BRIAN SKYRMS

Physical Laws and the Nature of Philosophical Reduction

Introduction

This paper is addressed to a philosophical problem posed by physical laws. The problem is one of meaning and metaphysics. How are the meanings of physical laws to be distinguished from the meanings of accidental generalizations? Two prima facie alternatives are as follows:

(1) Physical necessity and possibility and causal relationships are *real relations* in the world. The claim made by a physical law is the claim made by the corresponding accidental generalization *plus* the claim that such relations obtain.

(2) The apparent difference in meaning between physical laws and accidental generalizations is an illusion. The meaning of the law "All *F*'s are *G*'s" is exhausted by "$(x)(Fx \supset Gx)$."

(1) and (2) are not without distinguished proponents. However, to many of us (1) is a metaphysical romance and (2) is unable to account for the role of laws and disposition terms in scientific (or for that matter nonscientific) thought. Attempts to avoid both (1) and (2) have centered on achieving a *philosophical reduction* of laws of nature and related suspect concepts to a *philosophically clear* language where they can be compared with extensional generalizations, and the difference between the two exhibited.

Despite an enormous expenditure of effort and intelligence, no reduction to date seems to work. Is it possible that no reduction can work? This question forces us to ask ourselves another. Just what constitutes an acceptable philosophical reduction? The answer to this question is by no means apparent. And this suggests the attractive speculation that at least some of the disheartening results have been the result of an overly narrow view of philosophical reduction.

In fact, I believe that this is the case; that progress has been blocked by an inadequate view of philosophical reduction; and that this view of reduction ultimately stems from an inadequate philosophy of language.

1. Ideal Language Games

The classical view of language goes something like this:[1]

> In the study of language use, we distinguish three factors: (1) The language user together with the context in which he uses (speaks, hears, etc.) the elements of language; (2) those elements of the world that are referred to or designated by the elements of language; and (3) the elements of language themselves. The discipline which studies factor (1) (whether in connection with factors (2) and (3) or not) is called *pragmatics*. That which abstracts from (1) and deals only with the relation of (2) and (3) is called *semantics*. That which abstracts from (1) and (2) and deals with (3) alone is called *syntax*.
>
> However, as long as we confine ourselves to the *description* of language use we can never really abstract completely from (1). We may get statements like "In 98 percent of the cases 'mond' was used to refer to the moon." Such statements may be called *descriptive semantic* statements because of their focus on designation relations. Nevertheless, they are part of pragmatics since they refer to incidents of language use.
>
> The only way that reference to (1) can be really eliminated is by the construction of an idealized language system *L*. Then the contingent statement (A) is idealized to:
>
> (A′) "Anyone operating in system *L* who uses 'mond' uses it to refer to the moon."
>
> (A′) is taken to be definitive of L and thus necessary. (A′) licenses (or perhaps is equivalent to) the *semantical rule*:
>
> (A″) 'mond' designates (in *L*) the moon.
>
> (A″) is a statement of pure semantics (which, unlike descriptive semantics, is not a part of pragmatics).
>
> Thus pragmatics corresponds with the descriptive study of language use, whereas pure semantics and syntax define an idealized language system.

If we identify pure semantics and syntax with the sorts of considerations which normally go under those names, then the preceding picture will lead us to identify an *idealized language system* with the familiar notion of a formal language. Such a conclusion, however, would be too hasty. If we look at the process of idealization closely, we will find that

[1] See Morris (1939), Carnap (1937a, 1937b, 1939), and appendix D of Carnap (1956).

it yields rules which do not fall within semantics or syntax as commonly conceived.

One example of idealization which led to an ordinary *semantical* rule has already been indicated. In order to get a syntactical rule of inference we need to idealize some notion of conditional acceptance or belief. Roughly, we start with

(B) Almost all instances in which an English user accepts a sentence of the form

α and if α then β

he is thereby disposed to accept β

and end up with modus ponens.

Here, I think, as in all other cases we are idealizing on certain attitudes, intentions, and dispositions that language users have toward the elements of language and toward each other. The same process yields the following:

(C) In almost all cases in which an English user uses the word "I" he uses it to refer to himself.

(C′) (x) Relative to a context where the sign user is x. "I" denotes (in *L*)*x*.

This sort of idealization leads to language systems with context-variable semantics as developed by Richard Montague and others[2] under the name "pragmatics."[3] This "pure pragmatics" exposes an illicit assumption of the classical view. That is, the assumption that *all* reference to the sign user and context of use must be eliminated in the process of idealization. What is important in the process is that empirical regularities are idealized into rules, and that following these rules is definitive of use of *L*.

Consider now the notion of conditional acceptance or belief utilized in (B) to deliver, under idealization, the syntactical notion of a rule of inference. If this notion is suitable for idealization, then why not a notion of partial conditional degree of acceptance or belief. Here acceptance of a probability measure is built into the definition of using a certain lan-

[2] See Montague, "Pragmatics" and "Pragmatics and Intensional Logic."

[3] It *is*, of course, pragmatics, *as defined* since it studies relations between signs and contexts of sign use. It is *pure* pragmatics since the process of idealization has taken place. But in terms of the purposes to which the contexts are put, it is closer to semantics than to other parts of pragmatics.

guage system *L*.[4] Likewise, "*x* takes *p* to be an adequate explanatory account for *q*" may be idealized to "*p* is an adequate explanatory account for *q*"[5] and "*x* regards *p* as relevant challenge to *q*" (where *q* might be a question or an imperative or a factual claim) can be idealized to build rules of rational dialectic[6] into the language system.[7]

Recalling that idealization need not eliminate reference to the sign user and context of use, we can go even further. One way is to build in practical reasoning — rules which connect belief or degree of belief with action. Another way is to build in connections between having certain experiences and changes in degree of belief of associated sentences.[8]

[4] Carnap's conception of probability usually seems quite different from this. But he appears to come close to this view in section 2, "Linguistic Frameworks," of "Empiricism, Semantics, and Ontology." Speaking of the thing-world framework, he says that questions of existence and reality ". . . are to be answered by empirical investigations. Results of observations are evaluated according to certain rules as confirming or disconfirming evidence for possible answers. (This evaluation is usually carried out, of course, as a matter of habit rather than a deliberate rational procedure. But it is possible, in a rational reconstruction to lay down explicit rules for the evaluation. This is one of the main tasks of a pure, as distinguished from a psychological, epistemology.) The concept of reality occurring in these internal questions is an empirical, scientific, non-metaphysical concept. To recognize something as a real thing an event means to succeed in incorporating it into the system of things at a particular space-time position so that it fits together with the other things recognized as real, according to the rules of the framework" (Carnap, 1950, p. 207).

This would make the justification of the confirmation rules an *external question*. It would be part of the question of justifying the adoption of *this linguistic framework* rather than another (and not necessarily a neatly separable part).

[5] q could have been either a sentence or an event. In the first place we end up with a syntactic relation (in the sense of being a relation between items of language); in the second a semantic relation (in the sense of being a relation between an item of language and an item in the world).

[6] See Skyrms (1967).

[7] I argued in Skyrms (1966) that these sorts of things could be built into the notion of an ideal language game. But I classified them as "pure pragmatics" on no better grounds than that they did not belong to semantics, conceived of as the theory of reference (and perhaps sense) or the theory of syntax, conceived of as the study of purely formal properties of signs. Of course the metalanguage defining the ideal language game can be divided up any way you please. But it need not be divided in such a way as to give semantics and syntax such restricted roles. See fn. 10.

[8] This sort of idea has been persuasively advanced by Richard Jeffrey. "[C]oming to have suitable degrees of belief is a matter of training — a *skill* which we begin acquiring in early childhood, and are never quite done polishing. The skill consists not only in coming to have appropriate degrees of belief in appropriate propositions in paradigmatically good conditions of observation, but also in coming to have appropriate degrees of belief between zero and one when conditions are less than ideal.

"Then learning to use a language properly is in large part like learning such skills as riding bicycles and flying aeroplanes. One must train oneself to have the right

Pure syntax, semantics, and pragmatics may now appear as rather motley categories.[9] Perhaps the traditional way is not the most illuminating way of dividing up the metalanguage. For the time being, I will simply refer to *the metalanguage* without worrying about how or whether to draw the syntax-semantics-pragmatics distinction.

The idea of a language system has already been stretched far beyond the customary notion of a formal language, to the point where I think that it is reasonable to begin using the term "ideal language game." Is there no end to this process?

When an ideal language game is intended as an *explication* of actual language use, the regularities which are idealized into rules should be those which can be taken as indicative of competence of language use. The limits on the process of idealization are then the limits of logical richness of actual language use. Of course what is indicative of competence in language use is rarely clear-cut. Too bad, but that's the way language use is, by and large.[10] For this reason, among others, empirical linguistic regularities will typically underdetermine the corresponding ideal language game, leading to a range of alternative explications.

sorts of responses to various sorts of experiences, where the responses are degrees of belief in propositions" (1968, pp. 179–80).

This view has the attractive feature of enabling empiricist epistemology to avoid the pretense of the myth of the given and the neoidealism of the coherence theory of knowledge (in this case, the coherence theory of rational belief).

[9] Actually, the formulation given is not precise enough to tell us where the boundaries are. Logical consequence is a relation between items of language, unlike the denotation relation. Is it therefore a syntactical relation rather than a semantical one? Or is the distinction an epistemological one; that in order to learn about logical consequence we must *first* study the denotation relation? Is semantics restricted to denotation and things definable in terms of it, or does it include all sorts of relations between items of language and items in the world? What about an idealized explanatory relation between explanatory accounts and events?

