General Statements as Rules of Inference?

Several philosophers recently have suggested that it is often profitable to regard universal general statements as rules of inference, as material rules rather than formal rules, as P-rules of our language rather than L-rules (to use Carnap's terms). The view is held in different forms. At one extreme there is the comparatively innocuous claim that in some contexts general statements perform a rule analogous to that of deductive rules of inference; at the other, there is the contention that implicit in both everyday language and scientific discourse are P-rules of inference which are in some sense more fundamental than the general statements which can be derived from them.

The considerations which have led philosophers to take up these various connected positions can be listed under four heads. First, there is the fact—noted already by Descartes and Locke—that in everyday discourse we seldom argue in explicit syllogisms. In the same field of ordinary discourse there are the familiar difficulties in interpreting universal statements extensionally. Second, there is the general problem of induction. The old view according to which we need a supreme major premise for all inductive inferences is unattractive, and this is said to be avoided if one treats general laws as principles rather than premises. The third line of argument is the one which has been strongly advocated by Toulmin—that in scientific practice, deductions are not made from laws but in accordance with them. Fourth—although obviously closely related to the first—is the puzzle of counterfactual conditionals: if general statements did not have the force of rules of inference, it would be impossible to derive counterfactual conditionals from them.

It may be interesting to examine the validity of these four lines of thought. As we shall see, there are considerable difficulties that have not been so clearly stated.
"This egg has been boiling for six minutes now, and so it will be hard."

"President Eisenhower is a man; therefore he is mortal."

Traditional logic books treat these as enthymemes of the first order, that is as syllogisms with their major premises suppressed. Critics of traditional logic claim that it is pointless and even misleading to try to squeeze all inference into the strait jacket of the syllogistic forms. Among such critics Mill held that in arriving at the conclusion that Eisenhower is mortal we do not need to presuppose that all men are mortal; all we need to do is argue by analogy with cases such as Socrates and the Duke of Wellington, both of whom have died. Contemporary philosophers such as Ryle and Black, on the other hand, say that we infer directly from the single premise to the conclusion, using a non-logical rule of inference.*

Mill’s view clearly will not do by itself. Similarity is an incomplete predicate and therefore in order to know that Eisenhower is mortal, we have to know in what important respects he is like Socrates and the Duke of Wellington. It is not in being more than five feet long or in weighing more than 140 pounds; it is in being a man, or perhaps an animal. So in a way we do presuppose a major premise. The contemporary view is much more plausible. Certainly one of the main functions of general laws is to enable us to infer from one singular statement to another; and this function is so important that it is tempting to follow Ryle and call general laws inference tickets.

But if we thus abolish the traditional category of enthymemes of the first order, why should we not do the same with enthymemes of the second order? “All cows are ruminants; so Flossie is a ruminant.” “All cows are mammals; therefore, Flossie is a mammal.” Following Ryle and Black we now say that it is wrong to regard these as syllogisms with a suppressed minor premise, “Flossie is a cow.” No, these are direct inferences which have a rule of inference corresponding to “Flossie is a cow.” It might be thought that this is too particular to be a rule of inference. But this is not the case, for we have already seen that there are two different inferences in which it can be used as a governing rule

of inference, and there are obviously infinitely many more. The rule can in fact be put in logical form as $(\phi)[\forall(x)((x \text{ is a cow}) \supset \phi x) \supset \phi \text{ Flossie}]$. A possible objection is that though it is quite common to suppress minor premises and thus to use a special rule of inference, this is not so frequent as the use of a general statement as a principle of inference. This may be true, but the difference is not very considerable. In fact, both general statements and singular statements are sometimes used as conclusions of arguments (inductive or deductive) to ‘state facts,’ sometimes as explicit or implicit steps in an argument. If we wish to say that in the latter use the general statements are principles of inference whenever they are not explicitly stated, then there is no reason why we should not say the same about the singular statements. If, as I would suggest, this seems an excessive multiplication of rules of inference, then the fault lies at the beginning. The traditional account in terms of enthymemes seems preferable to an account that considers only the explicit form of the argument.

This is not the only difficulty of the Ryle-Black approach. No one has shown more clearly than Ryle himself that the vocabulary suitable for talking about rules is different from that suitable for statements. Statements can be true or false, probable or improbable; rules can be correct or incorrect, useful or useless.* Normally we say that some general statements are true, others false, or that some are probably true. If then we wish to say that general statements are rules of inference, it has to be shown that by saying that they are correct rules we are doing much the same as when we said that the corresponding general statements were true. What then does it mean to say that some rules are correct, others incorrect? It cannot mean, as it does with rules of grammar, that correct rules are the ones which are accepted and conformed to by the majority (or by the French Academy). The correctness or incorrectness of these P-rules must somehow depend on empirical facts. The only possible meaning is that a rule is correct if the world is such that by following the rule we arrive at true conclusions.

