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**Introduction**

Even the most casual reader of the voluminous writings on induction can affirm that there seem to be two basic problems about induction. These are, it should be noted, philosophical problems about an ongoing satisfactory practice, rather than practical problems about how to engage in it. The statisticians have brilliantly succeeded in devising probability theory and the attendant techniques of projective statistics — which seem to have endless fruitful applications.

The problems of which I speak are these. First, there is the logical problem of somehow "formalizing" (in a sense to be made clear shortly) traditional inductive argument forms, to put inductive logic on a par with deductive logic. Look at any introductory logic text and you will find the notions of "inductive-strength" and "deductive-validity" defined as if they were roughly similar properties; as for instance:

An argument is *deductively-valid* if and only if it is impossible for the premises to be true and the conclusion false.

An argument is *inductively-strong* if and only if it is not impossible but is unlikely that the conclusion would be false given that the premises were true.

Of course, the student later discovers the scandalous truth: that while there are agreed- upon inference rules for assessing validity irrespective of context, no such neat inductive logic exists. Instead, he is faced with a mélange of topics (Mill's methods, enumerative induction, eliminative induction, analogy, theory confirmation, and so on), with the probability calculus—a deductive system—hovering mysteriously in the background.

The second problem of induction is not so much logical as skeptical: given an intuitively satisfactory inductive logic, how can we justify its use or prove its adequacy? This second problem is what is called (with a note of exasperation) "the" problem of induction, and is one of Hume's enduring (if not endearing) legacies.

The thesis of this paper and its sequel is that both problems are pseudo-problems which arise out of mistaken presuppositions.

In Part I (this paper), I argue that the logical problem of induction (devising a formal account of induction which accords with the obviously satisfactory probability calculus) is resolvable if and only if we give up the presupposition that inductive reasoning is to be analyzed by a statement (assertoric) logic similar to deductive logic, and come to view such reasoning as being dialectical in nature. (The way is then open to use formal dialectic to explicate such reasoning.)

The flow of my arguments is as follows: first, to argue that there are no other evidential relations besides (at most) deductive-validity and inductive-strength; second, to argue that all traditional inductive argument forms are inter-derivable, and hence all are equally open to the same criticisms; third, to argue that construing induction as traditionally done renders context- free formalization impossible; fourth, to argue that if we give up the presupposition that there is such an evidential relation as "inductive-strength," the way becomes open for such context-free treatment via formal dialectic.

I will take up the skeptical problem of induction in Part II (the sequel to this paper).

**Is There a Third Evidential Relation?**

The traditional view of inductive logic has held that there are exactly two of evidential relationships, two different ways in which the premises of an argument can support its conclusion: inductive-strength and deductive-validity. But some prominent thinkers, C.S. Peirce and N.R. Hanson in particular, have felt that we ought to distinguish yet another type of evidential relation, that of retroductive-strength. That is, these writers have urged that just as we distinguish between certainty and mere probability, we ought to distinguish between probability and mere plausibility.

Delineating such a third evidential relation was the heart of Peirce's (and later Hanson's) search for a "logic of discovery." Peirce characterized the process of discovering any hypothesis as being an inference of the form:

* 1. Surprising fact F is observed.
  2. If hypothesis H were true, then F would be a matter of course (would be best explained).

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So H is plausible (worthy of investigation).

But this sort of argument form (which Peirce called variously "hypothesis," "retroduction" and "abduction," but which we will call the inference to the best explanation—the "IBE") equally well represents hypothesis/theory confirmation, except for the temporal order in which data- gathering and hypothesis-conceiving take place. And isn't theory confirmation just part of inductive logic after all? We as logicians are interested in the evidential relation between the hypothesis and the evidence for it; and isn't that relation the same whether the data suggests the hypothesis or tests one already conceived?

Since there seems offhand to be no compelling reason to distinguish a third evidential relation, the burden of proof rests on the man who claims that there is some inherent evidential difference between retroductive logic and inductive logic/confirmation theory. Several differences have been mentioned which I shall now review. None of them seems convincing.

(a) It is often said that induction generalizes, whereas retroduction explains. Or as Peirce put it, induction is reasoning from particulars to generals, and retroduction is reasoning from effect to cause.[[1]](#footnote-1)

But this confuses "broad" and "narrow" induction. Broadly speaking, an inductive argument is one in which the premises furnish good but not logically conclusive evidence for the conclusion. There are many types of inductive (in the broad sense) argument: analogy, the paradigm confirmation argument, induction by simple enumeration, eliminative induction, and so on. By narrow induction we mean generalization, induction by simple enumeration. Granted the IBE doesn't resemble enumerative induction—although we shall see in Section Three that the two argument forms are in fact related—still, the IBE is identical to the paradigm confirmation argument, which is certainly inductive in the broad sense.