There are lots of alternative ways to answer such questions. One way is in terms of various functions of language. For instance, if we are interested in the language function of describing the world, we will need referential semantics which will include that part of traditional syntax *which is exclusive of inference rules.* If we are also interested in inferential functions of language more comes in. How much more depends on how broadly inference is conceived. Epistemological concerns cut across all categories. If we are interested in illocutionary force, etc., we bring in a whole new array of factors. The best way of sorting out all these factors will depend on what question is being investigated.

[10] As a situation where things are relatively clear-cut and which illustrates some of the richness of the notion of an ideal language game, Professor N. Belnap has suggested that we look at the legal procedures for a trial. For example, consider the concept of inadmissible evidence. If certain kinds of evidence are admitted in a criminal trial, an appellate court may declare a mistrial. In doing so, it is in effect saying the criminal trial game has not been played. Rules of admissibility of evi-

2. The Nature of Philosophical Reduction

Philosophical reduction is different in both aim and method from reduction in science.[11] In *scientific reduction,* the typical situation is that of an old theory being *replaced* by a new better theory. The new theory is *incompatible* with the old, but it is a consequence of the new theory that the old should give results which are in some sense an *approximation*[12] to the correct results when applied *within a limited domain* of phenomena. *Philosophical reduction* is not an attempt to *replace* a theory with a better one, but rather an attempt to *make sense* of language use which does not make sense on the face of it.[13]

Such an attempt is motivated by some set of criteria of prima facie philosophical intelligibility. These are often keyed to the type of objects referred to (sense data, physical objects, events, universals) or the way they are characterized (Are spatiotemporal relationships intelligible? Are causal relations?), but there are, no doubt, many other forms a philosophical conscience can assume.[14]

Traditionally, there are two kinds of reductions: those that are successful and those that aren't. Those that are successful establish some kind of translation[15] of prima facie unintelligible discourse into philosophically impeccable discourse. Good examples of such successful reductions are hard to come by (partly because of general disagreement on criteria of philosophical intelligibility and successful translation). An old favorite in Russell's Theory of Descriptions and despite recent criticisms it is still a powerful illustration. What I have in mind as an unsuccessful reduction is an instrumentalist treatment of a certain body of discourse.

dence are thus part of the metalanguage of the ideal language game explicating legal language use in the criminal courts.

[11] See Bohm (1957) and Feyerabend (1962). (This is not a blanket endorsement.)

[12] There are lots of different kinds of approximations. A close look at statistical mechanics and thermodynamics is a good antidote to overly simplistic views of what approximations must be like.

[13] This fundamental distinction is often lost sight of in discussions of Craig's theorem. No phenomonalist in his right mind wants to say that scientific theories should be *replaced* by their Craigian counterparts. The phenomonalist wants to make sense of scientific theories in phenomonalistic terms.

[14] For example, consider the intuitionist criticism of classical logic.

[15] Criteria for a correct translation have come in many shapes and sizes; some stringent, some easygoing, some philosophically unintelligible from other viewpoints. If the criterion for having a correct translation is represented as *having the same meaning,* then the paradox of analysis rears its head. In the general framework given here, however, it is hard to motivate the paradox of analysis at all.

It is the sort of attempt to make sense of a body of discourse that is grasped at when all else fails, but the discourse seems too important to just dismiss as nonsense. The prima facie unintelligible discourse becomes intelligible as a machine which processes intelligible inputs and produces intelligible outputs. The classical example of this sort of move is the treatment by finitists, nominalists, and some constructivists of those parts of mathematics which do not satisfy their respective criteria of philosophical intelligibility.

I wish to consider a conception of philosophical reduction which has these two classical forms as special cases, but which covers a rich spectrum of other cases as well.

Suppose we start with a body of language usage some parts of which are philosophically intelligible and some parts of which are not. A *philosophical reduction* of the prima facie unintelligible parts will consist of (1) an *ideal language game* which is an explication of that body of usage such that the metalanguage used to define the language game is itself prima facie intelligible; and (2) a *justification* of the ideal language game.

The notion of an ideal language game was introduced in the last section in a rather elastic way. The metalanguage defining such a game can have quite a lot or quite a little built into it. Consequently, the concept of a philosophical reduction here introduced partakes of the same character. Degenerate cases of philosophical reduction are almost always achievable. Thus, when approaching some prima facie unintelligible usage we should not ask, "Is a philosophical reduction possible?" but rather, "How rich a philosophical reduction is possible?" "How much can be built into the metalanguage?" "How extensive are the connections set up between the prima facie intelligible and unintelligible usages?" The *richness* of the reduction determines the *degree* to which the prima facie unintelligible usage is rendered intelligible.

It is time now for this general conception to be tied down a little by looking at particular cases. Let us first note that the body of language usage that we start out with might (in whole or in part) consist of the use of some ideal language game.[16] Such a fortunate special case eliminates some of the looseness introduced into a philosophical reduction by

[16] For example, the problem might be in relating an unintelligible language game to an intelligible one, or an intelligible game to unintelligible usage.

the explication relation. Typically, however, the demand for reduction arises out of language usage which does not hew to the lines of the idealized structure of an ideal language game.[17]

In the special case where all that the metalanguage tells us about the prima facie unintelligible portion of discourse consists of formal rules of formation and of inference both within that part and between it and the intelligible part, we have the substance of a classical *instrumentalist* picture.

As an illustration, consider Boolean algebra as originally formulated by Boole.[18] Boole gives a logical interpretation to algebraic symbols as follows: x, y, z are *elective* symbols which represent an election of objects from a universal set. The *product* $x \cdot y$ represents intersection and the *sum* $x + y$ (which is only defined for disjoint sets) represents union. *Difference* $x - y$ represents the class resulting from removing the members of y from the members of x. The symbol 1 represents the universal class and the symbol 0 the empty class. As rules of inference Boole adopts all the procedures of ordinary numerical algebra. This allows the derivation of equations which have no logical interpretation (e.g., equations with numerical coefficients different from 0 or 1). Further derivation from such equations can lead back to logically interpretable equations. Taking the logically uninterpretable equations as the philosophically unintelligible ones, Boole took the classical instrumentalist position. The rules of inference are the rules of ordinary algebra together with the rule $x^2 = x$ for elective symbols. (This rule does not apply to numerical coefficients.) They are clearly sound for the intelligible sentences. The reduction is completed by a *justification* which, in this case, is a proof that chains of inference leading from intelligible sentences through unintelligible sentences back to intelligible sentences are truth-preserving.[19]

As has been already mentioned, a similar instrumentalist position toward large parts of classical mathematics has been adopted by various philosophical schools. Here, however, reduction has failed for lack of an appropriate *justification*.

[17] However, this matter, like so many others, is really a matter of degree. As soon as we talk of *rules* of a language, no matter how few, we have begun the process of idealization. To this extent, English, French, and German are ideal language games.

[18] See C. I. Lewis (1960), pp. 51–72.

[19] See E. W. Beth (1966), secs. 23 and 25.

It may be worthwhile at this point to take a quick look at the relevance of Craig's theorem to the instrumental view of theories. Assume that the vocabulary of a language is divided into theoretical terms and observation terms and that sentences containing only observation terms are prima facie intelligible while others are not. Formal rules of formation and inference are specified for the whole language so that its theorems are recursively enumerable. The instrumentalist wants to regard the theoretical part of the language as a machine for deriving theorems which contain only observation terms. This it certainly is, whatever else it may be. An intelligible metalanguage appropriate to an instrumentalist has two major problems left. The first is to find an appropriate justification.[20] The second is that the instrumentalistic position assumes that an instrumental reduction is the strongest reduction possible. If the theoretical part of the language does other things which can be rendered philosophically intelligible, then the instrumentalist must move from his cramped perspective to a richer appreciation of theories.[21] Having gotten thus far without Craig's theorem, is the instrumentalist helped by Craig's theorem with either of his two outstanding problems?

Craig's theorem tells us that there is a recursively axiomatizable theory, couched only in observation terms, which has as its theorems exactly the theorems of the original theory which contain only observation terms. So what? Given the instrumentalist characterization of the theoretical machine, Craig's theorem tells us that, with its aid, we can build another machine, an observational machine, that will do the same job. This neither helps us justify what these machines do nor gives us any reason to think that we have not neglected other vital functions of the theoretical machine.

Another type of reduction arises when the metalanguage establishes an equivalent intelligible sentence for every prima facie unintelligible sentence. Then we have a classical *translational* reduction (with "equivalent" mirroring the vagueness of "translation"). To be more specific, consider the case in which equivalence is logical equivalence in the fol-

[20] This of course is a problem which, in different guise, plagues realists as well. But we are not here assigning points in metaphysical debate. It is still correct that the reduction cannot be called complete without some such justification.