Thus instead of saying that a general statement is universally true, we say that the corresponding rule of inference always enables us to reach true conclusions from true premises; instead of saying that most $\psi$’s are $\psi$’s, we say that the rule of inference $(\psi)(\forall x \supset \psi x)$ usually enables

* But see the paper by M. Scriven in this volume where he argues that rules can be true or false.
us to reach a true conclusion when we are given a true premise of the
form $\Phi x$. But if this is accepted, it becomes obvious that one of the
advantages we had hoped for has been lost. Certainly some philosophers,
lke Ramsey, have been puzzled by the logic of general statements which
are so obviously not truth-functional and have hoped that the puzzle
might be bypassed by regarding general statements as rules of inference.
But we have only been able to do this at the cost of bringing in other
general statements; for we now have the general statement that in all
cases the use of the rule of inference enables us to arrive at true con-
clusions. So we are no better off.*

Of course, this difficulty might be avoided by holding that, when we
say a rule is correct, all that we mean is that it is a rule observed by
all users of a certain language (in the philosophical sense of language),
and that when we say a rule is probably correct, what we mean is that
it might be useful to adopt this as a rule of our language. This would
be to make such rules meaning rules or what are normally called analytic
truths. This line of thought is the one put forward by Wilfrid Sellars.
It is closely connected with the problem of counterfactual conditionals
and will therefore be discussed in the last part of this paper. At the
moment it is sufficient to notice that it goes considerably beyond the
arguments of writers like Ryle and Black and therefore cannot be used
by them as a way out of this difficulty unless they are willing to change
much of their account. Unless they are prepared to deny that there are
any purely empirical general statements or rules—that is statements
(or rules) which are not true (or correct) ex vi terminorum—then it
must follow that there are some general statements, either in the object
language or the metalanguage, which cannot be interpreted as rules.

II

I have thus been claiming that it is unhelpful and, in fact, mislead-
ing to speak of material rules of inference rather than of implied major
premises. But a strong argument against this position can be drawn
from a discussion of induction. Many classical logicians regarded in-
ductive inferences as concealed deductive arguments with a suppressed
major premise about the uniformity of nature. In this case, however,
such an analysis appears artificial. The only apparent alternative is to

* I owe the substance of this paragraph to a remark made in discussion by Dr.
W. Rozeboom.

regard inductive inferences as arguments governed by a non-deductive
rule of inference—and if we allow a non-deductive rule of inference here,
why not accept them elsewhere? Another advantage claimed by Braith-
waite and Black * for this approach is that it may enable us to show
that induction can be justified inductively without arguing in a circle.
Unfortunately, when one examines this last claim, one realizes that it
raises considerable difficulties which make the principle-of-inference
interpretation of induction as unpalatable as the suppressed-major-premise
view.

In his exposition, which is rather more easily summarized than Braith-
waite’s, Black does not actually subscribe to the view that there is a
single supreme inductive rule or principle. He does however consider
two forms of a rule which is a very strong candidate for such a position.
Rule 1 ($R_1$) is to argue from All examined A’s have been B to All A’s
are B. $R_2$ is to argue from Most A’s examined in a wide variety of con-
ditions have been found to be B to (probably) The next A will be B.
Now it is possible to construct the following self-supporting argument:

(a) All examined instances of the use of $R_1$ in arguments with true
premises have been instances in which $R_1$ has been successful.

Hence, all instances of the use of $R_1$ in arguments with true premises
are instances in which $R_1$ is successful.

This argument uses $R_1$ to reach the conclusion that $R_1$ is always
successful; a similar argument can clearly be constructed for $R_2$. The
obvious criticism is that these arguments are circular. But, Black points
out, this is not quite so obvious as it seems at first. A circular argument,
in the usual sense, is either one in which one of the premises is identical
with the conclusion or else one in which it is impossible to get to
know the truth of one of the premises without first getting to know
the truth of the conclusion. But here there is only one premise, which
is clearly independent of the conclusion, so that the argument is not
circular in the usual way. At this point Braithwaite cites the difference
in a deductive system between a rule of inference and a formula, and
reminds that the rule of detachment may be used in order to derive
the formula $p \cdot (p \supset q) \supset q$.

But in spite of this analogy, one is reluctant to accept this self-sup-

* R. B. Braithwaite, Scientific Explanation, Chapter IX. Cambridge: Cambridge
porting 'justification' of induction. One reason is that if one accepts this, then it seems one would also have to accept arguments like the following:

Rule 4: to argue from The Pope speaking ex cathedra says p to p.  
(a₄) The Pope speaking ex cathedra has stated that whenever the Pope speaks ex cathedra what he says is true.  
Hence whatever the Pope says ex cathedra is true.  
Or Rule 5: to argue from The Bible states that p to p.  
(a₅) In 2 Tim. 3:16 it is stated that all scripture is given by inspiration of God and is profitable for doctrine, hence etc.*

These are clearly circular and yet they seem of exactly the same form as Black's argument.