(b) It is often said that retroduction goes from the observable to the unobservable. Peirce spoke this way when he said that induction infers phenomena similar to cases that have been observed, while retroduction is the supposition of something of a kind from what we have directed observed, and frequently something unobservable.[[2]](#footnote-2)

This sort of claim again shows confusion between broad and narrow induction, for (to reiterate) the paradigm confirmation argument is a perfectly good broad inductive argument, and it certainly applies to theories involving unobservables. Moreover, consider narrow induction, simplistically put as:

All known A's are B

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All A's are B

Do we need to stipulate that A's be observable, or that B be an observable trait? Surely not. All electrons ever examined have had spin ½, so, all electrons have spin ½.

(c) It is often said (in a manner similar to (b) above) that retroduction involves the introduction of new concepts, induction not.

This claim is only plausible if you view retroduction as being an inference from the data to the hypothesis. But the inference is really from the data plus the recognition that that hypothesis would explain the data, to confidence in the hypothesis, if it is an inference at all. (It isn't.)

It is, of course, quite true that inductive logic cannot mirror conceptual evolution. But neither can the IBE or any retroductive logic. No assertoric logic can analyze learning processes adequately; for that, one needs dialectic.

(d) It has been said that in induction (specifically theory confirmation), one begins with a theory and then gets evidence. In retroduction, one begins with the initial data, and finds a hypothesis to explain it. The temporal order of the processes is different.

But this is again misleading. A researcher, pondering in puzzling data, may have a *geistesblitz*—the sudden occurrence of an idea. But this (again) isn't yet hypothesis conception; the idea must be recognized as being such that it would (if true) account for the data. And the representation of that inference is the same as the representation of the typical inference involved in hypothesis testing. The temporal order in which the researcher's mind apprehended the premises is irrelevant to the logic of those situations, however much they differ dialectically.

(e) Many writers claim that retroduction and induction differ in the "strength" of the inference. As Peirce puts it,[[3]](#footnote-3) induction is the stronger inference, plausibility being a weaker evidential relation than probability. And although the word "plausible" is often used as a synonym for "probable," we also quite often speak of something as being "only" plausible, not probable.[[4]](#footnote-4)

Even if it were true that the strength of evidence in retroductive arguments is generally weak, it would not follow that the nature of the evidential relation in a retroductive argument is of some special sort. Why not just say the obvious: retroductive arguments are inductive arguments, but comparatively weak ones? Naturally, it takes more evidence to prove a hypothesis than to justify taking it seriously (worthy of testing), but the nature of the evidential relation is the same in proposing as in testing.

Consider this example. Suppose Fred's statement that he just saw Bob at home suggests to me that Bob is at home. I may verify my hypothesis (in the ordinary sense of "verify") by asking several other individuals who have just returned from Bob's house. If they testify that Bob is at home, my proof of the hypothesis consists not only of their testimony, but Fred's as well. The initial evidence (testimony) continues to be evidence for the claim, and forms part of the decisive proof (the totality of all the witnesses' testimony).

(f) One last attempt to distinguish retroduction from induction, retroductive strength from inductive strength, should be mentioned. Hanson drew a distinction between reasons for accepting a particular, minutely specified hypothesis from reasons for suggesting that the true hypothesis, whatever it turns out to be, will be of a certain kind.[[5]](#footnote-5) In conceiving a hypothesis, we have reasons to think that it will be of a certain kind, before we have hit upon the precise hypothesis which succeeds in its predictions. In particular, said Hanson, there are reasons which suggest that the particular hypothesis H will be of a certain kind, yet which cannot establish H. Hanson gives us an example of Kepler's reasoning by analogy that since Mars had an elliptical orbit, and since it was a typical planet, then perhaps all planets have non-circular orbits of some sort (though perhaps not ellipses).

Hanson was fusing two distinct claims: first, that certain reasons (e.g., analogical ones) can be good enough to show a hypothesis plausible, yet cannot be good enough to establish its truth; second, that there could be evidence for a general hypothesis type which was not evidence for specific versions of that hypothesis. (The first claim has only incidentally to do with analogical reasoning—Hanson also mentions reasoning from considerations of simplicity or symmetry.[[6]](#footnote-6)

Both claims are wrong. Consider the first. Why can't an analogical argument be strong enough to establish the truth of a hypothesis? Granted it can't ever be conclusive in the deductive sense—but then, neither can arguments from personal observation or any other inductive arguments. An analogical argument can have as high a degree of inductive strength as any other type of inductive argument.

Part of what Hanson had in mind is, of course, the point that often (indeed, typically) the evidence which suggests a hypothesis is weaker than the final evidence which establishes it. This point was examined in (e) above and found to be uninteresting.

But Hanson had more in mind: he felt (apparently) that certain "types" of evidence are never, by their very nature, able to prove hypotheses. Our earlier example can be modified to clarify this point.