[21] I find Hempel's point about inductive systematization relevant in just this way. See Hempel (1958), pp. 37–93.

lowing sense: The metalanguage defines, in intelligible terms,[22] a sense of "interpretation of *L*" "true in an interpretation of *L*" for the language game *L*. (These definitions cover both intelligible and unintelligible sentences of *L*.) Two sentences are equivalent if they have the same truth-value under all interpretations. As an example, let us consider again Russell's Theory of Descriptions. The ideal language game which we are considering contains formulas of the first-order functional calculus with identity (intelligible) together with formulas containing '$(\iota\ x)$', '$(E!)$', etc., in specified ways (unintelligible). Here the sense of interpretation is the standard one for the intelligible part, and the task of finding a translation is trivial once the semantics of the prima facie unintelligible part is set up. Neither of these features holds in general for translational reductions. Another example of a successful translational reduction, for those who don't like Russell's Theory of Descriptions, is the reduction of numerical quantifiers (There are at least *n*; there are at most *n*; there are exactly *n*; where *n* is a positive integer) to first-order logic with identity.

The type of translational reduction just discussed is a special case of a semantical reduction. In a *semantical* reduction the metalanguage establishes, *in intelligible terms*, a semantics for each sentence of the language game *L*. This sort of reduction has a special significance because of its connection with metaphysical questions. If we have an intelligible semantics for a sentence, then it makes sense to say that sentence is *true or false*. If we have an intelligible sense of denotation for a class of terms, then it makes sense to say that such things *exist*. If we have an intelligible sense of satisfaction for a property or relational predicate, then it makes sense to say that the corresponding property or relation is *real*.

It is of some importance to notice that a semantical reduction may be possible where a translational one is not, since metaphysical discussions

[22] This requirement may seem too strong for certain kinds of translational reduction. For instance the virtual theory of classes establishes translations of a certain (small) portion of set theoretical discourse into set-free discourse. If set talk were taken as prima facie unintelligible and first-order logic intelligible, this would appear to be an acceptable reduction. But the metalanguage would appear to require set theory (or something which might be considered just as bad) to accomplish its task. Perhaps the best way to look at it is this: Separate the criteria of philosophical intelligibility of the object level C_o and the meta-level C_{2m}. Speak of a reduction having been achieved *from the standpoint m*. Speak of a thorough reduction having been achieved just when $m = o$.

often seem to focus exclusively on translational reductions.

An example which is interesting for just such reasons is the comparison of tense and date languages for dealing with time. Suppose we take the view that date languages are intelligible while tense languages are prima facie unintelligible. Consider the following simplified ideal language:[23]

Predicates:

P (a monadic predicate of times).
= (identity).

Terms:

0 is a term.
Now is a term.
If **a** is a term, **Aa** (one unit of time after α) and **Ba** (one unit of time before α) are terms.

Sentences:

If x and y are terms, **Px**, **Py**, x = y, y = x are sentences.

Sentences containing **Now** are here regarded as unintelligible, all others as intelligible. A semantical reduction is accomplished by the following. A world, W, consists of (1) D = the set containing 0 and the positive and negative integers (the set of times).[24]
(2) Sw = a subset of *Iw* (the extension of ϕ).

Denotation:

0 denotes 0 at *t*.
Now denotes *t* at *t*.
If x denotes y at *t* then **Ax** denotes y + 1 at *t* and **Bx** denotes y − 1 at *t*.

Truth:

Px is true in *W* at *t* just in case $(\exists\eta)$ x denotes η at *t* and $\eta \epsilon$ *Sw*.
x = y is true in *W* at *t* just in case $(\exists n)(\exists m)$ x denotes *n* at *t* and

[23] Here a name of a symbol is formed by printing it in boldface. The name of a concatenation is formed by concatenating the names of its parts. Boldface type was not available for the identity sign; instances of it should be considered boldface as appropriate.
[24] We represent time as a framework concept by choosing D_w to be the same for all worlds.

y denotes m at t and $n = m$.

Logical Equivalence:

x is logically equivalent to y just in case for all worlds w and times t x is true in w at t if y is true in w at t.

The foregoing semantical reduction seems trivial. Yet it establishes that no translational reduction is possible. Consider the sentence "Now = 0." It is true at times 0 and false otherwise. No intelligible sentence has this character, and thus no intelligible sentence is an adequate translation of it. The same argument holds good if we enrich the language by adding truth functions, temporal variables, and quantifiers, earlier than and later than relations and other predicates (allowing the recovery of a full-tensed language by definition). The argument remains the same if we let "0" denote different times in different worlds. A slightly different argument establishes the same result if we make sentence tokens rather than sentences the bearers of truth.[25]

[25] For every predicate, P, and for every term, T, in the foregoing type language there is an infinite family of tokens $[Pu]$, $[Tu]$ indexed by the positive integers. If $T(\mathrm{u})$ is a term token then $\mathbf{P}(s)\ T(\mathrm{u})$ is a sentence token provided that $(\mathrm{u} = s)$. If $T(\mathrm{u})$ and $T(v)$ are term tokens then $T(\mathrm{u}) = (s)\ T(v)$ and $T(v) = (s)\ T(\mathrm{u})$ are sentence tokens provided that $\mathrm{u} = s = v$. In the token language a world, w, must also specify a function fw from the positive integers into Dw [specifying the time of production $fw(v)$ of a token, e.g., $\phi(\mathrm{u})$ $\mathbf{Now}(\mathrm{u})$, in w.] The semantics must be changed as follows:

Denotation:

$\mathbf{0}(\mathrm{u})$ denotes 0 in w.

$\mathbf{Now}(\mathrm{u})$ denotes $fw(\mathrm{u})$ in w.

"at t" is replaced by "in w" in the other definitions.

Truth:

$\mathbf{P}(\mathrm{u})x$ is true in w just in case $(\exists n)x$ denotes n in w and $n \in Sw$.

$x = (\mathbf{u})y$ is true in w just in case $(\exists n)$ $(\exists m)$ x denotes m in w and y denotes n in w and $n = m$.

Logical equivalence:

Tokens x and y are logically equivalent if their truth-value agrees in all worlds. Logical necessity and impossibility for tokens are defined as truth in all or no worlds.

That there is still no translational reduction possible can be seen from the following: Consider the sentence token $\mathbf{0}(\mathrm{u}) = (\mathrm{u})\ \mathbf{Now}(\mathrm{u})$. It is true in a world, w, just in case $f_w(\mathrm{u}) = 0$. Thus there are two worlds differing only in the function f, such that in one the chosen sentence token is true; in the other it is false. Since the truth-value of sentence tokens not containing Now-tokens does not depend on choice of f, there can be no sentence token not containing a Now-token logically equivalent to the token in question.

This result, of course, comes directly from allowing a token to be produced at different times in different worlds. This is not a trick, however, but rather is necessary for an adequate explication. For if a token had to be produced at the same time

The miniature temporal language game that I used to illustrate semantical reduction did not incorporate any formal rules of inference. In other cases (modal logic, intuitionistic logic) inference rules will play a prominent role in the language game accomplishing the reduction.

In these cases a justification of the inference rules is called for. This may take the form of a proof of soundness or even completeness. The exact role of the inference rules and nature of the justification required will depend on the character of the language use being explicated.[26]

The types of reduction discussed fall within the purview of explications accomplished by formal languages as traditionally conceived. The generalization to reductions accomplishable by an ideal language game multiplies the types of reduction possible. For example, consider an ideal language game where degrees of belief are required to obey the rules of the probability calculus and where players are required to coordinate them with choices by way of a rule of rational decision — say the rule to maximize expected utility. Suppose that statements of fact and attributions of value to world states are taken to be intelligible, but that, because of complementarities of goods, attribution of values to less specific goods are held to be unintelligible (G. E. Moore's principle of organic unities). These prima facie unintelligible values may be reduced by defining them as probability weighted averages of the true values of world states.[27] The corresponding value attribution statements are not now to be regarded as true or false, since they depend both on the values of the world states and on the subjects state of belief. But they have a pragmatic significance by virtue of their relation to rational decision.

Any of the other possible facets of an ideal language game may figure in a philosophical reduction. The more closely the unintelligible usage is knitted to the intelligible usage, the better the reduction. Thus, the logic of explanation, the logic of practical reason, and other varieties of what is now called "philosophical logic" can join with the logic of partial belief in effecting a philosophical reduction. It is hard to specify precisely in general what sort of justifications are wanted for philosophical reductions. Roughly, however, justifications always seem to be proofs of

in each world, tokens such as $\mathbf{O}(u) = (u)\ \mathbf{Now}(u)$ would have to be either logically necessary or logically impossible.

[26] For instance, given the historical context in which the intuitionist propositional calculus was advanced, a proof of completeness rather than merely soundness is required for a successful philosophical reduction.

[27] See Jeffrey (1965).

some type of coherence between the various strands of the ideal language game effecting the reduction. The proofs forming the basis of Bayesian decision theory[28] constitute just such a coherence proof linking a type of logic of partial belief to a type of logic of practical reason.

A philosophical reduction bestows *meaning* on prima facie unintelligible usage. Due appreciation of the various forms of philosophical reduction leads to the conclusions:

(1) There is a broad spectrum of different *kinds* of meaning.

(2) The meaning conferred by a reduction is a matter of *degree*.

(3) The meaning is conferred to a body of usage. It is only derivatively or in special cases that we can speak of the meaning of a term or a sentence.