Wesley Salmon has given an even more convincing reason for rejecting Black's argument as a possible justification of induction.† This is that by citing the same evidence as Black would give in support of R₄ one could produce a self-supporting argument for R₅: to argue from Most examined A's have been B to (probably) The next A will not be B. Salmon thus holds that Black's arguments are in fact circular although not in the usual way. He writes:

For an argument to establish its conclusion, either inductively or deductively, two conditions must be fulfilled. First, the premises must be true, and second, the rules of inference used by the argument must be correct. . . . Unless we are justified in accepting the premises as true and in accepting the rules of inference as correct, the argument is inconclusive. . . . To regard the facts in the premises as evidence for the conclusion is to assume that the rule of inference used in the argument is a correct one. And [in the case of the self-supporting arguments] this is precisely what is to be proved.

But this view has difficulties of its own. One is that it involves us in something very similar to the principle of the uniformity of nature. Before, writers like Mill wanted such a principle as a major premise; now, writers like Salmon want it as a metacomment telling us that the principle of inductive inference is always (or usually) successful. The difference here is minimal.

A second difficulty in Salmon's account lies in its application to deduction. For according to him, in order to reach conclusions deductively, we must have a justification of the rules of inference. Now it is not easy to see that deductive rules can be justified. It is true that Reichenbach purports to give a justification of the rule of detachment in his Elements of Symbolic Logic; * but all that he succeeds in showing is that it is inconsistent to admit that a statement 'If . . . then . . . ' can be represented by p ⊃ q (where '⊃' is defined truth-functionally) and at the same time to assert both the hypothetical statement and its antecedent but deny its consequent. Actually we only can tell that a given statement in words can be represented by p ⊃ q, if we first see that it would be falsified if the antecedent were true and the consequent false. But even if we were to grant the validity of Reichenbach's 'justification,' we should still be left with the paradoxical conclusion that no deductive arguments were conclusive before Reichenbach's time. God has not been so sparing to men as to make them barely two-legged creatures and left it to Reichenbach to enable them to reach conclusions.

We thus seem to have reached an impasse. For the three theories of inductive inference that we have considered are all untenable. Mill's view that all inductive arguments are enthymes, with the suppressed major premise that nature is uniform, seems completely artificial as an analysis of the inference from 'Usually when the wind goes round to the S.W. with a falling glass, rain follows' to 'Probably when the wind next goes round to the S.W., etc.' Black avoids this artificiality by regarding this and similar arguments as valid because of material rules of inference. But this leads to the conclusion that there are a large number of non-circular self-supporting arguments such as our two theological examples—which is absurd. Salmon escapes this absurdity by demanding that the rules of inference be justified. But if this demand is extended to all rules of inference, it makes nonsense of deduction. If it is applied only to non-deductive rules, it completely spoils the analogy that is being drawn between these so-called material rules of inference and the genuine (deductive) rules of inference. The one positive conclusion that one can draw from the statement of this impasse is that here also the introduction of the idea of material rules of inference raises more difficulties than it solves.

However it may be possible to be slightly more constructive and to indicate a way of escape from this puzzling situation. Here it is perhaps illuminating to consider a special use of the concept of necessity. 'All men are mortal, President Eisenhower is a man, so President Eisenhower

* Cf. Westminster Confession of Faith, Chapter 1.
† "Should We Attempt to Justify Induction?" Philosophical Studies, April 1957.

is necessarily mortal’ or ‘he must be mortal.’ Here the word ‘must’ does nothing more than signal the fact that this is the conclusion of a deductive argument. Often we use the same phrasing without ever stating the premises—‘He must be ill’ or ‘It must be going to rain.’ Here the function of ‘must’ is again to indicate that these statements could be exhibited as the conclusions of deductive arguments. If asked why, we would outline the argument by quoting either the major or the minor premise or both: ‘He’s pale and shivering and can’t concentrate,’ ‘Always when the wind goes to the S.W. and the glass is falling, rain follows.’ Thus to say in this sense of ‘must’ ‘A must be B’ is not to say that A and B are connected by some special sort of close tie; it is only to say that A is B, and that a deductive argument which has as a conclusion ‘A is B’ can be constructed with acceptable premises.

I would suggest that ‘probably’ plays a somewhat similar role. When we say ‘It will probably rain’ what we mean is that it is possible to construct an argument of the form ‘Usually when conditions x, y, z hold, rain follows within 24 hours; conditions x, y, z hold now; therefore it will probably rain.’ When we say ‘All observed A’s are B; therefore it is probable that all A’s are B’ we mean that it is possible to construct an argument of the form ‘Usually generalizations made on the basis of evidence of such and such a kind have proved true; our observations of A’s which are B constitute evidence of this kind; therefore it is probable that all A’s are B.’ This is the basic use of ‘probable.’ The frequency concept of probability is a derived use which makes the concept more precise, but at the price of making it inapplicable in those situations where it is impossible to gather statistics.

On this view there is a rule of inference which enables one to move from ‘Most A’s have been B’ to ‘Probably this A is B.’ But it is a rule of inference which is deductive rather than inductive. There is now no problem of justifying the move, any more than there is a problem of justifying the move from ‘Most A’s have been B’ to ‘It is reasonable to expect this A to be B.’ The moves are valid because of the meanings of the phrases ‘probable’ and ‘reasonable expectation.’

