable, we now have that 'All men are mortal' entails 'If Socrates is an immoral man then it is not the case that Socrates is an immortal man'. And so it does: there is nothing untoward so far.

But if hypothetical syllogism and universal instantiation are valid in all cases, it likewise follows from 'All men are mortal' that 'If Socrates is an immortal man then it is not the case that Socrates is an immortal man'. And that is false. 'All men are mortal' is contingent. But 'If Socrates is an immortal man then it is not the case that Socrates is an immortal man', is necessarily false. And a contingent proposition cannot entail a necessary falsity.

This is an instance of what I meant by speaking earlier of *indirect* counter-examples. I call it indirect, because it makes use of metalogical truths: the transitivity of entailment, and the dispensability of necessarily true premisses. But, assuming those standard principles, what the case shows is this: purely formal judgements of validity, concerning hypothetical syllogism and universal instantiation, are inconsistent, not only with a certain true singular judgement of invalidity, but with the general non-formal judgement of invalidity, that contingents never entail necessary falsities. And which side of this inconsistency ought to be given up, is perfectly obvious.

But Hyland's example is more directly instructive as well. 'All men are mortal' really does entail 'If Socrates is an immoral man then it is not the case that Socrates is an immortal man', and really does not entail 'If Socrates is an immortal man then it is not the case that Socrates is an immortal man'. So the difference between a valid argument and an invalid one sometimes depends on the absence at one point of the letter '*t*'. Yet there actually are people who believe in the possibility of *formal* logic: a hopeful undertaking, indeed!



With this I return my specimens to their plain wrapper.

In the validity of those 'valid forms' which logicians have made familiar to us, most of us have at first a degree of confidence which, as it is peculiarly high, is also peculiarly fragile. To preserve that degree of confidence in a purely formal judgement of validity, is impossible for a rational person, once *even a reasonable suspicion* has been raised about it. With purely formal judgements of validity, it is as with female sexual purity under an extreme purist regimen: giving grounds for belief in a lapse, is itself one kind of lapse. Suppose, then, what as far as I can see is impossible: that my three specimens should be able to be somehow reconciled with the purely formal judgements of validity against which I advanced them. Still, as these specimens at least give *some* grounds for believing those judgements to be false, to have the perfect confidence we once had in the general validity of transposition, etc., will be impossible for us, if we are rational, *even now*. And then, it is to be remembered, there are plenty more where my three specimens came from.

The conclusion which I draw from this and the preceding section, even if it is not true, has at least the merit of being a natural one. There are no logical forms, above a low level of generality, of which every instance is invalid: every such supposed form has valid cases. There are few or no logical forms, above a low level of generality, of which every instance is valid: nearly every such supposed form has invalid cases or paradoxical cases. The natural conclusion to draw is that formal logic is a myth, and that over validity, as well as over invalidity, forms do *not* rule: cases do.

## (iv)

I do not know of anyone except myself who believes or ever has believed this philosophy of logic. Nor do I expect it to make quick converts now. It is too complete an inversion of the common opinion. That opinion is, of course, that logic is essentially formal, that logic is nothing if not formal, that the very phrase 'formal logic' is pleonastic, etc. Something of logicians' snobbery contributes to this opinion, of course: think, for example, of the kind of distaste which most logicians feel for courses called 'informal logic'. But it is also a conviction deeply and widely held.

As this is the common opinion, anyone may be excused for thinking at first when he hears it said that formal logic is a myth, that he is hearing an attack on logic itself; some sort of scepticism about logical truth, and hence about the possibility of logical knowledge; some counsel of despair about logic. But this would be not only a mistake, but the very reverse of the truth. My philosophy of logic is so far from being sceptical that it is if anything indecently affirmative. Not only do I believe, as I have implied, that there *are* logical truths, true judgements of validity or of invalidity; I believe that every normal human being is, in the extent of his knowledge of such truths, a millionaire. Only, I hold, as I have implied, that almost every logical truth which anyone knows, or could know, is either not purely formal, or is singular or of low generality.

Of course to the *formalist*, anyone who generalizes less hastily than he does looks like a sceptic, just as the tortoise looks to the hare to be standing still. But we must simply disabuse the formalist of this error. I am perfectly entitled to say, for example, that argument (b) above is valid, but that I will judge the next case of undistributed middle that I meet on its merits. Indeed, since some cases of that form *are* valid, and some are not, it is absurd to say anything less. This is not sceptical refusal to generalize; it is merely respect for very obvious facts. Similarly in general: there is nothing in the least sceptical in saying, about one instance of a certain logical form, that it is valid, and saying, about another instance of it, that it is not. Such sayings jar on formalist ears, of course, but that counts for nothing.