The entire discussion so far has been relativized to a set of criteria of philosophical intelligibility. But how do we find the correct criteria of philosophical intelligibility? Certainly there is no consensus (and if there were one it would probably only be of sociological interest). I am not sure that we ought to think that there is an answer to this question. Differing philosophical consciences may just be what we are stuck with. And I do not think that this is necessarily a philosophical disaster. Suppose that from perspective A a certain body of usage is prima facie unintelligible and from perspective B it is intelligible. A philosophical reduction, achieved from perspective A, is not *worthless* from perspective B. From perspective B *conceptual connections* are still being uncovered, although within a body of discourse which is *already* intelligible. Is it then more intelligible from perspective B? It is certainly better understood. It is reasonable to attempt a philosophical reduction of prima facie intelligible usage. In general, then, it might be better to switch to more neutral labels: the *reducing* usage and the *reduced* usage. Philosophical reduction, then, has a rationale independent of philosophical perspective — the rationale of charting conceptual interconnections. What philosophical conscience brings to a particular project is a *sense of urgency*.

3. Laws and Extensional Generalizations

The philosophical perspective operative for the rest of this paper is that laws are prima facie unintelligible and that sentences of extensional

[28] See Ramsey (1960); Jeffrey (1965).

logic are prima facie intelligible. The simplest reduction imaginable would be at hand if a law, "All *F*'s are *G*'s" could be adequately represented by its extensional counterpart "$(x)\ (Fx \supset Gx)$"; and the acceptability of the law would be construed to be just the probability of that extensional counterpart. Such an identification, however, gives rise to a cornucopia of puzzles and paradoxes. These difficulties are grounds for giving serious attention to radical alternatives.

The radical alternative with the most distinguished pedigree is *qualified instance confirmation*. The qualified instance confirmation of the law "All *F*'s are *G*'s" is the conditional probability $Pr\ (G\alpha \text{ given } F\alpha)$ for some new individual constant α not mentioned in our evidence. (The instance confirmation is $Pr\ (F\alpha \supset G\alpha)$.) Carnap, like Ramsey before him, suggested that it is this quantity — rather than the probability of the extensional counterpart of the law $Pr(x)\ (Fx \supset Gx)$ — which is relevant to grading the acceptability of laws.

There are powerful considerations pressing us in both the "instance" direction and the "qualified" direction. But there are also strong arguments that appear to block any such treatment of laws and throw us back to the identification of laws and extensional generalizations. We shall see, however, that it is possible in large measure to combine the advantages of both views; and that the means of effecting this combination illuminate the nature of nomic necessity.

4. The Paradox of Provisional Acceptance

The argument pushing us in the *instance* direction is quite simple. When we accept a law, we do so with the expectation that it will eventually be superseded and replaced by a more adequate law. This expectation is inductively well founded in the history of science.[29] Thus, when we accept a law we do so with the expectation that someday a counterinstance will be found. But if there *is* a counterinstance, whether it has been found yet or not, the factual claim made by the extensional counterpart of the law is *false*. So it appears that when we accept a law, we take it as making a certain factual claim and we believe both that that claim is true and that it is false.

To say that we accept laws only provisionally is not very helpful without an explanation of why we should provisionally accept something

[29] This point is developed in great detail in Bohm (1957).

we believe to be false. To say that laws acquire high probability rather than certainty leaves us with the embarrassing question: How can a statement and its denial both have high probability?

In short, if, when we hold a law in high regard, we believe it probable that we will eventually find a counterinstance to that law, then that holding in high regard cannot consistently be construed as attributing high probability to the relevant extensional generalization.

That regard can consistently be interpreted as attributing high instance confirmation to the law, since one can quite consistently hold (I) that it is probable that some item constitutes a counterexample and (II) that there is no item such that it is probable that *that* is a counterexample.

And high instance confirmation gives some reason for holding in high regard some statements we believe to be false, since each application of such a statement would be a low risk operation.

5. Predictive Force

The qualified instance confirmation of a law measures a certain kind of predictive force. It is this kind of predictive force which is relevant when we come to know $F\alpha$ with certainty or near certainty and predict $G\alpha$ on the basis of this knowledge together with the law.

The classical account of the predictive function of laws goes something like this: A law makes an assertion about the world; the assertion expressed by the associated extensional generalization. The probability of a law is just the probability of this assertion being correct. Thus, if the probability of the law "All *F*'s are *G*'s" is high, we run little risk in taking the generalization $(x)(Fx \supset Gx)$ as true and proceeding deductively from there.

One of the assumptions of the classical view — that what is at stake with a law is the probability of its extensional counterpart — led to the paradox of provisional acceptance. But even if we modify the classical view by substituting high instance confirmation of a law for high probability of its extensional counterpart, it still retains the following assumption:

> If the instance confirmation of a law L_1 is high and the extensional counterpart of another law L_2 is a logical consequence of that of L_1, then the predictive force of L_2 must also be high.

This assumption is false. The instance confirmation of a law may be as close to 1 as you please, and yet the predictive force of some consequence of it, equivalent of it, or of it itself, may be as close to 0 as you please. The classical account of the predictive function of laws rests on probabilistic fallacies!

In short, if, when we hold a law "All F's are G's" in high regard, it is to follow that upon learning Fa we are entitled to predict with high probability Ga, then that holding in regard cannot be consistently construed as attributing high instance confirmation to the law.

It can consistently be construed as attributing high qualified instance confirmation to the law. In fact, that construal yields just what is here required and nothing more.

The way in which the classical account of the predictive function of laws fails casts some light on a number of odd cases. Some of them are given below.

Necessarily vacuous antecedents and necessarily universal consequents. If a law saying "There aren't any F's" has high instance confirmation, then so does any law saying "All F's are G's" since $Pr(Fa \supset Ga) \geqq Pr(\sim Fa)$. Science doesn't work this way. For example, let Fx be:

> x is an event consisting of a closed system consisting of a reservoir and a particle undergoing a process whose net effect is to take heat from the reservoir and do work on the particle;

and let Gx be:

> x is an event consisting of a closed system undergoing a change in total energy.

Then, "All F's and G's" is the unlikely law that if an event were to violate the second law of phenomonological thermodynamics, it would violate conservation of energy. No scientist would have accepted this as a law, but would have chosen, if anything, its contrary, "All F's are non-G's" since the law of conservation of energy was supported by a mass of evidence independent of the evidence for the second law.

Examples of this kind can be multiplied. The antecedent of Newton's first law (Every body not acted on by any external force . . .) is rendered necessarily vacuous by his law of universal gravitation. Ideal gases are not only nowhere to be found, but are regarded as physically impossible, yet the ideal gas laws are not regarded as trivial consequences of this fact.

It is interesting to note that necessary universality of the consequent is *not* on a par with necessary vacuousness of the antecedent. In most cases (though not all) the inference from the law "Everything is a *G*" to "All *F*'s are *G*'s" appears to be quite in order. (Note that contraposition will transform a necessarily vacuous antecedent into a necessarily universal consequent, and conversely.)

How does qualified instance confirmation (QIC) explain what's going on? Let $Pr(Fa \,\&\, Ga) = a$; $Pr(Fa \,\&\, {\sim}Ga) = b$; $Pr({\sim}Fa \,\&\, Ga) = c$; $Pr({\sim}Fa \,\&\, {\sim}Ga) = d$. Then QIC "All *F*'s are *G*'s" is $a/(a + b)$. And interpreting "Nothing has *F*" as "All *U*'s have non-*F*" where *U* is a universal property, its qualified instance confirmation, its QIC, is $c + d$. The quantity $c + d$ may be as high as you please (short of 1) and yet $a/(a + b)$ may take on any probability value whatsoever. In our example, the remark that "the law of conservation of energy is supported by a mass of evidence independent of the evidence for the first law" is represented as $b/(a + b)$ being large. In terms of QIC, this explains the failure of "All *F*'s are *G*'s" and the success of "All *F*'s are non-*G*'s."

When we come to necessarily universal consequents things change. QIC "Everything is *G*" is $a + c$, which certainly has a more favorable connection to $a/(a + b)$ than did $c + d$. In fact if "*Fa*" and "*Ga*" are *independent*, then $a/(a + b) = a + c$. Things *can* still go wrong, however, if *F* is *unfavorable* to *G*. Let *Bx* be:

> x is an event consisting of a closed system consisting of a reservoir and a (Brownian) particle undergoing an acceleration.

When it was still reasonable to accord high QIC to "Everything is a non-*F*" it would not have been reasonable to accord high QIC to the (thought experiment) law "Every *B* is a non-*F*."

A variant of Hempel's paradox.[30] Consider the law "All non-white things are non-swans." It is plausible to assign it high instance confirmation on a sufficiently long string of observations of non-white non-swans (red shoes, yellow butterflies, etc.). Its contrapositive "All swans are white" must have equally high instance confirmation on this evidence, although the evidence appears to have no *relevance* to this law.

Viewing the situation in terms of qualified instance confirmation brings some interesting facts to light. If our evidence consisted solely of observation of an enormously long string of non-white non-swans, it

[30] Compare Hempel (1948).

seems reasonable that our probabilities regarding an unexamined individual, a, might look like this:

$$\begin{array}{ll} Sa \,\&\, Wa & \epsilon \\ Sa \,\&\, {\sim}Wa & \epsilon \\ {\sim}Sa \,\&\, Wa & \epsilon \\ {\sim}Sa \,\&\, {\sim}Wa & 1-3\epsilon \end{array}$$

Such a probability assignment would indeed yield high QIC for "All non-swans are non-white" but would make QIC "All swans are white" only 1/2. This fact may well make us queasy about saying that "All swans are white" is well confirmed, for it lacks the predictive force that we expect from a well-confirmed law.