One can also see that on this analysis Black’s self-supporting arguments become less perplexing. Now it is necessary to distinguish the two different cases separately—first, the inference to ‘(Probably) all uses of R₁ will be successful’ where R₁ is the rule of arguing from all observed cases to all cases; and second, the inference to ‘Probably the next use of R₂ will be successful’ where R₂ is the rule of arguing from most observed cases to the next case. The second of these arguments is now non-circular but unexciting—just as unexciting as any other case of immediate inference. The first argument is, however, invalid if the conclusion contains the word ‘probably’ and illegitimate if it does not contain it. For if it contains the word ‘probably’ then, as we have seen above, the implication is that we could construct an argument with a higher order generalization which stated that most other general hypotheses posited on the basis of an amount of evidence similar to that which we possess here have not been disproved later. But clearly this particular case is unique so that it is quite impossible to formulate a generalization about similar cases. So the claim implied by the word ‘probably’ cannot be fulfilled and the argument is therefore invalid.

If, on the other hand, the word ‘probably’ is omitted, then the argument conforms to no pattern of inference at all—except the pattern of inference of arguments contained in the induction chapters of logic textbooks. Except when we are discussing induction, we never argue from ‘All observed A’s are B’ to ‘All A’s are B’ but rather to ‘Probably all A’s are B’ or ‘It is reasonable to assume that . . . ’ or ‘We can expect . . . ’ It is true that if the generalizations continue to be confirmed we will drop the qualifying phrase, but this is either because we cease to question the truth of the generalization and instead use it as a premise for other arguments or else because we see that the generalization can be seen as a particular case of a higher order generalization. The words ‘probably’ and ‘necessarily’ (in the sense I have discussed above) are used to indicate the line of argument we have followed; when we go on toward some different conclusion starting from this first conclusion as a premise, then we no longer need these indicator words.

III

It is impossible within the space of a few pages to discuss at all adequately the way in which laws function in natural science. The most that can be done is to indicate some of the lines of thought that have led writers such as Toulmin to conclude that natural laws are more like rules than like statements.

One possible argument, though not one that is now often explicitly held, is that science is interested in the prediction and control of nature. Now all prediction and all control must involve singular statements.
Thus general laws only function as steppingstones or rules of inference, which enable us to move from the set of singular observation statements to prediction statements. This argument is clearly inconclusive. For it is not clear that the intermediate general truths are rules rather than statements. And further, the account only applies to some scientists, especially applied scientists. Many pure scientists are just interested in discovering laws of nature or general truths—and truths or statements, not just patterns of inference.

Another argument that I think has influenced Toulmin starts from the fact that many scientific laws are such that no one conceivable state of affairs would count as a conclusive refutation. I have suggested elsewhere* that this is one of the main considerations that has led Kneale to call natural laws ‘principles of natural necessitation’ and Quine to posit a continuum from particular empirical statements at one end to the laws of logic at the other. But although it may be impossible to refute laws of nature—they are abandoned rather than refuted—it is possible to give evidence in their support. But it is clearly impossible (logically) to give evidence for rules—one cannot give evidence for the statement that certain rules are observed; one can give empirical grounds in support of the claim that certain rules should be accepted; but one cannot give evidence for the rules themselves. And it does not make sense to say rules are true—whether true empirically or true ex vi terminorum. The fact that scientific laws are not refutable in the same way as empirical generalizations must be explained by discussing the way in which the statements of science hang together (cf. Duhem and Quine) and cannot be verified or falsified individually, rather than by evading the issue by calling them rules.

It is also possible that some writers who take this view of scientific laws have been influenced by the same sort of consideration that we considered above—that in drawing an inference the law is usually not stated explicitly. Thus in Britain one might look at an electric light bulb, see that it was marked 60 watts, and then say, ‘It must have a resistance of about 880 ohms.’ This is clearly drawn by means of the formula, derived from Ohm’s Law, ‘Power = E²/R,’ but this formula is not stated. Thus it could be said that this formula served as a principle of inference rather than as a suppressed premise. But equally the argument depends on the fact that the standard British power supply is 230 volts. If this is a suppressed premise rather than a rule of inference, the same can equally well be said of the law that the power dissipated (in watts) is equal to the square of the voltage divided by the resistance in ohms.

The principal argument used by Toulmin* is more interesting than any of these. It is that “laws of nature tell us nothing about phenomena, if taken by themselves, but rather express the form of a regularity whose scope is stated elsewhere” (Introduction to the Philosophy of Science, p. 86). As it stands this is unexceptionable; for, at most, it represents a slight narrowing of normal scientific usage in confining the name law to the mathematical formula which is, as it were, the core of the statement. But Toulmin goes on to draw a parallel between these laws—in his narrow sense—and rules or principles. “In this respect, laws of nature resemble other kinds of laws, rules, and regulations. These are not themselves true or false, though statements about their range of application can be” (p. 79). On p. 93 he cites with approval Ryle’s metaphorical phrase ‘inference-ticket’, and on p. 102 he also claims that there is a similarity between laws of nature and principles of deductive inference.