But to satisfy the reader that my view of logic is dogmatic rather than sceptical, let me remind him: I denied that all instances of undistributed middle are invalid because *I claimed to know* of instances of it which are valid. And I denied that hypothetical syllogism and universal instantiation are valid in all cases, because *I claimed to know* that contingents never entail necessary falsities. These are claims to logical knowledge; even, in the second case, a claim to *general* logical knowledge. They may be mistaken claims, of course; but at any rate the making of them is not consistent with despairing of the possibility of logical knowledge.



Of course, I *do* counsel the *formal* logician to despair, since I have given reasons to believe that what he seeks is not to be found. But to despair of the possibility of formal logical knowledge does not at all require that we despair of the possibility of logical knowledge.

The formal logician, though perpetually drowning in an ocean of counter-examples, paradoxes, and the like, equally perpetually lives in hope. He is like Boxer in Orwell's *Animal Farm*, and repeats to himself, 'I must work harder'. 'I *may* finally arrive at purely formal judgements, of validity at least, which are of high generality, and free, now and for evermore, from every suspicion of falsity'.

Why, and so he *may*. Still, this hope of his is almost entirely groundless. Almost nothing in the historical record supports it, and almost everything points in fact the other way. Does anyone suppose that, in logic nowadays, weird counter-examples, paradoxes, etc., are a stationary population, or a species actually in danger of extinction? The fact, of course, is precisely the reverse: anomalies *increase* with increasing formality. This is known as the progress of formal logic, and is reckoned one of the glories of the twentieth century, by contrast with all of the earlier modern period, which logicians love to call 'the dark ages of logic'.[2]

And how, indeed, could things be otherwise? In natural science, our generalizations are mostly of low generality, and even then, before we can find a counter-example to any of them, nature itself must give us its co-operation. There are no such salutary impediments in the case of formal logic. There, the generalizations are of such boundless extent as to afford boundless encouragement to the search for counter-examples; and to supply those, nothing more is required than the energies of men who are clever, leisured, and deeply contra-suggestible. The supply has been more than adequate in the past, as any rational person would expect from so fertile a source; and if you are rational, you will expect the supply to be kept up in future too.

But the aspiration to high generality in logic is not only groundless; it is a colossal nuisance, as causing endless waste of

[2] A. N. Prior; but I have been unable to rediscover where I read these words.

time and effort. For it invests the obstacles it encounters with an importance which is entirely illusory.

If, for example, the Dudman argument about Baby is invalid, as it is, this is a fact which, to put it mildly, is of little interest or importance in itself. It does not even mean, as I have stressed, that even a single other case of transposition is invalid. It is of interest or importance *solely* as refuting the claim of logicians that transposition is a valid form. Similarly with the Kintominas example, and with every other specimen in the museum of pornology. No one would need to devote a moment's thought to such silly things, nobody would care anything at all about them, if they, or things like them, were not needed for the job of knocking formalist hopes on the head. But since in fact those hopes, though groundless, are perpetually renewed, the tiresome but necessary work of extinguishing them is likewise always still to do.

It is the same all through the history of formal logic: with the Liar Paradox, with the class of all classes not members of themselves, etc., etc. What a chronicle of wasted time! What is there, what could there possibly be, in the statement 'This statement is false', to make it worth one-thousandth part of the attention which has been lavished on it during two thousand years? Actual mental disorder apart, nothing, it is evident, could ever have invested such a trifle with importance, and of course nothing did, except its being an obstacle to certain formalist expectations. But it is *those expectations which deserve critical scrutiny*, though they almost never get it—not the poor uninteresting Liar, which has had so much of it.

In logic what is needed is to mortify, not to inflame, the passion for high generality. It would help to this end if we sometimes asked ourselves the common-sense question, 'What do we want generality *for*?' Where natural science is concerned, this question is easily answered. We want generality because it is needed for prediction, for control, and for explanation, and not for any reason independent of these three. But none of these reasons is available, where it is logic that is in question. To talk of prediction or of control in connection with logic would be a poor joke enough. And whether or not explanation is as important even in natural science as many philosophers now suppose, it is not even a joke in logic. There are *no* explanations in logic.



Indeed, I do not think that anyone has ever claimed, for logic, that it *does* explain anything. There is, of course, a faint insinuation of some such claim, in certain neologistic phrases like 'quantification *theory*'. But such phrases are a mere abuse of the word 'theory' (and, I may add, a self-serving abuse, it being supposed nowadays to be *a good thing* to have a theory). For a theory is, whatever else it is, something true or false, something which someone might believe or disbelieve; but what logicians dignify with the name of 'quantification theory' is nothing of that sort. In any case, there are certainly no explanations in logic, in *my* sense of 'logic': one true judgement of validity or of invalidity can, of course, *entail* another and less general one, but even then it never *explains* it. The greatest logician in the world cannot explain, any more than the layman can, why 'All swans are black and Abe is a swan' entails 'Abe is black'.