Indeed, suppose that under such circumstances someone finally sighted a swan (at dusk) but was unable to determine its color. Our body of evidence now looks like this:

$${\sim}Wa \,\&\, {\sim}Sa \,\&\, {\sim}Wb \,\&\, {\sim}Sb \,\&\, \ldots \,\&\, Sz.$$

If we change our degrees of belief by conditionalization, both Sz & Wz and Sz & ${\sim}Wz$ which previously had probability ϵ now receive probability 1/2. *But* Sz & ${\sim}Wz$ *is incompatible with the law in question! Spotting that swan disconfirmed the law even though we couldn't see its color.* It is a paradoxical sort of confirmation for a law that yields the result that the sort of conditions which would normally set up the predictive inference automatically disconfirm the law. Such is, however, the case whenever the QIC of a law is low.

A variant of Goodman's paradox:[31] Consider the law (L_1) "All emerald stages[32] are either green and before 2000 A.D. or blue and at or after 2000 A.D." Observation of a long enough string of emerald stages before the year 2000 might be thought to give high instance confirmation to (L_0) "All emerald stages are green and before 2000" and thus to (L_1) and thus to (L_2) "All emerald stages after the year 2000 are blue."

Now L_0's positive instances support neither it nor L_1 in the way assumed. Goodman's tough question is "Why don't they?" and I have no answer for it here. What I would like to point out is that even if L_0 had high *qualified* instance confirmation, L_2 need not have high

[31] Compare Goodman (1955).
[32] Time slices of emeralds.

qualified instance confirmation. The move from L_0 to L_1 does preserve QIC, but the move from L_1 to L_2 need not.

6. The Claims of Deduction

Given the facts discussed in the last section, why not take QIC itself as the prime measure of the acceptability of scientific laws? The main reason against such a radical revision was clearly stated in Hempel's "Studies in the Logic of Confirmation."[33] However essential high predictive force may be, it is not in itself sufficient to account for the role that laws play in science. In scientific practice, laws are not distinguished from their extensional counterparts. Inferences from law to law are freely made in accordance with rules of deduction for their extensional counterparts. Short of challenging fundamental scientific practice, any acceptable account of laws must provide a justification for such inferences.

Doesn't the failure of these inferences put qualified instance confirmation on a collision course with scientific practice? Only if we take high QIC as both necessary and *sufficient* for holding a law in high regard. Since the type of prediction to which QIC is relevant is *important* in science, high QIC should be necessary. Since inferences which need not preserve high QIC are important to science, high QIC is not sufficient to qualify a statement to function as a premise for such an inference.

We now have some bounds on reasonable requirements for a law to be held in high regard. They should not be as stringent as high probability of its extensional counterpart, on pain of the paradox of provisional acceptance. They should guarantee high qualified instance confirmation. They should be preserved by all or most customary scientific inferences. Are there sets of requirements that fill this bill?

7. Prediction and Deduction Reconciled

For any set of elementary monadic predicates, a complex predicate which is a conjunction containing for each elementary predicate either it or its negation (but not both) and nothing else is called a *Q-predicate* for that set. (If the set is the set of predicates for a language then it is a Q-predicate for the language.) Thus, let the set be:

[33] Hempel (1948).

Sx: x is a swan.
Wx: x is white.

The Q-predicates are as follows:

Q_1: $Sx \,\&\, Wx$
Q_2: $Sx \,\&\, {\sim}Wx$
Q_3: ${\sim}Sx \,\&\, Wx$
Q_4: ${\sim}Sx \,\&\, {\sim}Wx$.

Now consider the law:

L: $(x)\ Sx \rightarrow Wx$.

Let us assume that substitution of L-equivalents *within* the antecedent a consequent leaves the law essentially the same. (It can be shown that such substitutions leave the predictive force of the law unchanged.) Then the laws that can be written *in terms of S and W* and whose extensional counterparts are equivalent to that of L are limited essentially to the following, written in terms of their Q-predicates:

$*L_1$: $(x)\,[Q_1x \vee Q_2x \vee Q_3x \vee Q_4x \rightarrow Q_1x \vee Q_3x \vee Q_4x]$
$*L_2$: $(x)\,[Q_1x \vee Q_2x \vee Q_3x \rightarrow Q_1x \vee Q_3x]$
L_3: $(x)\,[Q_1x \vee Q_2x \vee Q_3x \rightarrow Q_1x \vee Q_3x \vee Q_4x]$
$*L_4$: $(x)\,[Q_1x \vee Q_2x \vee Q_4x \rightarrow Q_1x \vee Q_4x]$
L_5: $(x)\,[Q_1x \vee Q_2x \vee Q_4x \rightarrow Q_1x \vee Q_3x \vee Q_4x]$
$*L_6$: $(x)\,[Q_2x \vee Q_3x \vee Q_4x \rightarrow Q_3x \vee Q_4x]$
L_7: $(x)\,[Q_2x \vee Q_3x \vee Q_4x \rightarrow Q_3x \vee Q_4x \vee Q_1x]$
$*L_8$: $(x)\,[Q_1x \vee Q_2x \rightarrow Q_1x]$
L_9: $(x)\,[Q_1x \vee Q_2x \rightarrow Q_1x \vee Q_3x]$
L_{10}: $(x)\,[Q_1x \vee Q_2x \rightarrow Q_1x \vee Q_4x]$
L_{11}: $(x)\,[Q_1x \vee Q_2x \rightarrow Q_1x \vee Q_3x \vee Q_4x]$
$*L_{12}$: $(x)\,[Q_2x \vee Q_3x \rightarrow Q_3x]$
L_{13}: $(x)\,[Q_2x \vee Q_3x \rightarrow Q_3x \vee Q_1x]$
L_{14}: $(x)\,[Q_2x \vee Q_3x \rightarrow Q_3x \vee Q_4x]$
L_{15}: $(x)\,[Q_2x \vee Q_3x \rightarrow Q_3x \vee Q_1x \vee Q_4x]$
$*L_{16}$: $(x)\,[Q_2x \vee Q_4x \rightarrow Q_4x]$
L_{17}: $(x)\,[Q_2x \vee Q_4x \rightarrow Q_4x \vee Q_1x]$
L_{18}: $(x)\,[Q_2x \vee Q_4x \rightarrow Q_4x \vee Q_3x]$
L_{19}: $(x)\,[Q_2x \vee Q_4x \rightarrow Q_4x \vee Q_1x \vee Q_3x]$
$*L_{20}$: $(x)\,[Q_2x \rightarrow Q_1x]$

L_{21}: $(x)[Q_2x \rightarrow Q_1x \vee Q_3x]$
L_{22}: $(x)[Q_2x \rightarrow Q_1x \vee Q_4x]$
L_{23}: $(x)[Q_2x \rightarrow Q_1x \vee Q_3x \vee Q_4x]$
L_{24}: $(x)[Q_2x \rightarrow Q_3x]$
L_{25}: $(x)[Q_2x \rightarrow Q_3x \vee Q_4x]$
L_{26}: $(x)[Q_2x \rightarrow Q_4x]$
L_{27}: $(x)[Q_2x \rightarrow Q_1x \,\&\, Q_2x]$.

Now, note that the predictive force of the following groups of laws must be the same: (L_2L_3) (L_4L_5) (L_6L_7) $(L_8L_9L_{10}L_{11})$ $(L_{12}L_{13}L_{14}L_{15})$ $(L_{16}L_{17}L_{18}L_{19})$ $(L_{20\text{-}27})$. This is because their antecedents are *L*-equivalent and, although their consequents are not *L*-equivalent, the conjunctions of consequent and antecedent are *L*-equivalent. That is, their reference classes do not coincide absolutely, but coincide within their attribute classes. Let us treat such laws as equivalent. Then the number of classes of equivalent laws reduces to eight, those whose simplest members are starred. "All swans are white" appears as L_8 and "All non-white things are non-swans" appears as L_{16}. L_{20} and its equivalents are in a rather strange form for predictive instruments, their consequents being inconsistent with their antecedents, giving a QIC of absolutely zero. What is required to assure high QIC to L_8, L_{12}, and L_{16} is that the probability that an unexamined individual is a falsifier $[Pr\ Q_2\ (a)]$ is small compared with the probabilities of each of the other *Q*-properties. Happily this condition assures that the QIC of every other member of the list (excepting L_{20} and its equivalents) is also high! Thus, any quantity which must be less than or equal to QIC (L_8), QIC (L_{12}), QIC (L_{16}) is such that the predictive force of any member of the group whose consequent is consistent with its antecedent must be at least as high as it.

Unfortunately, this no longer holds good when we consider laws with equivalent extensional generalizations that we can write with the addition of new primitive predicates. For instance assume that QIC $(L_8) = [Pr(Q_1a)]/[Pr(Q_1a) + Pr(Q_2a)]$ is high and that H is another primitive predicate. Expending L_5 in terms of it we get $(x)\ [(Q_1x \,\&\, Hx) \vee (Q_1x \,\&\, {\sim} Hx) \vee (Q_2x \,\&\, Hx) \vee (Q_2x \,\&\, {\sim}Hx) \rightarrow (Q_1x \,\&\, Hx) \vee (Q_1x \,\&\, {\sim}Hx)]$ and

$$\text{QIC}\ (L_8) = \frac{Pr(Q_1a \,\&\, Ha) + Pr(Q_1a \,\&\, {\sim}Ha)}{Pr(Q_1a \,\&\, Ha + Pr(Q_1a \,\&\, {\sim}Ha) + Pr(Q_2a \,\&\, Ha) + Pr(Q_2a \,\&\, {\sim}Ha)}$$

but that this quantity is high in no way guarantees that Pre $(x)[((Q_1x \,\&\, Hx) \vee Q_2x \rightarrow Q_1x \,\&\, Hx]$ will be high, for $Pr(Q_1a \,\&\, Ha)$ might be as small as you please with the difference being made up by $Pr(Q_1a \,\&\, {\sim}Ha)$.