Suppose my hypothesis that Bob is at home was suggested by Fred's testimony, but proved (not by other people's testimony but) by my own eyes. I simply go to Bob's house and see him sitting there. Granted, in some vague sense, the evidence which suggested my hypothesis was of a different type (testimony) from that which proved it (my own observation), but so what? Fred's testimony continues to be evidence for my hypothesis, but the evidence from my own eyes is (normally) so decisive that Fred's testimony is otiose. The testimony of others is not a *logically* different kind of evidence from personal observation (all of it is inductive); it is merely otiose given the latter.[[7]](#footnote-7)

So it is with Kepler. Mars' orbit being elliptical is good evidence for (say) Jupiter's orbit being elliptical. But if we have decisive evidence in the form of the demonstration that all observed points of Jupiter's orbit fall on an ellipse, the original evidence is otiose. That it was analogic is beside the point.[[8]](#footnote-8)

So Hanson's first claim is easily seen to be false, especially when you keep in mind that its plausibility rests on overlooking the fact that fairly good evidence can be rendered otiose, can be overshadowed by decisive evidence.

Also giving Hanson's first claim its air of plausibility is the fact—admitted in (e) above— that the evidence which suggest a hypothesis is usually weaker than that which confirms it. That, as we saw, proves nothing to the effect that retroductive arguments are not (as they seem to be) merely a subclass of inductively strong arguments.

Hanson's second claim, that there can be evidence for a general hypothesis which is not evidence for specific versions of it, is misleading if not false. Again, we can grant the point made in (e) that it takes more evidence to establish a specific version of a hypothesis than a general version, that sufficient or decisive for a general hypothesis is not usually decisive for specific versions of it. That is trivial and doesn't establish a difference between retroduction and induction.

No, what Hanson means is that there can be evidence for a general hypothesis which isn't evidence for each of the specific versions of it. This is not true, at least in those cases where there is not background information bearing on the various specific versions of the general hypothesis.

Consider a simple example. Suppose a fire occurs in a warehouse, and empty gas cans are found nearby. This, together with the fact that only humans are known to set fires, is evidence for (would suggest) the general hypothesis that a person set the fire. It is also—absent any background information—evidence for the hypothesis that a man set the fire. It is (again, absent any background information) equally good evidence that a woman did it. Of course, if you add in to the premises the fact that more men than women commit arson, then it is a stronger inference to conclude that a man did it. But that is just what I mean by background information.

As a general rule, it is not necessarily true that if E (some evidence) supports a general hypothesis, it supports every specific version of it. But it is true that if E contains no information against a given specific version, then E supports that version (given that it supports the general hypothesis). Thus, Hanson's second claim is, if not false, quite misleading. Hanson's claims thus do not succeed in distinguishing retroduction from induction.

Summing up this section, it appears that there is no reason to deny what most logicians take for granted, viz., that retroductively-strong arguments are just a subclass of inductively- strong arguments, and that the evidential relation of retroductive strength ("plausibility") is just inductive strength, usually to a lesser degree. To that extent, retroduction is induction.

Yet Hanson was right in his intuition that there is some difference between hypothesis conception and hypothesis testing, a difference he was never able to delineate. The key to the problem is the false dilemma he set for himself:

Now, no one will deny that some differences exist between what is required to show H true, and what is required for deciding H constitutes a plausible kind of conjecture. The question is: are these logical in nature, or should they more properly be called "psychological" or "sociological?"[[9]](#footnote-9) [17](#bookmark53)

Hanson meant by "logic" assertoric logic, and then—since he felt the distinction between hypothesis conception and hypothesis checking is more than merely a psychological difference—had trouble pointing out a difference in the logic of hypothesis conception (from hypothesis checking). In fact, there is no difference under that interpretation of "logic."

Yet there clearly is a logical difference if you construe "logic" broadly, to include dialectic. Reasons for proposing H differ dialectically from those for accepting H they play different roles in the learning process, even if they are logically indistinguishable from the point of view of assertoric logic. It is this insight I shall explore in Section Five of this paper.

**Paradigm Inductive Arguments and Their Interrelations**

It would be worthwhile at this point to review those argument forms traditionally pointed to as being inductively strong, with an eye to seeing if they are interrelated.

1. *Enumerative Induction*: This form of inductive argument is usually rendered (taking the special case of the "straight rule" where the ratio of m/n = 1):

Pa1 & Qa1

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**.**

Pan & Qan

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(x)(Px Qx)

This argument is subject to certain criteria or constraints. The most commonly cited constraints are that n be sufficiently large and that the sample ai's have no biasing properties. Of course, making these intuitively obvious constraints precise, and stating with precision when these constraints are in fact satisfied, is a matter of some controversy.

Now, enumerative induction and the underlying straight rule have been dismissed by Ackerman as being simply fallacious, because the conclusion follows neither deductively nor by the probability calculus.[[10]](#footnote-10) I think this view is essentially correct, as I shall explain later.

But of more immediate concern is this. As Mill noted, and as Goodman has emphasized, we must also stipulate that Q be a "projectable" property. If we check one bar of copper and find that it easily conducts electricity, we may well immediately generalize that all copper conducts electricity well.[[11]](#footnote-11) But seeing a million grue emeralds won't justify us in saying that all emeralds are grue. Yet it seems that what counts as projectable depends upon our background knowledge and beliefs. Doesn't this mean that the assessment of enumerative inductive arguments depends upon pragmatic (i.e., extra-logical community based) factors? Doesn't that destroy any hope of an inductive logic, conceived of—following deductive logic—as a syntactic (formal) undertaking? This is just the problem of context-dependency which we must address in the next section.