As it is scarcely possible, after 2500 years of hoping and searching, to point to a single purely formal judgement of validity which is not false or paradoxical, it can hardly be said that formal logic has been rich in positive results. It must be confessed, on the other hand, that it has been singularly fruitful of other results, especially in the present century. The foundations of logic itself, and with them the foundations both of mathematical and of natural science, thrown into complete and irreversible confusion: these are not contemptible consequences of the search for high generality and pure formality in logic. We didn't get where we are today by adopting an unambitious, piecemeal approach to logic, no sir! And as for the stone age philosophy of 'cases rule': why, if we had been content to settle for that, we would probably still be judging each argument on its merits to this day.

## (v)

Deductive logic, then, is not purely formal. But what has this got to do with the defence of the arguments in Chapters V–VII above?

The summary answer is this. Non-deductive logic in any form, and hence the theory of logical probability, is now thought by many philosophers to have been discredited once and for all by the problem about 'grue', first brought to light by Nelson

Goodman in 1954.[3] A reader of this book is therefore likely to think that the arguments of Chapters V–VII are (somehow) fatally exposed to the 'grue' problem. None of this is true. The importance of 'grue' has been greatly exaggerated. In fact, 'grue' is fatal only to the belief that non-deductive logic is *purely formal.* But that is not something which the premisses of my arguments in Chapters V–VII commit me to, or even something which influenced me in framing those arguments. And even those philosophers who *did* believe that non-deductive logic is purely formal, did so only because they believed that *deductive* logic is purely formal. Once *that* illusion is dispelled, therefore, 'grue' is not a fatal problem to anyone.

But this is far too summary. We need to consider just what the case of 'grue' shows, and what it does not. To do this, we need first to go back a little in history.

That all inductive inferences are fallible—that is to say, invalid, 'non-deductive'—is a truth which philosophers have been remarkably slow to admit. Hume laboured long and hard to bring it home to their minds, but in vain, until, long afterwards, Einstein came to his assistance, and 'history teaching by example' made it a truth impossible to resist any longer.[4] *Then,* indeed, philosophers made up for lost time, and soon so changed the meaning of the word 'inductive' as to make it *analytic* that inductive inference is fallible.[5] But never mind: one way or another, by the mid-twentieth century at the latest, the fallibility of induction was a truth fully absorbed by philosophers.

Once that happens, however, a philosopher faces a stern dilemma. He must either embrace inductive scepticism, or abandon deductivism. He must, that is, either affirm that a proposition about the observed is never a reason to believe a proposition about the unobserved; or he must admit that one proposition can be a reason to believe another, without the inference from the one to the other being valid ('deductive').

As the former alternative is scarcely compatible with sanity, most philosophers have sensibly preferred the latter. That is, they have abandoned deductivism, even if they have done so, in most cases, neither very consciously nor very enthusiastically.

[3] Goodman (1954), pp. 74–5.
[4] Stove (1973), pp. 98–104.
[5] See ch. VII (v), and Stove (1973), pp. 22–3 and 107–10.



But to abandon deductivism is to acknowledge the existence of non-deductive logic.

Logic, however, had always been supposed to be essentially formal; and, before this time, it had never been necessary to acknowledge the existence of non-deductive as well as of deductive logic. That having now become necessary, it was the most natural thing in the world for philosophers to assume that non-deductive logic, too, is essentially formal. And assume this they did.

This assumption was, I think, shared for several decades by *all* philosophers who had abandoned deductivism and thereby admitted the existence of non-deductive logic. But the assumption was most influential, naturally, in the work of those philosophers who set out to treat non-deductive logic *systematically*; most notably, therefore, in the work of Hempel and Carnap.

Hempel is perfectly explicit. He tells us that he aims 'to set up purely formal criteria of confirmation in a manner similar to that in which deductive logic provides purely formal criteria for the validity of deductive inference'.[6] He even entitled his basic contribution to non-deductive logic 'A Purely Syntactical Definition of Confirmation'.[7] Not everything that Hempel meant by calling his theory of confirmation 'purely syntactical' is relevant here. But part of what he meant, that is relevant here, and a respect in which Carnap's theory of logical probability (or 'degree of confirmation', as he called it) is also purely syntactical, can be explained as follows.