Now as a matter of scientific usage, it is just false to say that the statement of a law never mentions its scope.† If one thumbs through a textbook of optics, one finds the following statements:

“Kirchoff’s law of radiation states that the ratio of the radiant emittance to the absorptance is the same for all bodies at a given temperature.”

“Bouguer’s law of absorption states that in a semi-transparent medium layers of equal thickness absorb equal fractions of the intensity of the light incident upon them, whatever this intensity may be.”

“According to Stokes’ law, the wavelength of the fluorescent light is always longer than that of the absorbed light.”

“Fermat’s principle should read: the path taken by a light ray in going from one point to another through any set of media is such as to render its optical path equal, in the first approximation, to other paths closely adjacent to the actual one.”

In all these the scope is included in the statement of the law. But

† This has been pointed out by reviewers of Toulmin’s book, e.g., M. Scriven in Philosophical Review, 64:124–128 (1955) and E. Nagel in Mind, 63:403–412 (1954).
it is true that Toulmin's account does apply in some cases. Thus, as he points out, a textbook will say, "In calcite, Snell's law of refraction holds for one ray but not for the other"; it does not say that when calcite was discovered, Snell's law was found to be not strictly true. Does it follow from this that Snell's law is a rule or an inference license? The first thing to be noticed here is that any scientific law—unlike simple generalizations of natural history—is multiply general; that is if we were to represent it by a logical formula, we would need several quantifiers. At the first stage Snell's law gives us a formula which sums up the relations between the angles of refraction and the angles of incidence for all angles of incidence. Second, Snell's law—if we use the name now to include not only the formula but the statements of its scope—tells us that for all different specimens of the same two media, the formula holds and the constant of proportionality between the sines of the angles is the same. Third, Snell's law tells us that the formula holds for all—or almost all—transparent media but that the constant of proportionality is different for different pairs of media. And perhaps fourth one might say this was true at all times. Thus theoretically the law could be falsified in four different ways: (1) by finding, say, that the refractive index of substances varied systematically with time; (2) by finding a transparent medium for which the law did not hold; (3) by finding that there were certain pieces of flint glass which, though identical in all other respects, had different refractive indexes; (3a) by finding that there were certain pieces of flint glass identical in all other respects with normal specimens for which the relation was \( \sin i / \sin r = \text{constant} \); (4) by finding that all previous observations had been wildly mistaken, and that the law which held for all transparent media was \( \sin i / \sin r = \text{constant} \). Now because of this multiple generality of law statements, it is understandable that a shortcoming in one respect should not lead us to abandon it altogether.

In practice, the fourth kind of 'falsification' will always be such that the first formula will be a valid first approximation to the more accurate formula. There will therefore be cases in which the law in its first inaccurate form will still be used. The third kind of falsification never, I believe, occurs in the physical sciences. Two specimens will never differ in only one respect, and therefore if two pieces of glass have different refractive indexes we know they will exhibit other differences so that they cannot be called instances of the same kind of glass. 'Falsifica-

cation' in the second way is not infrequent; as in the case of abnormal refraction, we do not abandon the law completely but note that its scope is restricted. The first kind of disproof has never yet been observed, but if Whitehead is right it may sometime be found.

But this feature of multiple generality is not confined to the physical sciences. As a British visitor to the United States, I am expected to report some general facts about American university life. These reports could be doubly general, that is, true about all (or most) students or faculty on all (or most) campuses. Basing my reports on my observations on the Minnesota campus, I might reach conclusions such as "Almost all men undergraduate students dress informally, with no ties and no jackets (coats)" or "Most full professors and university administrators wear dark suits." An American friend will then tell me that the latter generalization is true of all American campuses, but that the former only holds in the Midwest. So what I might do is make generalizations like those in the accompanying list.

**Generalization**

<table>
<thead>
<tr>
<th>All professors dress formally</th>
<th>Everywhere</th>
</tr>
</thead>
<tbody>
<tr>
<td>Undergraduates dress very informally</td>
<td>Midwest</td>
</tr>
<tr>
<td>Undergraduates take outside work in term time</td>
<td>Not in liberal arts colleges</td>
</tr>
</tbody>
</table>

Now if I constructed such a list, Toulmin would presumably say that the sentences on the left were rules or inference licenses. But this is ridiculous. If anything they are analogous to propositional functions. That is, they are incomplete statements which can be completed by either supplying a particular context, e.g., 'on the Harvard campus' or else by quantifying, 'on all campuses', 'on some campuses', or 'on most campuses'.

This, I think, is sufficient to show that Toulmin's argument is invalid. But there is still a problem as to the way we apply laws to particular situations. Nagel, for instance, points out that we can infer the motion of a projectile from the laws of mechanics. This, presumably, might be done by treating the projectile as a homogeneous body of mass m, imagining that the air resistance is a simple function of the velocity and can be represented by a simple force acting in the line of motion, and neglecting any effects due to the spin of the projectile. We then write down the equations, substitute in the initial conditions,
and deduce that the projectile will land one thousand yards away in
the direction in which it was fired.