Often overlooked is the fact that there is a sort of "second-order" induction by enumeration, where one generalizes on properties rather than individuals. One might, that is, infer from the fact that a and b have all observed properties in common, that they share all (relevant) properties:

P1a & P1b

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**.**

**.**

Pna & Pnb

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(II)(IIa IIb)

1. *Analogy:* Analogy is often rendered:

Pa1 & Qa1

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**.**

Pan & Qan

Pan+1

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Qan+1

with constraints identical to enumerative induction. Russell[[12]](#footnote-12) regarded this as a safer inference than enumerative induction, in that the conclusion in an analogical inference refers only to one individual. But this seems odd, in that the individual we refer to (the an + 1st) is arbitrary — we could just as well argue from the same premises to any other P-individual we run into. Thus, we can derive enumerative induction from analogy (assuming we can mix inductive and deductive inference) via universal generalization.

On the other hand, some argue that people really (as a matter of psychological fact) don't reason by analogy, but instead, generalize first, then instantiate next. The best rendering of this conjectured compressed inference would be as follows.

Pa1 & Qa1 (premise)

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**.**

**.**

Pan & Qan (premise)

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Pan+1 (premise)

(x)(Px Qx) (follows from simple enumeration) Pan+1 Qan+1 (follows by universal instantiation)

Qan+1 (follows by Modus Ponens)

Thus, first-order analogy and first-order enumerative induction are inter-derivable.

As in enumerative induction, we seem to have the problem of "background information" in figuring out what properties are relevant and which are not.

There is a second-order analogical argument, of course. If a and b share all relevant properties so far examined, then they will share the next relevant property. Again this seems inter-derivable from second-order enumerative induction, assuming the embeddability of second- order deductive rules of inference.

1. *Eliminative induction*: Eliminative induction, which Keynes and C.D. Broad took to be basic, involves gathering evidence of different sorts, from different areas, in generalizing. Thus, for example, the generalization "all frogs are green" is best supported by looking at frogs from many different regions of the earth (as opposed to looking at the same number of frogs, but all from the same area). Clearly, eliminative induction has the same form as enumerative induction, but with the constraints stated differently (the demand that the sample be stratified instead of "sufficiently" large).
2. *Inference to the Best Explanation*: Finally, there is the IBE. As before, we have an argument from which can be put syntactically, and constraints which cannot be so stated:

F

H would, if true, best explain F

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H

where the constraints center around what it is for a hypothesis to be better than another. One usually sees listed various virtues that a hypothesis should possess: simplicity; consilience (the ability to account for evidence in various- domains); conservativity (not conflicting with established theories); generality (having the most content); and so on.

Again, none of these virtues can be stated purely formally, or so it seems after intensive work. And, like moral virtues, they are at tension with one another (as generosity is at tension with prudence).

How does the IBE fit in with the other traditional inductive argument forms? Well, as Harman points out enumerative induction (and by extension analogy) can be viewed as a special case of inference to the best explanation. For when do we generalize from the fact that all observed P's are Q's to the hypothesis that all P's are Q's? We do so just when that hypothesis is the best explanation of what we have seen, i.e., when there is no evidence that our sample was insufficient or biased.[[13]](#footnote-13)

On the other hand, we can represent any reasoning characterizable as IBE as an analogy as well. For consider a situation where I hypothesize that this F case was caused by an H — say, where I hypothesize that these footprints were caused by a man walking on the beach. Presumably, if that is the best explanation (at least, in my opinion), then in the majority of cases like this footprints are caused by men (or at least that is what I would believe).[[14]](#footnote-14)

But then my mental act, my inference, can as well be represented as an analogy:

This is an F

Most F's are H's

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This is an H

as it can by the IBE:

This is an F

Were this an H, its being an F would be explained

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This is an H

Thus it seems that all of the traditional forms of inductive arguments are "inter- convertible," in that any inference representable by one can be represented by any of the others. The question of which is "basic" is for our purposes irrelevant. It is irrelevant if one means by that to ask "what argument form actually represents how we, in fact, mentally process information inductively, " since that is a question for cognitive scientists to answer empirically. (We shall soon show how to represent "inductive" reasoning dialectically.)

The question is also irrelevant if by it one means to ask "which form plays the role in axiomatized formal inductive logic that Modus Ponens plays in the typical deductive logistic?" That question is not interesting because—as I will point out next—those traditional forms are not formalizable at all.

**Traditional Inductive Arguments and Context-Dependency**

We say that a rule is *formal* if it can be stated without reference to any particular reasoner or group of reasoners. Of course, the modern approach to formulating logical rules involves constructing artificial language schemata which incorporate in their rules of transformation the logical rules. Thus, in a symbolic system, the rules being purely formal (in my sense) amounts to their being formulatable on the syntactic level only.