The measure of "convertibility" of laws that we have gained must then be relativized to some finite set of predicates.

The situation, in general, can be summed up as follows:

Let L be a law, S be a finite set of predicates, $\{Q_i\}$ be the Q-predicates constructed out of S, and $Q_j \ldots Q_{j+n}$ be the Q-predicates the possession of which would falsify L. A measure of *convertible confirmation* of L with respect to S $\{CC_s(L)\}$ is a quantity which is always less than or equal to the minimum of:

$$\frac{Pr(Q_i(a))}{Pr(Q_i(a)) + Pr(Q_j(a)) + \ldots + Pr(Q_{j+n}(a))}$$

for all i different from $j \ldots j+n$.[34] Call any law an instance of whose consequent is logically incompatible with the corresponding instance of its antecedent *Pickwickian*. Call any law whose extensional counterpart is logically equivalent to that of L a *quasi equivalent of L*.

Then: If L_1 and L_2 are quasi equivalents containing only predicates in S then $CC_s(L_1) = CC_s(L_2)$ and if L_2 is non-Pickwickian QIC $(L_2) \geqq CC_s(L_1)$.

Looking back at the familiar cases of swans and ravens, we find that if we do *not* suppress background evidence, typical cases where we regard the law as well confirmed are cases where its convertible confirmation is high over a fairly rich set of predicates. And this explains why we do not typically fall into paradox when we treat quasi equivalents of a law as genuine equivalents. Put in the following way, the requirement that a well-confirmed law have high convertible confirmation seems almost trivially correct. For "All swans are white" to be well confirmed the probability that the something is not a non-white swan must be low not only given that it is something or other; but also given that it is a swan; given that it is a non-white thing; given that it is a fat swan; given that it is a non-white bird; etc.

Our attempts to recapture the equivalents have met with a certain degree of success. What about consequences? Let us call L_2 a *quasi*

[34] Thus the minimum over i of these quotients is itself a measure of convertible confirmation. So is the product over i.

consequence of L_1 if its extensional counterpart is a consequence of that of L_1. A law, with an antecedent and consequent written as disjuncts of Q-properties, would look like this:

$$(x)[Q_ix \vee \ldots \vee Q_{i+m}x \vee \ldots \vee Q_jx \vee \ldots \vee Q_{j+n}x \rightarrow Q_ix \vee \ldots \vee Q_{i+m}x \vee \ldots \vee Q_kx \vee \ldots \vee Q_{k+o}x]$$

where $Q_j \ldots Q_{j+n}$ are the falsifier properties. $\{Q_i \ldots Q_{i+m}\}$ must contain at least one property if the law is to be non-Pickwickian. $\{Q_k \ldots Q_{k+o}\}$ may be empty and if it is not the law it is equivalent to that which results when it is.

L_2 is a quasi consequence of L_1 if L_2's falsifier properties are a subset of those of L_1. It follows that if L_1 has high convertible confirmation L_2's falsifier properties must have low probability relative to all the Q's which are not falsifiers of L_1. But it does *not* follow that L_2's falsifiers must have high probability relative to all the Q's which are not falsifiers of L_2. Thus quasi consequence need not preserve convertible confirmation. For example, let there be two primitive predicates in S and thus four Q-predicates.

$$L_1\colon (x)[Q_1x \vee Q_2x \vee Q_3x \rightarrow Q_1x]$$
$$L_2\colon (x)[Q_2x \vee Q_3x \rightarrow Q_2x]$$
$$Pr[Q_1a] = .5 - \epsilon$$
$$Pr[Q_2a] = \epsilon$$
$$Pr[Q_3a] = \epsilon$$
$$Pr[Q_4a] = .5 - \epsilon.$$

$CC_s(L_1)$ is high but $CC_s(L_2)$ and QIC (L_2) are low. This sort of failure, however, does not contravene the actual use of deduction in scientific practice. The antecedent of L_2 cannot be instantiated unless L_1 is falsified. Thus, the only conditions under which L_2 would be of any use as a predictive instrument are conditions under which L_1 would no longer be available to serve as a premise.

This suggests that we define a relative sense of convertible confirmation as follows: Let F be a set of Q-predicates that we treat as immune to instantiation. Let $Q_j \ldots Q_{j+n}$ be the falsifier predicates for L. Then $CC_{sf}(L)$ is a quantity which is always less than or equal to the minimum of:

$$\frac{Pr(Q_i(a))}{Pr(Q_i(a)) + Pr(Q_j(a)) + \ldots + Pr(Q_{j+n}(a))}$$

for all Q_i which are neither falsifier predicates for L nor members of F. Now we can say that if L_2 is a quasi consequence of L_1 and F is the set of falsifier predicates of L_1, then $CC_{sf}(L_2) \geqq CC_s(L_1)$.

But what is really important is the upshot of this for the predictive force of L_2 and its equivalents. Let us say that a law is *Pickwickian with respect to F* if the instantiation of both its antecedent and consequent is impossible without instantiating some member of F. And a law, L_1, is Pickwickian with respect to another L_1 just in case it is Pickwickian with respect to the set of falsifiers of L_2. Then if $CC_{sf}(L_2)$ is high and L_3 is a quasi equivalent of L_2 which is not Pickwickian with respect to F, then $QIC(L_3)$ is at least as high. Consequently, *if L_2 is a quasi consequence of L_1 written in terms of S which is non-Pickwickian with respect to L_1, then $QIC(L_2) \geqq CC_s(L_1)$*. Finally, we want to talk about consequences of sets of laws rather than just single laws. Let us define a measure of convertible confirmation of a *network of laws* as a quantity always less than or equal to

$$\operatorname{Min}_{i} \frac{Pr(Q_i(a))}{Pr(Q_i(a)) + Pr(Q_j(a)) + \ldots + Pr(Q_{j+n}(a))} \quad i \neq j \ldots i \neq j+n$$

where $Q_j \ldots Q_{j+n}$ here represent the Q-predicates the instantiation of which would falsify at least one member of the network. The term *network* is used to emphasize the interdependent nature of the convertible confirmation. For the convertible confirmation of a network of laws may be high while the convertible confirmation of its members is not. For example:

L_1: $(x)[Q_1s \vee Q_2x \rightarrow Q_1x]$
L_2: $(x)[Q_1x \vee Q_3x \rightarrow Q_1x]$
$Pr\, Q_1(a) = .5 - \epsilon$
$Pr\, Q_2(a) = \epsilon$
$Pr\, Q_3(a) = \epsilon$
$Pr\, Q_4(a) = .5 - \epsilon.$

The convertible confirmation of the network $\{L_1,L_2\}$ is high. $CC_s \{L_1L_2\} = [(.5 - \epsilon)/(.5 - \epsilon)]^2$. But $CC_s (L_1) = CC_s (L_2) = 1/2\, [(.5 - \epsilon)/(.5)]^2$. The reason is, of course, that the only conditional probabilities that we required to be high for CC of the network to be high are conditions which do not falsify any law in the network. Such exemptions are not available for laws standing alone. So if F is the class of falsifiers for a network, whenever CC_s of the network is high, CC_{sf} of each member of the network is high. Notice that networks of laws may, for this reason, exhibit a hierarchical organization. If CC of a subnetwork is as high as CC of the network, this means that the predictive force of the subnetwork will survive intact upon the discovery of instances which falsify other members of the network. It is interesting that in this context convertible confirmation becomes a measure of *independence*.

The situation with respect to quasi consequences of a network of laws and the relation of being Pickwickian with respect to a network of laws is a straightforward generalization of the foregoing. If CC_s of a network is high and F is the set of falsifiers for the network then CC_{sf} of each quasi consequence of the network written in terms of predicates in S is at least as high. And each of those quasi consequences which is not Pickwickian with respect to the network has high qualified instance confirmation.

The concept of a network of laws also allows us to look at the question of quasi consequences in the following rather attractive way:

I: Let N be a network of laws; L be a quasi consequence of N written in terms of predicates of S; and N_L the network formed by adding L to N. Then

$$CC_s\,(N_L) = CC_s(N).$$

II. Let N be a network of laws and L be a member of N (written in terms of predicates of S) which is not Pickwickian with respect to N. Then

$$QIC(L) \geqq CC_s(N).$$

We traced many of the paradoxes of section three to a failure of predictive force. But the quasi consequence relation proved so poor at preserving predictive force that it seemed quite hard to make any sense at all of the use of deductive technique in science. I and II above reestablish a broad scope for nonparadoxical deductive inference. Paradoxical inferences arise when CC of the premises is not high (though some other supposedly relevant measure may be) or when the conclusion is Pickwickian with respect to the relevant network.

8. Law, Accident, and Observation

Any quantity which is a measure of convertible confirmation has the virtues exhibited in the last two sections. But these virtues, by themselves, are not enough. Laws pose additional problems with which we must deal. A fundamental one is the distinction between law and accident. I wish to introduce it here, because the solution seems almost within our grasp with methods already developed.