This deduction therefore takes two steps. The first is to the inter-
mediate conclusion that certain laws, and only these laws, are applicable
to this instance; the second is the deduction made by substituting initial
conditions into these laws. Often the first step proves to be unjustified
as in the case when one applies the simple laws of mechanics to the
flight of a cricket ball or baseball and reaches the conclusion that such
balls never ‘swerve in the air.’ Sometimes the deduction may be wrong
because the formula used in the deduction is only true to a first approxi-
mation and in this particular case the second order differences are
important.

But an exactly similar account would apply to my deduction that
Franklin Smith usually wears a T-shirt. First, I would notice that he was
an undergraduate at a Midwestern state university; then I would see
which laws held for Midwestern state university campuses and draw my
deduction. Of course, in this example, both steps of the inference are
of the same kind and both are very obvious. In the case of the projec-
tile, the first step is by means of a verbal argument, the second by
mathematical reasoning. It is therefore understandable that the attention
of scientists should have been focused on the second stage and that
as a consequence philosophers of science should have been misled into
thinking that the mathematical formula by itself was the law. In fact,
the formula by itself is only a formula; when the scope is supplied it
becomes part of a statement. But in no sense is it like a rule.

IV

A. The fourth argument in support of material rules of inference is
one put forward by Wilfrid Sellars and expressed most clearly in his
paper “Inference and Meaning.” * Briefly, it is that subjunctive con-
ditionals are expressions in the material mode of material rules of in-
fERENCE in the formal mode. He writes thus:

“If anything were red and square, it would be red” cannot plausibly

* Mind, 62:313–338 (1953). Professor Sellars’ paper in this volume completes,
and also to some extent modifies, the theory outlined in his earlier article. Since I
wrote the present paper before his paper for this volume was completed, I have only
discussed in the text his earlier article. Thus some of my criticisms are not applicable
to his present views, and some of them have been implicitly countered by some of
his arguments.

be claimed to assert the same as ‘(In point of fact) all red and square
things are red’; this subjunctive conditional conveys the same informa-
tion as the logical rule permitting the inference of x is red from x is red
and x is square” (p. 323).

“If there were to be a flash of lightning, there would be thunder”
gives expression to some such rule as “There is thunder at time t + n
may be inferred from there is lightning at time t.” (p. 323).

“Whenever we assert a subjunctive conditional of the latter form
(‘if x were A, x would be B’), we would deny that it was merely in
point of fact that all A’s are B” (p. 324).

“Unless some way can be found of interpreting such subjunctive
conditionals in terms of logical principles of inference, we have estab-
lished not only that they are the expression of material rules of in-
fERENCE, but that the authority of these rules is not derivative from
formal rules” (p. 325).

“Material transformation rules determine the descriptive meaning
of the expressions of a language within the framework established by its
logical transformation rules. In other words where ‘ya’ is P-derivable
from ‘ya’, it is as correct to say that ‘ya ⊃ ya’ is true by virtue of the
meanings of ‘P’ and ‘ya’, as it is to say this where ‘ya’ is L-derivable
from ‘ya”’ (p. 336).

Thus Sellars’ view, as expressed in his 1953 paper, is that a neces-
Sary and sufficient condition for a generalization to provide grounds
for asserting a subjunctive conditional is that the generalization shall
be the expression in the object language of a material rule of inference.
That is, the generalization has either to be one that contains the word
necessarily explicitly or else has to be in such a context that necessarily
is implied. Generalizations of this kind can then be said to be true
ex vi terminorum, if we take the widest possible concept of meaning;
for such generalizations can all be ranged in a continuum which ends
at one extreme in ‘truths’ such as ‘All bachelors are unmarried.’

It appears then that we can place all possible generalizations into four
classes. At what might be termed the lower extremity, we have those
generalizations which can only be established by complete enumer-
ation—of these the standard example is ‘All the coins in my pocket were
minted after 1940.’ From this first class one can only derive what Good-
man calls counter-identical conditionals such as ‘If this coin had been
identical with one of the coins in my pocket, it would have been minted
after 1940.’

The second class contains those empirical generalizations which are
arrived at by genuine induction but which are not ‘necessarily true,’ not expressions of material rules of inference. These are the truths which Sellars regards as ‘merely true in point of fact.’ They also are below the salt, for from them, Sellars holds, one can again derive nothing stronger than counter-identical conditionals. A special subclass of such generalizations would appear to be general statements which mention a particular individual.

The third class—which, as we have seen, merges into the tautologies of which the fourth class is made up—comprises the laws of nature, necessary truths which hold by virtue of the meanings of the words. From such statements (and from the tautologies) one can derive genuine counterfactuals.

(It is however important to realize that one cannot always derive the most usual type of counterfactual. Thus, if one assumes that solar disturbances are a necessary and sufficient condition for the occurrence of auroral displays, we can assert that ‘If solar disturbances had occurred on Tuesday, auroral displays would have taken place on Wednesday.’ On the other hand, it would sound odd to say ‘If auroral displays had taken place on Wednesday, solar disturbances would have occurred on Tuesday.’ Rather we would say ‘If auroras had occurred, this would have meant that solar disturbances had occurred.’ Similarly we might on the basis of our knowledge of segregation in the United States in general and Jonesville in particular, say ‘If Smith had been colored, he would have lived in northeast Jonesville.’ But we should not say, ‘If Smith had lived in northeast Jonesville, he would have been colored’; we could only say, ‘If ..., it would have meant that...’)