The very nature of logic thus being to formulate general rules, it is clear that logicians should in theory seek formal rules where possible. After all, a formal rule, a rule not limited to person or group, is more general than informal rules.

What are some of the ways a logic, or a set of logical rules, can be said to be pragmatic, community—or context—dependent? Well, a logical system may be said to be pragmatic if its application by the logician to represent certain reasoning involves consideration of context (of whatever sort). Let us call this "application pragmaticity" or "application context-dependency."

Clearly, such context-dependency is not very disturbing. After all, classical, extensional deductive logic is "pragmatic" in that sense: I must use contextual clues and psychological conjectures to determine if your argument (say) is an instance Modus Tollens:

~ A

B A

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~ B

or is instead a case of contraposition followed by Modus Ponens:

~ A

B A

~ A ~ B

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~ B

A second way a logical rule may be said to be pragmatic (not fully formal, context- dependent) is for the rule's validity to depend upon the possession (by the reasoner) of certain concepts or beliefs ("background information"). This sort of pragmaticity is not easily brushed aside.

In the foregoing section I repeatedly hinted at the "context-dependency" (of the pernicious sort) of inductive evidential relations, as exemplified by those traditional argument forms. Indeed, this problem is not only constantly referred to in the professional literature, but is apparent to even the neophyte logic student.

Consider in some detail traditional argument forms and the ways they depend upon background beliefs. In the case of enumerative (and eliminative) induction, what properties you generalize upon (i.e., what predicates you project) depend upon your background. What sample you consider large enough and unbiased enough depends upon your background information (or similarly, how you stratify your sample depends upon the background information you possess). Crucial in that background information would be your assumptions about how that property is distributed in the population.

But if a simple enumerative argument is good or bad depending upon background information (or else "upon the way the world is"), doesn't that mean that assessing such arguments is impossible? The traditional answer is to float some version of the "requirement of total evidence," that is, to require that inductive arguments contain within their premises all relevant evidence, so that we can assess the argument without worrying about background or the way the world happens to be.

Alas, the requirement of total evidence is not a very good way out. Does it mean "all possible relevant evidence?" If so, it's unhelpful because the human race will never have all the evidence possible. Does it mean "all relevant evidence possessed at the present time?" Then the assessment of any argument is open to revision later. As Ackerman puts it,

This (the total evidence requirement) gives the statistical syllogism (i.e., the straight rule) a kind of temporal validity. Surely this is a very obscure piece of advice. Is one to run off all relevant experiments before computing a probability? Otherwise one apparently doesn't know when to stop gathering information, since to stop gathering it he must estimate the probability of finding further relevant information at zero, and this seems to involve the requirement of a paradox...[[15]](#footnote-15)

Indeed, Ackerman claims that the requirement of total evidence, intended to enable us to assess the argument by inspecting only it, has the opposite effect.

We cannot look at an inference and know that the premises embody the total evidence except by considerations outside the argument. Now it is usually thought in traditional logic that the validity of an inference should be determinable from an examination of its premises and conclusion. In inference forms involving the requirement of total evidence, this feature is absent....[[16]](#footnote-16)

To be fair, however, the proponent of the requirement might reply that such inductive arguments contain a premise of the form "There is no other evidence relevant to the matter"—and, while determining the truth of that premise would involve outside considerations, assessing the relation between those premises and the conclusion does not. (Of course, such a premise is in fact unknowable.) But we still have the problem: what counts as "relevant?"

With analogy, to compare two things is to say they are like each other in all "relevant" or "significant" respects. But again, what counts as relevant? (Projectability problems.) And how many similarities does it take to establish the analogical conclusion? (Sample-size problems.) As before, it all depends upon background considerations.

Needless to say, the IBE also falls prey to these attacks, these allegations of context- dependency. To say a hypothesis is (say) simpler than another is to make a claim the acceptance of which depends upon one's having certain ideas about simplicity, about what aspects (number of entities posited? Length of formulae? Number of new predicates introduced?) are relevant.

If one reviews the literature and these various accusations, he sees that they are all variants of two fundamental criticisms. Consider the inductive evidential relation "E therefore C." One problem stressed is that this relation is not invariant with respect to translation, whereas the relation of deductive-validity is. Hence the valid argument "all emeralds are green; thus this emerald is green" remains valid when translated into Goodman English (English, but with Goodman-type predicates): "all emeralds are grue; thus this emerald is grue." But the inductive argument "all emeralds up until now have been green; so the next one I see will be green" loses its strength when translated (after the year 2000) "all emeralds have been grue, so the next emerald I see will be grue."

The second problem stressed in the literature is that the strength of "E, so C" is not knowable by inspection, as is the validity of an argument. For example, "This child has fever, congestion and pains, and all his family currently has the flu, so he has the flu" seems strong, but if the child has suffered from bouts of malaria over the years, the argument is not, in fact, strong. The observer can't tell just by inspecting the argument. (Compare any valid argument; there, the matter can be settled by inspection alone.)