Deductive logic, when it is thought of in the usual formalist way, does not itself *contain* any of those propositions which, at the start of this chapter, I called judgements of validity. Such propositions are, indeed, the very *raison d'être* of deductive logic; but they are not delivered directly, so to speak, by deductive logic. Neither present-day 'quantification theory', nor traditional 'syllogistic', is supposed to have, as part of its content, the proposition, for example, that '$(x)$ (Man $x \supset$ Mortal $x$) and Socrates is a man' entails 'Socrates is mortal'. What deductive logic does deliver directly are only certain *schemas* or *forms* for judgements of validity: for example, that '$(x)$ $(Fx \supset Gx)$ and $x$ is $F$' entails '$x$

[6] Hempel (1965), p. 10.
[7] Hempel (1943).

is *G*'. Of course this statement, and every other like it, is strictly false, or rather senseless, because of its dummy constants: '$(x)(Fx \supset Gx)$ and $x$ is *F*' is not a kind of thing, obviously, which *could* really entail anything. Still, everyone knows what is meant: namely, the kind of proposition discussed in section (iii) above, with '*F*', '*x*', etc. as *variables,* bound by universal quantifiers. And a judgement of validity, for example about the above argument concerning Socrates' humanity etc., is reached, by simply substituting individual constants and predicate constants for the variables or dummy constants of this form or schema. The freedom to make such substitutions is supposed to be unrestricted, though falsely supposed if section (iii) above is right; and it is in this that the *formal* character of deductive logic consists.

Non-deductive logic, then, set out to be formal in the same way. 'The criteria of confirmation' Hempel writes, 'should contain no reference to the specific subject matter of the hypothesis or of the evidence in question.'[8] Accordingly, Hempel's theory of confirmation does not itself contain any concrete 'judgements of confirmation', as I will call them: propositions such as '"Abe is black" confirms "$(x)$ Black $x$"'. Such propositions are, indeed, the *raison d'être* of the theory; but they are not delivered directly by the theory. The theory directly delivers only schemas or forms for judgements of confirmation: for example, '"$x$ is *F*" confirms "$(x)$ $Fx$"'. An actual judgement of confirmation, such as the one just mentioned about Abe, is reached by simply substituting individual constants and predicate constants for the variables or dummy constants of the form or schema. And the analogy with deductive logic, which was guiding Hempel's enterprise, required that the freedom to make such substitutions be unrestricted here too. In this respect, Carnap's theory of logical probability is likewise purely formal. What Carnap calls 'the sentences of the languages L' are, of course, not sentences at all in fact: only sentence-schemas. His statements of logical probability, which mention two such sentence-schemas, therefore inherit this schematic character themselves. And no restriction is placed on the individual constants or predicate constants which may be substituted for the variables or dummy constants in the sentence-schemas.

8 Hempel (1965), p. 10.



Non-deductive logic, then, as Hempel and Carnap conceived it, was purely formal. In particular, it placed no restriction on the predicates substitutable into a schema for judgements of confirmation or for statements of logical probability.

Yet one has only to say this, to realize at once that these writers did not at all consistently adhere to their conception of non-deductive logic as purely formal. This is especially obvious in the case of Hempel. For his theory *does* in fact, and expressly, place restrictions on the predicates substitutable into confirmation-schemas: they must, among other things, be *observational.*[9] Nor, of course, was this an accident. Hempel was a logical positivist, and regarded confirmability as a peculiarity which *distinguishes* empirical hypotheses both from the propositions of mathematics and logic, and from the 'pseudo-propositions' of metaphysics.[10]

An insoluble problem was posed for Hempel, therefore, by a judgement of confirmation such as '"The number three is prime" confirms "All numbers are prime"', or '"Socrates is predestined by God to eternal torment" confirms "Everyone is predestined by God to eternal torment."' The corresponding judgements of initial favourable relevance pose a similar insoluble problem for Carnap. As consistent logical positivists, Hempel and Carnap must have called these propositions false. As *formal* non-deductive logicians they must, if consistent, have called them true.[11]

Still, if Hempel and Carnap, under pressure from their other philosophical commitments, could not consistently adhere to the belief that non-deductive logic is purely formal, that is no more than an historical accident. Perhaps some one else could. But that is precisely the hope which 'grue', when it came along, extinguished.

It is obviously true that 'All the emeralds observed before AD 2000 are green' confirms 'Any emerald observed after AD 2000 will be green'. A systematic theory of confirmation, if it is any good, will deliver this judgement of confirmation, among many others. If 'green', at its last occurrence here, is replaced by 'blue', an obviously false judgement of confirmation results: a judge-

9 See Ibid., (1965) pp. 22, third paragraph, and Hempel (1943), p. 22.
10 Hempel (1965), p. 3.
11 Cf. Stove (1966).

ment which no systematic theory of confirmation, that was any good, would deliver.