The paradox of provisional acceptance showed us that the law "All *F*'s are *G*'s" can be held in high regard while $Pr\ (x)(Fx \supset Gx)$ is low. The opposite situation is also possible. Consider Goodman's famous example: "All coins in my pocket on V.E. day 1947 are silver." We can imagine events which would lend credence to the law (i.e., dropping copper pennies into the pocket and finding upon removal that they had been transmuted to silver, etc.) but we can also imagine less bizarre events which would lend credence to the extensional generalization but not the law (looking in my pocket on V.E. day, etc.). Thus, the nomic force of a generalization is determined not by its syntax or semantics, *but by the character of its evidential support.*

It is easy to think of accidental generalizations of spatiotemporally limited scope and tempting to think that there might be no others. In order to keep ourselves honest, we should also bear in mind Hempel's example: "All bodies of pure gold have a mass of less than 100,000 kilograms." (A little imagination will show that here too there are logically possible evidential situations which would lead us to regard this generalization as a law rather than as a global accident.) — So much for the problem.

Now remember that we defined QIC of "All *F*'s are *G*'s" as $Pr(G\alpha$ given $F\alpha)$ where 'α' is a "new" individual constant *not mentioned in our evidence.* But it might be difficult ultimately to maintain an epistemological picture where evidence comes as a neat list of sentences. This suggests that we might consider redefining QIC as the minimum value (or greatest lower bound) of $Pr(G\alpha$ given $F\alpha)$ for all individual constants, α, in the language, and making the coordinate redefinition of a measure of convertible confirmation. Such a redefinition has two pleasant consequences. First, the new qualified instance confirmation (QIC*) of a law is less than or equal to the probability that α is a counterinstance to that law, for each α:

$$\text{QIC* “All } F\text{'s are } G\text{'s”} \leq \text{Min}_\alpha\ 1 - Pr(F\alpha\ \&\ {\sim}G\alpha).$$

Second, it appears to weed out just those accidental generalizations which have been bothering us.

Let us look at our two examples. It may be very probable that every coin in my pocket on V.E. day was silver. I remember turning my pocket inside out and looking very carefully. Yet consider the coin I got in change for my ninety-nine–cent lunch. I looked at it carefully too, and a coppery A. Lincoln looked back. It is clearly quite unlikely that this coin was in my pocket on V.E. day. But that small unlikelihood is the sum of the probabilities that it was in my pocket and silver and that it was in my pocket and not silver. Their *relative* size is what is important in determining the conditional probability that the coin in question was silver *given* that it was in my pocket on V.E. day. It seems clear that this probability must be low; that should I suddenly become convinced that I was mistaken and that this coin really was in my pocket I should revise my belief about the composition of coins in my pocket rather more than my belief about the composition of this coin. On the other hand, in the bizarre "transmutation" case, where Goodman's generalization *does* have nomic force, it seems clear that this conditional probability *is* high.

In general, it seems that this will work for any accidental conditional about a finite class. Assume that our observations have rendered it likely that there are only a finite number of F's, $a_1, a_2, \ldots, a_n$ and that each of these has G. What is $Pr(Ga_{n+1}$ given $Fa_{n+1})$ where a_{n+1} is known to be distinct from $a_1 \ldots a_n$? Thus we have as mutually exclusive and exhaustive categories:

a. $(x)(Fx \supset x = a_1 \vee \ldots \vee x = a_n)\ \&\ Ga\ \&\ \ldots\ \&\ Ga_n$.
b. $(x)(Fx \supset x = a_1 \vee \ldots \vee x = a_n)\ \&\ (\exists x)({\sim}Gx)$.
c. $Fa_{n+1}\ \&\ Ga_{n+1}\ \&\ (x)(Fx \supset Gx)$.
d. $Fa_{n+1}\ \&\ Ga_{n+1}\ \&\ (\exists x)(Fx\ \&\ {\sim}Gx)$.
e. $Fa_{n+1}\ \&\ {\sim}Ga_{n+1}$.
f. None of the above and $(x)(Fx \supset Gx)$.
g. None of the above.

$$Pr\,[(x)(Fx \supset Gx)] = Pr\ (a) + Pr\ (c) + Pr\ (f).$$

$$Pr(Ga_{n+1} \text{ given } Fa_{n+1}) = \frac{Pr\ (c) + Pr\ (d)}{Pr\ (c) + Pr\ (d) + Pr\ (e)}.$$

If the generalization is "accidental" its probability will be high by virtue of *Pr* (a) being high and the conditional probability may fail to be high. If it is "lawlike" *Pr* (c) should be high *relative* to *Pr* (d) and *Pr* (e), and *Pr* (f) should be high relative to *Pr* (e).

Indeed, we seem to have equally good luck with universal accidents. We may assume that the old instance and qualified instance confirmations of "All bodies of pure gold have a mass of less than 100,000 kilograms" are high, but the probability that Ganymede has a mass of less than 100,000 kilos, *given* that Ganymede is pure gold, is not high. Thus, QIC* and coordinate measures of convertible confirmation will be appropriately low.

Unfortunately, in one peculiar sort of case, QIC* appears to disqualify perfectly decent laws as well. Suppose that we are in a position to accord high CC* to the law "All swans are white" and that we then observe a black bird without being able to determine whether it is a swan or not. If we believe the law, we shall of course *believe* that this bird is not a swan, but what we are interested in here is the probability that it is white *given* that it is a swan, and there is good reason to believe that now *that* conditional probability must be low. For if we subsequently determined that the bird *was* a swan, we would typically assign high probability to the proposition that it was a *black* swan and a disconfirmer of the law, rather than taking this new observation as grounds for repudiating our previous conviction that it was a black bird. This means that if we change our degrees of belief by conditionalization, *Pr*(White a given Swan a) *must have been low*. Thus, an observation that determines that a bird is black without determining that it is a swan or not renders QIC* and CC* of "All swans are white" low, although the law should certainly retain its viability in these circumstances.

Example:

		a is black	a is a swan
	Pr_1	Pr_2	Pr_3
$Sa \,\&\, Wa$	.33	.01	.36
$Sa \,\&\, {\sim}Wa$	.01	.02	.66
${\sim}Sa \,\&\, Wa$	.33	.01	.0002
${\sim}Sa \,\&\, {\sim}Wa$	.33	.96	.0198

If we choose more powerful laws as examples there is more of a tendency to retain the law at the expense of the earlier observation *but this tend-*

ency cannot be overriding lest it render the law immune to instantial disconfirmation.

It seems that we need to exempt cases where a has been examined for W but not for S or for S but not for W from those over which we take CC^*. But to state such an exemption we need to presuppose that neat distinction between knowledge by observation and knowledge by inference which is so much in doubt.

9. Resiliency

In connection with various problems, the distinction has been made between intensional and extensional evidence for a disjunction. The idea is that evidence for one of the disjuncts will automatically count as *extensional* evidence for the disjunction but not necessarily as intensional evidence. Intensional evidence should be evidence for the disjunction as a unified, connected whole — so that if one disjunct were cast into doubt, the other would tend to take up the slack.

This image naturally suggests:

$$I(p \vee q) = \mathrm{Min}\{Pr(p \text{ given } {\sim}q), Pr(q \text{ given } {\sim}p)\}$$

as a measure of the strength of the intensional evidence for $p \vee q$.

Since any truth function can be put in disjunctive normal form, we have a natural extension of this idea for all truth functions. Let $S = \{p_1 \ldots p_n\}$ a set of atomic sentences including the atomic sentences of q and let $\{S_i\}$ be the n disjuncts when q is written in disjunctive normal form in terms of the members of S. Then the *resiliency* of q over S is as follows:

$$\begin{aligned} Rs(q) = \mathrm{Min}\{&Pr\,[S_1 \text{ given } {\sim}(S_2 \vee S_3 \vee \ldots \vee S_n)] \\ &Pr\,[S_2 \text{ given } {\sim}(S_1 \vee S_3 \vee \ldots \vee S_n)] \\ &\quad\vdots \\ &Pr\,[S_n \text{ given } {\sim}(S_1 \vee S_2 \vee \ldots S_{n-1})]\}.\end{aligned}$$ [35]

Since $Pr\,[S_1 \text{ given } {\sim}(S_2 \vee S_3 \vee \ldots \vee S_n)]$ is just $Pr\,[q \text{ given } {\sim}(S_2 \vee S_3 \vee \ldots \vee S_n)]$ and since the members of $\{S_i\}$ are just the state-descrip-

[35] If we consider only belief states where just tautologies get probability 1 and just contradictions get probability zero, then these conditional probabilities will always be well defined. Such a restriction can be independently motivated.

tions over S which are compatible with q we might further generalize the concept of resiliency to any set of atomic sentences.

Let S be any set of atomic sentences, $\{S_i\}$ be the set of n state-descriptions in terms of members of S compatible with q, and let S_{-j} be the disjunction of all members of $\{S_i\}$ except S_j. Then

$$R_s\ (q) = \underset{j=1,\ n}{\mathrm{Min}}\ Pr\ [q \text{ given } S_{-j}].$$

If S is disjoint from the set of atomic sentences of q, then $Rs(q)$ is just the minimum of the conditional probabilities given each state-description over S.

The generalization is in a sense justified by the following fact: If r is compatible with q and S contains all the atomic sentences occurring in r, then $Pr\ (q \text{ given } r) \leq R_s\ (q)$.

There appear to be just two legitimate sources of resiliency of belief in a contingent statement; the first being habits stemming from recurrent patterns in our experience, the second being from the vividness of current observation.