It is however easy to show that this theory, as I have interpreted it, is untenable. For consider the case of a husband who, after twenty years of a happy marriage, remarks to his wife ‘You would have been furious if you had been there. The youths were torturing that cat out of pure sadistic pleasure.’ Here we have an example of a counterfactual being asserted by the husband—let us call him Mr. Smith—because he knows by experience that his wife becomes angry whenever she sees wanton cruelty to animals. I would suggest that this is clearly a generalization of the second class. It mentions an individual; it does not explicitly or implicitly include the adverb ‘necessarily’; and it can by no stretch of the imagination be said to be true ex vi terminorum because of the

meaning of ‘Mrs. Smith’. Yet the counterfactual is clearly a genuine one.*

It might be suggested in reply that although this generalization is empirical, the counterfactual really rests on a more fundamental generalization, some psychological law about the reaction of certain types of people in certain circumstances. But this is implausible. Are we going to say that Mr. Smith is able to make such counterfactual statements about his wife because he has majored in psychology at college, but that poor Mr. Brown, married equally long and equally happily, is unable to do so because he never has opened a psychology textbook or even attended a university? Or are we going to say Mr. Brown implicitly knows and relies on the psychological law even though he explicitly refuses to assent to the law when it is quoted to him?

It is true that one might say that both Mr. Smith and Mr. Brown are making the assumption that their respective wives have acted in consistent ways and will continue to do so. But this would only be to refer to the assumption of the uniformity of nature, the assumption which may be said to be implicit in every generalization reached by genuine ampliative induction and thus implicit in every generalization of the second group. In fact, this line of defense would make the second class empty, for it would ascribe a necessary nomological connection to each and every generalization in virtue of the fact that it was reached by ampliative induction.

There is an even simpler argument which enables us to reach this same conclusion. If one considers any particular counterfactual such as ‘If this stone had been released, it would have fallen’ or ‘If Grey had clearly and publicly explained Britain’s foreign policy, the first war would never have occurred,’ one sees that the way in which one would justify such assertions is exactly similar to the way in which one would justify a prediction. Mr. Dulles, knowing that President Eisenhower is about to announce the Eisenhower doctrine, tries to predict what world reaction will be; Mr. Smith, seeing his small son about to drop a stone out of the window, predicts what will happen; and the way in which they

*Professor Sellars now admits that his 1953 paper did not take into account these singular counterfactuals, and that it would be misleading to say that generalizations corresponding to these singular counterfactuals were true ex vi terminorum. His paper in this volume makes it clear that he is willing to say such generalizations are in a way necessary and that he therefore does not need to have recourse to either of the arguments I discuss in the two succeeding paragraphs.
**H. Gavin Alexander**

do so is the same as the way in which counterfactuals are established. The only difference is that predictions can usually be conclusively verified (or falsified) later; counterfactual conditionals never can be. Now it is obvious that in predicting the future we use empirical generalizations (of the second class); it is clear to me that we also use them in order to arrive at counterfactuals.

We have now seen that having the characteristics of the third class is not a necessary condition for a generalization to give rise to subjunctive conditionals. I want to go on to make the stronger claim that the third class, as defined by Sellars, is empty, that there are no laws of necessary connection. My claim is that when a generalization includes the word *necessarily* or when we say a general statement is necessary, one of a variety of different things is meant. One use is that mentioned above. We often say ‘All A’s are necessarily B’ when we are claiming not only that ‘All A’s are B’ but also that this fact can be justified by a deductive argument from a more general premise which is accepted by the speaker and usually by the listeners also. This is exactly the same as particular statements such as ‘It must be going to rain.’ Closely allied to this is the case when the generalization is justified by reference to considerations of a different kind as when we say, ‘All voters are necessarily over 21’ because of the law that no one under 21 may vote. Also similar is the case when the generalization is justified by reference to the meaning of the constituent words—the case of tautologies. What is common to all these is that in inserting the word *necessarily* we are indicating that our assertion is not based on examination of individual A’s which are B, and that it is therefore useless to search for A’s which are not B.

This, I believe, covers all the cases in which the word *necessarily* is actually included in the sentence itself. The same claim could conceivably be made by saying that the general statement ‘All A’s are B’ was necessary—but this would sound rather unnatural. There are, however, other situations in which philosophers might call a general statement necessary, although they would hesitate to insert the modal adverb necessarily in the object-language sentence. These are the cases, mentioned above, when a scientific generalization becomes so well established that no one conceivable state of affairs would be taken as constituting a refutation of it. This is what is sometimes called the functional or the pragmatic a priori. But this sort of necessity consists in

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the way in which scientists regard certain laws, the way in which they use them; it is not in any way an ‘intrinsic property’ of the statements. And since, if questioned, scientists would cite empirical evidence in support of these laws, it would, as we have seen, be misleading to say that they were true ex vi terminorum.