But now, as we have seen, a theory of confirmation, if as well as being systematic it is purely formal, will deliver the above true judgement of confirmation only *indirectly*: only, that is, as an instance of some *schema* for judgements of confirmation. As an instance, for example, of the schema: '"All the *F* observed before *t* are *G*" confirms "Any *F* observed after *t* is *G*"'. And Goodman's objection is, that there are predicate constants which, substituted into this or any similar schema, yield *false* judgements of confirmation.

> Now let me introduce another predicate less familiar than 'green'. It is the predicate 'grue' and it applies to all things examined before *t* just in case they are green but to other things just in case they are blue. Then at time *t* we have, for each evidence statement asserting that a given emerald is green, a parallel evidence statement asserting that that emerald is grue. And the statements that emerald *a* is grue, that emerald *b* is grue, and so on, will each confirm the general hypothesis that all emeralds are grue. Thus according to our definition, the prediction that all emeralds subsequently examined will be green and the prediction that all will be grue are alike confirmed by evidence statements describing the same observations. But if an emerald subsequently examined is grue, it is blue and hence not green. Thus although we are well aware which of the two incompatible predictions is genuinely confirmed, they are equally well confirmed according to our present definition.[12]

In other words:

(142) 'All the emeralds observed before AD 2000 are grue' confirms 'Any emerald observed after AD 2000 is grue',

is a false judgement of confirmation. For, given the definition of 'grue', it entails the obviously false:

(143) 'All the emeralds observed before AD 2000 are green' confirms 'Any emerald observed after AD 2000 is blue'.

Yet (142) is an instance of the schema:

(144) 'All the *F* observed before *t* are *G*' confirms 'Any *F* observed after *t* is *G*'.

[12] Goodman (1954), pp. 74–5.



And (144) is a schema which, in virtue of its many true instances, and in every other respect, has as good a claim on our acceptance as any schema for judgements of confirmation.

Assuming that, given the definition of 'grue', (142) really does entail (143), then the case of 'grue' shows what Goodman claimed it shows: that 'confirmation . . . depends rather heavily upon features . . . other than . . . syntactical form'.[13] It therefore shows that non-deductive logic is not purely formal. For, just as the cases mentioned in sections (ii) and (iii) above showed that deductive logic is not purely formal, by showing that typical purely formal judgements of validity or of invalidity have false instances as well as true ones; so the case of 'grue' shows that a typical purely formal judgement of confirmation, such as (144), has false instances as well as true ones.

We saw in section (iv) above, that the admission that deductive logic is not purely formal, is not at all sceptical: it is no bar whatever to the possibility of deductive logical truth, or of knowledge of such truth. Similarly, the admission that non-deductive logic is not purely formal, is not sceptical: it is no bar whatever to the possibility of non-deductive logical truth, or of knowledge of such truth. On the contrary, and just as in the deductive case: it is precisely our claims to know non-deductive logical truths, such as that (142) is *false*, and that some other instances of (144) are *true*, which compel us to say that non-deductive logic is not purely formal. Yet scepticism about non-deductive logic has in fact been greatly encouraged by the case of 'grue'. Indeed, 'grue' has been thought to prove all sorts of things which it does not prove at all.

It is widely supposed, for example, that the 'grue' case is an objection or counter-example *to Hempel's definition of confirmation itself*; or in other words that the outrageous (143), or what is supposed to entail it, (142), or at the very least (144), is a consequence rigorously derivable from Hempel's definition. Several things Goodman says, including the last five words of the quotation above,[14] suggest that he believed this. Hempel himself has never, to my knowledge, denied that (143) is a consequence of his definition; which has helped to spread the belief that it is. But it is not. Not that Hempel saw 'grue' coming, and provided

[13] Ibid., pp. 73.
[14] Ibid., pp. 73, 75, and 76.

against it: it is well known that he did not. But as it happens, various provisions, inserted in his definition for other reasons, make it impossible for (143), or (142), or even (144), to be rigorously derived from that definition.

I will not attempt to prove this. (Strictly, indeed, it is impossible to prove such a thing.) Even to state in detail my reasons for believing it, would take us much too far out of our way. But anyone who supposes that (143), (142), or even (144), *can* be rigorously derived from Hempel's definition, should try to derive it rigorously. He will find the experiment instructive.

If Goodman did think that his case directly hit Hempel's definition, then he was still wider of the mark in saying (in the passage quoted above) that, given green emeralds before the year 2000, the 'green' prediction and the 'grue' or the 'blue' prediction, 'are *equally well* confirmed *according to our present definition*'. Hempel's definition cannot possibly have any consequences about two hypotheses being *equally well* confirmed. As is well known, his definition was just of the classificatory concept, 'confirms', and says nothing whatever about the comparative concept, 'confirms . . . as well as . . .'. Indeed, whether or not it was Hempel's definition that Goodman meant when he refers, as he constantly does, to 'our definition', he was at fault here. For he evidently drew his conclusion about *equal* confirmation, from premisses which are judgements of *confirmation*, and nothing more.