With regard to the first source, I wish to make two connections between resiliency and the preceding discussions. The first is the obvious connection with convertible confirmation. The resiliency of the *material* conditional for an unexamined individual is a measure of convertible confirmation:

$$R_T\ (Fa \supset Ga) = CC_S \text{ "All F's are G's"}$$

(Where $\lambda \in S$ iff $\lambda\, a \in T$).

The second is the connection with lawlikeness itself. Suppose we expand our language by the addition of one new individual constant, δ. Then the discussion of the last section suggested that lawlikeness has to do with the resiliency of "$F\delta \supset G\delta$" over ('$\delta = a_1$', '$\delta = a_2$' . . .) for each individual constant in the language — barring certain exceptional cases.

This brings us back to our outstanding problem, which consists of just those exceptional cases. It may now be viewed as a collision between resiliency owing to the first source (patterns in experience) and resiliency owing to the second source (current observation).

High resiliency is a strong requirement, and it is easy for conflicts in resiliency to arise. For instance, if p and q are atomic sentences it is impossible for $R_{\{p,q\}}(p)$ and $R_{\{p,q\}}(p \supset q)$ both to be high. For the first to be high, Pr (p given $\sim q$) must be high. For the second to be high, Pr ($\sim p$ given $\sim q$) must be high. Whichever statement wins in the crunch (autonomous increase in probability of $\sim q$) gets the high resiliency.

We have already seen that where the contest is between resiliency of $p \supset q$ deriving from a law and resiliency of p (or of $\sim q$) deriving from observation, the resiliency deriving from observation must in some cases (good clear current observation) be allowed to win if we are to account for instantial disconfirmation of laws (although it is worth noticing that a highly confirmed physical law may be so resilient that *repeated* counterobservations are required to disconfirm it).

But although p and $p \supset q$ cannot both be resilient (over $\{p,q\}$) their conjunction can, since $R_s(p \& (p \supset q)) = R_s(p \& q)$ and $R_{\{p,q\}}(p \& q) = Pr(p \& q)$. Thus $R_{\{Fa,\,Ga\}}[(Fa \supset Ga) \& Fa]$ can be as high as can $R_{\{Fa,Ga\}}[(Fa \supset Ga) \& \sim Ga]$ but $R_{\{Fa,Ga\}}[(Fa \supset Ga) \& Fa \& \sim Ga]$ is, of course, zero. The preceding statement holds good also if resiliency is taken over a broader basis.

This suggests that the problem has been put wrong, that what receives high confirmation is not a law nor a network of laws but a network of both laws and observation statements. The smaller the subnetworks containing a law which retain resiliency, the more central the law is to our system of beliefs. The number of statements in such a system which owe their resiliency directly to observation will be small, for only current observation in favorable conditions will provide the required resiliency.

The idea of a *network* which emerged in the context of convertible confirmation and quasi consequence now extended to include nonlaws through resiliency thus solves our outstanding problem. It is interesting that the logic of laws appears to *force* this wholistic viewpoint upon us.

10. Conclusion

This paper has taken only the first steps toward a philosophical reduction of laws of nature. But if these steps are in the right direction, we can already draw some nontrivial philosophical conclusions. The logic of laws is, in the most general case, a logic of networks. Our most useful measures of virtue for laws and networks of laws, *resiliencies*, are functions of our probability assignments on an extensional language although they are not themselves probabilities.[36] The status of a law within a network is determined by the resiliency of subnetworks which contain it. Resiliency accounts both for the probabilistic presuppositions of deductive science and for the nomic force of laws. Resiliency is a kind of modal notion. If a sentence is resilient over *S* in a belief state, it will retain high probability in any belief state accessible from the first by conditionalization[37] on any sentence which is consistent with it and written in terms of *S*, and conversely. Thus the resiliency of *S* in a given belief state may be thought of in two ways — as a function of the probabilities that *S* has in certain related belief states, or as a function of the probabilities of other statements in the original belief state. It is thus a sort of modality without metaphysics, reducing nomic necessity from an ontological to an inferential status.

[36] Resiliency is not additive.

[37] Or by a weighted average of conditionalizations on p and $\sim$p à la Jeffrey. See Jeffrey (1965), ch. 11. This sort of transformation is to be preferred since it avoids introducing zeros and ones into the probability distribution.

REFERENCES

Adams, E. W. (1965). "The Logic of Conditionals," *Inquiry*, vol. 8, pp. 166–97.

Adams, E. W. (1966). "Probability and the Logic of Conditionals," in J. Hintikka and P. Suppes, eds., *Aspects of Inductive Logic*. Amsterdam: North Holland. Pp. 265–316.

Beth, E. W. (1966). *The Foundations of Mathematics*. New York: Harper Torchbooks.

Bohm, D. (1957). *Causality and Chance in Modern Physics*. New York: Van Nostrand.

Carnap, R. (1937a). *Logical Syntax at Language*. New York: Harcourt, Brace.

Carnap, R. (1937b). *Introduction to Semantics*. Cambridge, Mass.: Harvard University Press.

Carnap, R. (1939). "Foundations of Logic and Mathematics," in *International Encyclopedia* of Unified Science, vol. 1. Chicago: University of Chicago Press.

Carnap, R. (1950). *Logical Foundations of Probability*. Chicago: University of Chicago Press.

Carnap, R. (1956). *Meaning and Necessity*, enlarged ed. Chicago: University of Chicago Press.

Chisholm, R. M. (1946). "The Contrary-to-Fact Conditional," *Mind*, vol. 55, pp. 289–307.

Ellis, B. (1968). "Probability Logic," mimeo.

Feyerabend, P. K. (1962). "Explanation, Reduction, and Empiricism," in H. Feigl and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science*, vol. 3. Minneapolis: University of Minnesota Press.

Goodman, N. (1955). *Fact, Fiction and Forecast*. Cambridge, Mass.: Harvard University Press.

Hempel, C. G. (1948). "Studies in the Logic of Confirmation," Mind, vol. 54, pp. 1–29, 97–121.

Hempel, C. G. (1958). "The Theoretician's Dilemma," in H. Feigl, M. Scriven, and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science*, vol. 2. Minneapolis: University of Minnesota Press.

Hempel, C. G. (1966). *Philosophy of Natural Science*. Englewood Cliffs, N.J.: Prentice-Hall.

Hempel, C. G. (1968). "On a Claim by Skyrms Concerning Lawlikeness and Confirmation," *Philosophy of Science*, vol. 35, pp. 274–78.

Jeffrey, R. C. (1964a). "If," abstract in *Journal of Philosophy*, vol. 61, pp. 702–3.

Jeffrey, R. C. (1964b). "Review of *Logic Methodology and Philosophy of Science*," *Journal of Philosophy*, vol. 61, pp. 79–88.

Jeffrey, R. C. (1965). *The Logic of Decision*. New York: McGraw-Hill.

Jeffrey, R. C. (1966). "Goodman's Query," *Journal of Philosophy*, vol. 62, pp. 281–83.

Jeffrey, R. C. (1968). "Probable Knowledge," in I. Lakatos, ed., *Inductive Logic*. Amsterdam: North Holland. Pp. 166–80.

Lewis, C. I. (1960). *A Survey of Symbolic Logic*. New York: Dover. Pp. 51–72.

Montague, R. (1968). "Pragmatics," in R. Klibansky, ed., *Contemporary Philosophy*. Florence: La Nuova Italia Editrice.

Montague, R. (1972). "Pragmatics and Intensional Logic," in D. Davidson and G. Harman, eds., *Semantics of Natural Language*. Dordrecht: Reidel.

Morris, C. W. (1939). "Foundations of the Theory of Signs," in *International Encyclopedia of Unified Science*, vol. 1. Chicago: University of Chicago Press.

Pap, A. (1958). "Disposition Concepts and Extensional Logic," in H. Feigl, M. Scriven, and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science*, vol. 2. Minneapolis: University of Minnesota Press. Pp. 196–224.

Ramsey, F. P. (1960). *The Foundations of Mathematics*. New York: Humanities Press.

Rescher, N. (1964). *Hypothetical Reasoning*. Amsterdam: North Holland. (See appendix 3.)

Sellars, W. (1958). "Counterfactuals, Dispositions, and Causal Modalities," in H. Feigl, M. Scriven, and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science*, vol. 2. Minneapolis: University of Minnesota Press. Pp. 225–308.

Sellars, W. (1962). "Time and the World Order," in H. Feigl and G. Maxwell, eds., *Minnesota Studies in the Philosophy of Science*, vol. 3. Minneapolis: University of Minnesota Press. Pp, 527–616.

Skyrms, B. (1964). "On Failing to Vindicate Induction," *Philosophy of Science*, vol. 31, no. 1, pp. 62–64.

Skyrms, B. (1966). "Nomological Necessity and the Paradoxes of Confirmation," *Philosophy of Science*, vol. 33, pp. 230–49.

Skyrms, B. (1967). "The Explication of 'x knows that p,'" *Journal of Philosophy*, vol. 64, pp. 373–89.

Stalnaker, R. C. (1968). "A Theory of Conditionals," in N. Rescher, ed., *Studies in Logical Theory*. Oxford: Oxford University Press. Pp. 98–112.

Stalnaker, R. C. (1970). "Probability and Conditionals," *Philosophy of Science*. vol. 37, pp. 64–80.

Stevenson, C. L. (1970). "If-ICULTIES," *Philosophy of Science*, vol. 37, pp. 27–49.