The usual reply is to add a "total evidence has been given" premise. But again, aside from the puzzling nature of such a premise (it deals, not with the conclusion being discussed, but with the argument itself), it could never be known. And saving the inspectability of an argument by adding a premise which is by its nature unknowable seems a pyrrhic victory indeed.

**Dialectic and Induction**

Matters stand thus. If we assume that there is some evidential relation called "inductive-strength," and seek an inductive logic consisting of paradigmatically acceptable argument forms (rather like inference rules), we run up against the stone wall of context-dependency of a very pernicious sort. Is there some other way to view inductive reasoning, one that does not put it on a par with deductive reasoning, a view which gets around the problem of context-dependency and also allows us to more naturally bring in the probability calculus? I think there is—a view that utilizes formal dialectic. To get an intuitive feeling for this sort of view, let's consider Mill's methods for a moment.

Mill's five rules of scientific method are usually greatly misunderstood, not least because of the way they are stated. They are stated as if they are argument forms, but in fact they are something quite different. Mill's five canons purport to be rules of discovery of explanatory hypotheses—hypotheses about the causes of phenomena being investigated—as well as rules for proving those hypotheses. Let's focus on the Method of Agreement, my remarks applying equally well (mutatis mutandis) to the other four canons as well.

Mill's method of agreement is (bluntly stated) this: If all E type events (or phenomena) have factor F in common, and only factor F in common, then F causes E. For example, for the fact that the only thing bald men have in common is a deficiency in zinc, the hypothesis that zinc deficiency causes baldness in men suggests itself. But at the same time the hypothesis is proved by the very data which suggest it.

As it stands, then, this rule seems to be a deductively valid (and quite unhelpful) rule, if we mean by "cause" a necessary and sufficient condition (or set of conditions) for the occurrence of E. This rule seems unhelpful, since the antecedent (that the only factor E events have in common is F) can never be known in practice, since the number of factors is infinite.

But in reality, what the Millian has in mind is this. We begin our investigation with an idea of what factors are relevant, what factors could possibly be the cause of E. We begin, that is, with a list of possible candidates. Then we begin checking E case after E case, eliminating possible candidates by the deductively valid rule "if F is not present in any given E case, F cannot be the cause of E."

Thus the method of agreement is indeed a method, an interactive process for learning the answer to a question, and not an argument form at all. The iterative process, of course, involves the repeated application of an argument form, but to new information each time. (This situation is similar to the trial-and-error method of solving a problem, which consists in repeatedly applying the rule of Modus Tollens.) To logically represent such methods, we need dialectic, not merely assertoric logic.

It might be wondered why the method of agreement, which involves the iterative use of a *deductive* argument form, is included in writings on inductive logic. The usual answer is that the initial hypothesis about what factors are relevant is the result of some inductive inference or other. But this means that Mill's methods begin after induction is done!

Actually, "induction" as it applies to Mill's methods really amounts to deductive reasoning on the basis of *causal* assumptions—which suggests that we don't need any other evidential relation besides just the relation of deductive validity. What we *do* need is a formal dialectical system that has built within it both causal axioms *and* probability axioms (in addition to the axioms and inference rules of deductive logic).

I will outline such a system. I cannot completely review all the concepts of formal dialectic—the reader should perhaps look at Hamblin [1970] and [1971], as well as Jason [1979]. Dialectic is the study of dialogues or conversations. But we need only concern ourselves with *information-oriented dialogs*, by which I mean dialogs whose sole purpose is the acquisition and transmission of information. We can then more precisely define *formal dialectic* as the symbolic study, involving explication and evaluation, of information-oriented dialogs.

Such dialogs can be represented by matrices of locutions:

L11 **. . .** L1n

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**. .**

**. .**

Lm1 **. . .** Lmn

where each Lij is either a question, a statement or an imperative.

It should be noted that the matrix is *normally* assumed to represent a conversation between n participants, each of which speaks in turn, or at least "passes" by saying "pass." Yet it is quite conceivable for a person to have a dialogue with himself.

Note also that any Lij may be of the form "(P1 & ... & Pn) therefore C," representing (normally) one participant giving an argument for C.

We take m to represent the number of *rounds*, i.e., the number of sets of locutions-and- responses. For the sake of simplicity, we will assume that the first round begins with "start," so that L11 has the form "start: p," where p is either a statement or a question. Again for simplicity we will assume that the dialogue ends with all the participants except the last (the nth) passing, and nth participant saying "pass and over." Thus (turning from our normal interpretation of the matrix to the matrix itself) the mth row is (pass, pass ... pass and over).

Note that, when we speak of locutions, we are presupposing an underlying assertoric grammar, plus an underlying erotetic extension thereof. Dialectic is, in reality, only an extension of assertoric and erotetic logic. We select as the underlying assertoric and erotetic grammars ordinary classical quantificational calculus plus identity, supplemented by causal and ordinary modalities (as discussed by Burks[[17]](#footnote-17)), and erotetically extended (as discussed in Belnap/Steele[[18]](#footnote-18)).