It is widely supposed, similarly, that the case of 'grue' is an objection or counter-example to *Carnap's theory of logical probability.* What corresponds there to (142)–(144) are the following judgements of favourable relevance:

(142′) 'All the emeralds observed before 2000 are grue' is initially favourably relevant to 'Any emerald observed after 2000 is grue';

(143′) 'All the emeralds observed before 2000 are green' is initially favourably relevant to 'Any emerald observed after 2000 is blue';

(144′) 'All the *F* observed before *t* are *G*' is initially favourably relevant to 'Any *F* observed after *t* is *G*'.

The supposition is, therefore, that (143′), or (142′), or at least (144′), is a consequence rigorously derivable from Carnap's



theory of logical probability. But it is not so. Again, it is out of the question to do more here than to *state* this fact, and invite anyone who thinks otherwise to try to produce such a rigorous derivation.

It is not true, then, that either Hempel's theory of confirmation, or Carnap's theory of logical probability, is itself hit by the problem of 'grue'. What grue *does* hit is a belief without which those theories would never have been constructed: that non-deductive logic is purely formal. But that is not at all the same thing.

If even the systematic constructions of Carnap and Hempel are not refuted by the case of 'grue', then that case cannot possibly hit the few tiny fragments of non-deductive logic which I used as the premisses of my arguments in Chapters V–VII. Still, those arguments will be thought by many philosophers to be exposed to the following objection, which is an adaptation of the 'grue' case. It has been made to the arguments of Williams (1947) and Stove (1973), out of which the arguments of Chapters V–VII have grown.

> The difficulty is that an argument from logical probability is of a purely formal sort. As a result, it cannot differentiate between more and less 'natural' classes. The fact that all hitherto observed emeralds are green might be taken to bestow a probability upon the hypothesis that all other emeralds are also green. But what of the hypothesis that all emeralds are green up to AD 2000 but blue thereafter? That is to say, what of the hypothesis that emeralds are grue? If the evidence we have now (before AD 2000) bestows a certain logical probability on the hypothesis that all emeralds are green, why will it not bestow the same logical probability upon the hypothesis that all emeralds are grue?[15]

It is not obvious, and Armstrong says nothing to explain, what he means by calling the arguments of Williams (1947) or Stove (1973), 'purely formal'. Nor is it easy to be sure what his objection is supposed to prove. If it is supposed to be a proof of the invalidity of Williams's argument, or of mine, it can safely be rejected out of hand. For on that supposition it would be a 'proof of invalidity by a parallel argument'; and we saw in section (ii) above that there is no such thing. But I take it that the objection is supposed to be a proof, not of the invalidity, but of the 'non-proof-hood' of

[15] Armstrong (1983), pp. 57–8.

Williams's (1947) and Stove's (1973) argument. And I think that, by calling those arguments 'purely formal', Armstrong meant at least this: that those arguments have predicate variables or dummy constants in them, and that, as a result, a predicate like 'grue', as well as 'decent' predicates like 'black', or 'Australian swan', can be substituted into them.

When, making those assumptions, I try to re-formulate Armstrong's objection to my own satisfaction, the best I can do is the following. 'To "prove" in the way Stove (1973) did, or Williams (1947) did, that (for example) "All the many observed Australian swans have been black" is initially favourably relevant to "All Australian swans are black", cannot really be a proof of that proposition. For at that point in such a "proof" at which "black" and "Australian swans" were substituted for certain predicate variables or dummy constants, we were equally entitled to substitute instead "grue" and "emeralds". And then the argument would be a "proof" of the false proposition, that "All the emeralds observed before 2000 are green" is initially favourably relevant to "Any emerald observed after 2000 is blue".'

It is not easy to be sure that the objection, even in this version, is free from the reproach of trying to prove invalidity by a parallel argument. But I believe that it is, and that it is in fact a just objection to my (1973) arguments and those of Williams (1947). Or rather, I believe it is, *if (142′) does entail (143′)*; (and as to that, see below). Williams's version of his argument was certainly of the most unqualified, and quite unnecessary, generality. He was always enunciating his 'law of large numbers' for *any* attribute, any population, any large sample.[16] With 'grue' behind us, we can easily see that there was a standing invitation, in this generality, to an adaptation of the 'grue' case to his argument. (And Williams in fact acknowledged, in a letter to the present writer, that his argument needed 'some repair to cope with [grue]'.) Similarly, I believe, my argument (1973), by a piece of carelessness far less historically excusable than Williams's, is exposed to the adaptation of the 'grue' case formulated above.