B. There is, however, another way in which some philosophers have attempted to distinguish a special class of ‘necessary’ or ‘basic’ laws. This distinction is most easily explained by considering a universe which consists of a perfect billiard table and two perfect billiard balls in motion on it. It might be the case that because of the initial conditions, these two balls would never collide. If so, then the statement that these balls would never collide would be lawlike inasmuch as it would always be true, but yet it would be different from, say, the laws of reflection which hold for the collisions of the balls with the cushions. These latter laws could be called basic or necessary, the former structure dependent or contingent.

Here we have a view which is nearer to Kneale’s than to Sellars’. For it might seem that such basic laws would be ‘principles of natural necessitation’ to use Kneale’s phrase. That is, we would almost seem to have here a concept of metaphysical necessity instead of the concept of pragmatic necessity discussed by Sellars.

The difficulty about this distinction is that of applying it to our actual universe. For until, like God, we know all the secrets of the universe, it will be impossible to know which laws are structure dependent and which are not. If, for example, the Mach-Einstein program were carried through, then some of the basic laws of mechanics would be shown to be structure dependent. A second possible criticism of this distinction is that its persuasiveness is only superficial. Examples like the billiard table make the distinction appear important because experience in our actual universe has taught us that the laws of collision hold for all billiard tables and for other collisions as well, not just for the one table; the fact that the balls never meet we know would never hold for every table. That is, in considering the simple billiard table universe we tacitly bring in our knowledge of our actual multi-billiard-table universe. If—to formulate a rather outrageous counterfactual—I were one of the two billiard balls (and still possessed means of making observations), I might distinguish the two types of law but neither would seem more basic or more necessary than the other. “The laws
of collision might have been different, the initial conditions might have been different—but both are what they are."

Finally it should be noticed that even if one did accept this distinction, it would not coincide with the distinction Sellars wants to draw between his second and third class of general statements. For it is, I think, clear that psychological laws, sociological laws, biological laws, and possibly, as we have seen, even the laws of mechanics are structure dependent. There cannot be many basic laws remaining. If it were only these basic (and probably as yet unknown) laws that give rise to counterfactuals, then counterfactuals would be confined to the pages of the most recondite physics books, instead of being a pervasive feature of our ordinary language.

C. Thus the word necessary (and its cognates), when included in a general statement in the object language or used in the metalanguage to describe an object-language generalization, is ambiguous. When included in the statement it signals a claim about the way in which the generalization is justified; in the other cases when used about the generalization, it says something about the role the generalization plays in scientific theory. And, if this explanation is correct, we need no longer look for ties of necessary connection or even for statements (other than tautologies) true ex vi terminorum, or for object-language expressions of logically prior material rules of inference.

Actually some of the plausibility of Sellars’ argument is due to the misleading use of the phrase in point of fact. We are left with the impression that the only alternative to his view of generalizations as expressions of material rules of inference is to take them as generalizations which are ‘in point of fact true.’ But though in one sense an empiricist would accept this, it is unintentionally misleading because we often use the phrase in point of fact to distinguish between general statements which can only be validated by perfect induction—‘all the coins in my pocket were minted after 1940’—and those which are reached by ampliative induction. Of course, any empiricist would admit that, if and when we find them, the basic laws of the universe will, in a sense, be true in point of fact. But this is only to deny that they are tautologies, ‘principles of natural necessitation’ (whatever that may mean), or rules. It is not to deny that these laws fit into a very complex theoretical system and that they are such that they are not easily refutable by direct empirical evidence. And, more important, it is not to assert that they only happen to be true or are true by chance—for this notion of chance only applies within a universe in which either the basic laws are indeterministic or else are so complex that men do not know them; the notion cannot meaningfully be applied to the universe as a whole. When one realizes this, one sees that there is no need to accept Sellars’ third category of generalizations. Empirical generalizations of differing scope and generality are sufficient for prediction, retrodiction, explanation, and the formation of counterfactual conditionals; what more could one ask for?

We have thus seen how unsatisfactory are most of the arguments that have been put forward in support of the thesis that material rules of inference are an indispensable feature of our language. The strongest argument is that which starts from the fact that we often argue directly from particular premise to particular conclusion without stating any major premise. If one wished to construct a logic that represented the way we explicitly argued, then one would have to recognize material rules of inference. But does the idea of such a logic make sense? We have seen that it would also have to be applied to what previously have been regarded as enthymemes of the second order. It is however clear that such a logic would have to be so complicated as to be completely unwieldy—do we not before using logic often ‘put statements into logical form,’ that is, say that statements of different grammatical form have the same logical form? A logic which considered all the different ways arguments might be put would be very different from any present logic, and it would almost certainly be a logic which would be of little philosophical use.

If then one rejects the idea of such a logic, very little remains to be said in favor of material rules of inference, inference licenses, or inference tickets. In fact, I fail to see that these terms are of any use in a philosophical investigation of the way in which we talk, either in science or everyday discourse.