An important concept, especially from the point of view of this study, is that of the "commitment-store" of any participant at any time. (This is Hamblin's phrase.[[19]](#footnote-19)) The commitment-store of participant p at round r (abbreviated "Cp,r ") is that set of statements that he is committed to at that point. As Hamblin notes, to be committed to a statement is not quite the same thing as to believe it, but the difference need not concern us. Typically, in the cases Hamblin considers, the commitment-store begins empty, save perhaps the axioms of symbolic logistic.

We can now lay out part of our formal dialectical system. We begin with the vocabulary and the intended semantic interpretations.

|  |  |
| --- | --- |
| **Symbols** | **Intended Interpretation** |
| A, B, C,…,Z (subscripts allowed) | These are propositional constants, intended to express non-compound statements |
| p, q, r, s, tI | These are propositional variables, intended for the usual purpose |
| A1, B1,…, Z1; A2,...,Z2; ... (subscripts allowed)I | These are predicate constants, intended to designate particular properties and relations |
| a, b, c,..., o (subscripts allowed)I | Individual constants intended to denote particular individuals |
| u, v, w, x, y, z (subscripts allowed) | Individual variables, intended as usualI |
| p1,q1,r1,p2,... (subscripts allowed)I | Predicate variables, intended as usualI |
| ~,v, [],[c], = | Logical constants |
| (,),[,],{,} | Brackets, intended as punctuation devices |

Next, the derived symbols:

|  |  |
| --- | --- |
| **Derived symbols** | **Note that and β are metalinguistic variables** |
| & | (& β) =def ~(~v ~β) |
|  | (β) def (~v β) |
|  | (β) def [β) & (β )] |
| <> | <>def ~[] |
| <c> | <c> def |
|  | (β) def [](β) |
|  | (β) def [c](β) |
|  | (β) def [](β) |
|  | (β) def [c]( β) |
|  | (x)def ~ ()~ |

Finally, the non-assertoric symbols:

|  |  |
| --- | --- |
| **Non-assertoric symbols** | **Linguistic function** |
| ? | The erotetic operator |
|  | The addition operator |
| \ | The deletion operator |
|  | The passing operatorI |
| ! | The imperative operatorI |

We next stipulate the rules of construction. (These include rules for constructing statements, questions and commands). Here, “wff” abbreviates “well-formed formula.”

* + RF 1: If is a propositional symbol, then ‘’ is an assertoric wff ("awff").
  + RF 2: If n is an n-ary predicate symbol, and 1, 2, …, n are individual symbols, then ‘n(1,..., n)’ is an awff.
  + RF 3: If is an awff, then ‘~ ’ is an awff.
  + RF 4: If is an awff without any modal operators, then ‘[]’ and ‘[c]’ are awffs.
  + RF 5: If and β are awffs, then ‘v β’ is an awff.
  + RF 6: If is an awff and x is an individual variable, then ‘(x)’ is an awff.

 RF 7: If 1, …, n are all awffs, then ‘?()(1, …, n)’, ‘?()(1, …, n) ‘, ‘?(

)(1, …, n)‘, and?()(1, …, n)’ where (1 n) are all ewffs (erotetic wffs).

* + RF 8: If 11, 12, …, 1r are all one-place predicates and 1, 2, …, n are all individual variables, and n is an n-place predicate, then ‘?()(111, …, 1rr, r+1, …, n ||

n1....n)’, ‘?()(111, …, 1rr, r+1, …, n || n1....n)’, ‘?()(111, …,

1rr, r+1, …, n || n1....n)’, and ‘?()(111, …, 1rr, r+1, …, n || n1....n)’ are all ewffs.

* + RF 9: ‘!’ is an iwff (imperative wff).
  + RF 10: If is a wff, then ‘!()’ is an iwff.
  + RF 11: If and β are iwffs, then ‘!(v β)’ is an iwff.
  + RF 12: If is an awff or an ewff, then ‘!’ and ‘!\’ are iwffs.
  + RF 13: If 11, …, mn are all awffs, ewffs, or iwffs, then

11 … 11

.

.

.

11 … 11

is a wfd ("well-formed dialog").

Next we lay down the axioms, taken to be stored in the commitment-store. These include some typical axioms of quantifier calculus (with identity), augmented by Burks' modal axioms, plus (and this is most important from the point of view of this paper) the axioms of the probability calculus.

 AS 1: (& )

 AS 2: (& β) 

 AS 3 (β) [(β & γ) (& γ)]

* + AS 4: If x is not free in , then (x) is an axiom.

 AS 5: (x)(β) [(x)(x)β]

* + AS 6: Let β be the result of substituting y for all free occurrences of x in . If β has exactly as many free variables as does , then (x)β is an axiom.

 AS 7: (y){(y) (x)[(x y) & (x)])

The following six axiom schemata are restricted to non-modal a's.