*But the arguments of Chapter V–VII are not.* There is no way that anyone will ever be able to substitute 'grue' for any predicate variable or dummy constant in any of those arguments. The

[16] See Ibid., Williams (1947), ch. 4 *passim*.



simple reason is: *there are no predicate variables or dummy predicate constants in them.* Those arguments are, as the reader can easily check, from start to finish about the logical probability of certain concrete arguments. They are *never* about any argument-form or argument-schema which contains a predicate variable or a dummy predicate constant.

I adopted this course partly, of course, because I wished to prevent any possible adaptation of the 'grue' case to my arguments; but equally because I did not *need* to adopt any other. My purpose in Chapters V–VII was purely polemical: simply to prove the falsity of the sceptical thesis about induction. That thesis is a *universal* proposition. All I needed to do, therefore, was to prove a judgement of initial favourable relevance about, or an ascription of high logical probability to, *one concrete* inductive inference. That I could do. And prudence enjoined that I attempt to do no more.

The case would have been quite different, if I had undertaken some *systematic* work on logical probability; not a purely formal theory (for that is out of the question), but something of high generality: something, say, which would deliver numerical values of 'P($q/p$)' for a wide range of values of the two propositional variables. After Goodman, anyone who undertakes such work as that will indeed need to impose restrictions on the range of his predicate variables, so as to exclude 'grue' if he wishes to admit 'green' and 'blue'. (At least, he will, if (142′) does entail (143′).) But I was attempting no such thing, and I had no such need. All I did was to assert a few statements of logical probability, almost every one of which was free from all generality; and the few which do have some generality (such as (83)) still have none whatever in the predicate dimension. As a result, there is no predicate hole of any kind in the arguments of Chapter V–VII. So there is no hole into which anyone can plug 'grue'.

It is true, as I know from *expériences nombreuses et funestes,* that you cannot make the simplest and most specific assessment of logical probability, without some people supposing that you are thereby committed to so-and-so's *system* of logical probability, with all the attendant difficulties, however peculiar to it. You need only say that 'Abe is black' has probability 0·9 in relation to 'Abe is a raven and just 90 percent of ravens are black', and some philosophers will at once start talking to you about . . . *Carnap!*

About Carnap and 'the zero-probability of laws'; Carnap and 'grue'; Carnap and 'c-star' *versus* 'c-dagger'; and so on, and on. But this is no less ridiculous than it is vexatious. You might as well suppose that a man cannot say that 'All ravens are black and Abe is a raven' entails 'Abe is black', without his being thereby obliged to defend Aristotelian logic, or the system of *Principia Mathematica*, or Quine. This nuisance is, of course, another facet of something noticed in section (ii) above: the tendency to mistake system-makers, mere organizers of logical knowledge, for *sole proprietors* of it.

What, then, does 'grue' matter to the arguments of Chapters V–VII above? Nothing at all. Suppose, with Goodman, that (142) and (142′) do respectively entail the obviously false (143) and (143′), and hence are false themselves; or, suppose with Goodman, that 'All the emeralds observed before 2000 are grue' is *not* a reason to believe 'Any emerald observed after 2000 is grue.' What does that prove? Only that some inductive inferences are not rational, or, in Goodman's terminology, that some empirical predicates are not projectible. Well, what is that to my arguments? I only contended, in Chapters V–VII, against the inductive sceptic, and therefore contended only for the thesis that some inductive inferences *are* rational, or that some empirical predicates *are* projectible.

That some are *not*, we knew, in any case, long before. *Everyone* knows, as I said at the start of this book, that

(2) All the many ravens observed so far have been black, while it is reason to believe

(3) All ravens are black,

is not a reason to believe

(8) All ravens are observed.

Or again, everyone knows, without needing Goodman to tell them, that 'All the emeralds observed before 2000 are emeralds which are green and which exist before 2000', though it is a reason to believe 'All emeralds are green', is not a reason to believe 'All emeralds are emeralds which exist before 2000'. In short, no one should have had to wait for 'grue', to learn that non-deductive logic is not purely formal. And the arguments of Chapters V–VII, in particular, have nothing more to fear from the case of 'grue', than from the two cases just mentioned.



But 'grue' should not matter much even to *systematic* writers on non-deductive logic. It shows that they need to impose, on the range of their predicate variables, one more restriction than they had previously been aware of needing. But that is all it shows.

As to the would-be-*formal* non-deductive logicians, 'grue' shows, indeed, that *they* cannot have what they want. But why was *that* ever supposed, even by those philosophers themselves, to matter much? Well, because of the belief, of course, which they shared with all other philosophers, that *deductive* logic *is* purely formal. But that belief is not only false; it is one which, as there is nothing in sections (ii)–(iii) above which is not well known to philosophers, no one should ever have held in the first place.

But I think that the importance which was at first, and still is, attached to 'grue', has even less foundation than I have so far suggested. For I think that, given the definition of 'grue', (142) does *not* entail (143), and that (142′) does not entail (143′). I do not mean only that (142) does not entail (143) *given Hempel's theory of confirmation*, or only that (142′) does not entail (143′) given *Carnap's theory of logical probability*. I mean that (143) cannot be rigorously derived from (142), or (143′) from (142′), *via any* meta-principles of confirmation, or *via* any principles of logical probability, not able to be shown on other grounds to be false. The kind of things that would be needed to effect such a derivation are, of course, true propositions of the kind which Hempel sought under the names of 'equivalence conditions' on confirmation, 'consequence condition', and the like; or true principles of logical probability, and especially principles of relevance. But I do not believe that there are any such principles as will permit the derivation of (143) from (142), or of (143′) from (142′). In other words, I do not think that, even given the definition of 'grue', it is possible, without drawing on false premises, to get the 'green confirms blue' result from the supposition that 'grue confirms grue', or to get 'green favourably relevant to blue' from 'grue favourably relevant to grue'.

I cannot prove that this is so. I believe it, because I have never been able, despite many attempts, either to produce these rigorous derivations myself, or to meet with them in other writers.

Goodman's own 'derivation', quoted earlier in this section, is a model of non-rigour: a fact of which I have furnished some evidence above, and which is well-enough known—again!—in the *oral* tradition. But I do not know of any published discussion of 'grue' which either makes the required derivations rigorous, or says outright that they cannot be made so. There is, therefore, some reason to believe that they cannot be made so.

If (143′) *cannot* be rigorously derived from (142′), then, as I indicated earlier, 'grue' would be no problem even for the arguments of my (1973) or Williams's (1947). Those arguments would still, indeed, be imprudently and unnecessarily *general*, and would contain predicate variables which would allow 'grue' as a value. But there is no harm in licensing 'grue-to-grue' inductive inference, if that does not license 'green-to-blue' inductive inference.

But even if this last 'sceptical doubt' is misplaced, it is, for the reasons given earlier in this section, not easy to account entirely for the prodigious importance which has been attached to 'grue'. The main villain of the piece is undoubtedly the immemorial illusion that logic is nothing if not formal; but I suspect that the spirit of the age has also had a hand in the affair. If (142) does entail (143), or (142′) does entail (143′), then, as (143) and (143′) are obviously false, (142) and (142′) are false too, and if that is so then Goodman has added a new class of cases to the stock of inductive inferences which are not rational. As we had a sufficient stock of such cases before, this cannot possibly matter very much. But then, the spirit of the age is greedy of anything, however small, which strengthens the sceptical side of the question about induction. Speaking of the philosophical climate of 1947, Donald Williams truly said, what is even truer now, that our philosophy of induction,

> in its dread of superstition and dogmatic reaction, has been orientated purposely toward scepticism; that a conclusion is admired in proportion as it is sceptical; that a jejune argument for scepticism will be admitted where a scrupulous defence of knowledge is derided or ignored; that an affirmative theory is a mere annoyance, to be stamped down as quickly as possible to the normal level of denial and defeat.[17]

[17] Ibid., p. 15.

# X

## IS DEDUCTIVE LOGIC EMPIRICAL?

### (i)

OUR knowledge of logical probability, Carnap said, is intuitive, not empirical.[1] Take regularity, for example, or any special case of it such as (137) above; or a simpler instance of it still, such as

(145) $P(\text{Abe is black}/T) < 1$.

If a person could not simply see this truth for himself, then there would be nothing anyone could do to enable him to learn it. In particular, it is not a truth of a kind which could be learnt from experience. Or again, take the symmetry of individual constants, or any special case of it such as (136) above; or a simpler instance of it still, such as

(146) $P(\text{Abe is black}/T) = P(\text{Bob is black}/T)$.

If some one does not know the truth of this *a priori*, then there is no way in which he could learn its truth *a posteriori*.

Someone who lacked all such 'probabilistic intuitions', or was 'inductively blind', Carnap says, could never learn any 'inductive logic' at all. (He means, of course, '*non-deductively* blind', and '*non-deductive* logic'; obviously, the arguments assessed by (145) or by (146) are not *inductive* ones.) The theory of logical probability, Carnap therefore concludes, rests on intuition.

He uses the word 'intuition' reluctantly, because, as he says, people are apt to think that, if logical probability rests on intuition, it follows that the theory of logical probability is as ill-founded as the claims of gypsies, mystics, and the like. But, Carnap says, this does not follow, and we will see that it does not, once we realize that *deductive* logic, which no one thinks ill-founded, *also rests on intuition*.

[1] What follows is a summary of Carnap (1968).