 AS 8: [][c]

 AS 9: [c]

 AS 10: [](β) ([][]β)

 AS 11: [c](β) ([c][c]β)

 AS 12: (x)[][](x)

 AS 13: (x)[c][c](x)

* + AS 14: If is an axiom, so is (x).
  + AS 15: If is a non-modal axiom, so is [].
  + AS 16: Pr() = 1if is a tautology; Pr() = 0 if is a contradiction.

 AS 17: If [](β) then Pr() = Pr(β).

* + AS 18: If and β are independent, then Pr(& β) = Pr () x Pr(β).

 AS 19: If [](& β), then Pr(v β) = Pr() + Pr(β)

 AS 20: Pr( ~) = 1 Pr()

* + AS 21: If there is no set of elements in L such that any element of [E1,..., En) is entailed by it, and if [](E1,..., En) then Pr(Ej) = 1/n all j.

Delineating the rules governing legitimacy of dialogs (akin to inference rules or rules of transformation in assertoric logic) is trickier.

First, we ought to distinguish between logical rules governing dialogs and strategic rules. Rules of strategy are rules which any participant is free to break, but which he would do well to follow if he wishes to maximize his information gain. On the other hand, logical rules are rules which no participant is free to break. Compare standard draw poker: rules like "you may draw up to four cards after the first round of betting" define the game, and breaking them (cheating) will perhaps get the participant shot. On the other hand, a rule like "never draw to an inside straight" can be broken, but habitually doing so will very likely cause the player to lose over the long run. Indeed, the analogy extends to deductive logic: any good logic textbook will contrast the rules for justifying the steps in a proof (the inference rules) with the rules of strategy for divining those proofs.

Among the logical rules of dialectic, we ought to distinguish between those which arise out of, or are a part of the assertoric and erotetic underpinnings of the dialectical system, and those which are peculiar to the system as a whole. The former I call structural (logical) rules, the latter I call procedural (logical) rules, for no particular reason.

Clearly, the structural rules of dialectic are just the rules of the underlying assertoric and erotetic grammars:

Rules of Structure:

* + SR 1: From and β to infer β.
  + SR 2: The rule of substitution.

Erotetic Rules (where ‘**Q**’ is a question and **‘I’** an answer to it):

* + ER 1: If **Q** has the form ‘?()(A1, …, An)’ then **I** must have the form ‘B1 & ...& Bp’ where each of the Bi is an element of (A1, …, An) and p .
  + ER 2: If **Q** has the form ‘?()(C1x1, …, Crxr, xr+1, …, xn || Ax1…xn)’ then **I** must be

‘(Aa11…Aa1n & … & Aap1…Apn) where p .

* + ER 3: If **Q** is of the form‘?()(A1, …, An)’ then **I** must be ‘(B1 & … & Bp & C1

& … & Cq)’ where {B} {A} and {C} {A} and {B}{C} = and {B}{C} =

{A}and p .

* + ER 4: If **Q** is of the form ‘?()(C1x1, …, Crxr, xr+1, …, xn || Ax1…xn)’ then **I** must be ‘{[Aa11…a1n & … &Aap1…apn] & (x1)…(xn)[(C1x1 & … & Crxr) (Ax1…xn  ((x1,n = a11,n) v … v (x1,n = ap1,n)))]’ with p .
  + ER 5: If **Q** is of the form ‘?()(A1, …, An)’ then **I** must be ‘B1 & ... & Bp’ where 

p , and where for all i,j with 1 i, j p ~ (Bi Bj).

* + ER 6: If **Q** is of the form ‘?()(C1x1, …, Crxr, xr+1, …, xn || Ax1…xn)’ then **I** must be ‘{[Aa11…a1n & … & Aap1…apn] & [**&**(1 I I p) **V**(1 k n)(aik ajk)]}’.
  + ER 7: If **Q** is of the form ‘?()(A1, …, An)’ then **I** must have the form {[B1 & ... &

Bp] & [~ C1 & ... & Cq] & [**&**(1 I, j p)(Bi Bj)]} subject to earlier restrictions.

* + ER 8: If **Q** is of the form ‘?()(C1x1, …, Crxr, xr+1, …, xn || Ax1…xn)’ then **I** must have the form ‘{[Aa11…a1n & … & Aap1…apn] & [**&**(1 i, j p)**V**(1 k n)(ai aj)] & [(x1)…(xn)((C1x1 & … & Crxr) (Ax1…xn ((x1,n = a11,n) v … v(x1,n = ap1,n))))]’.
  + ER 9: If **Q** is of the form ‘? Why p’ then **I** must be ‘[L1 & .. . & Ln & C1 &... & Cm]’ where [L1 & ... & Cm] p, and where each Li is of the form [(x)(x => βx) & <> (x

& βx) & <> (x & βx) & <c>x & [c]βx] and where all the Li's and Ci's are elements of the commitment-store.

However, what about the rules of procedure? What do they look like? Well, in contrast to the rules of strategy, these rules are part of the essence, the definition of rational conversation (that is information oriented). Yet, in contrast to the structural rules, these rules govern the evolution of the rounds, the relation between responses to a given move and the moves prior to it. A few simple examples come to mind: never ask a question which presupposed the negation of a previous answer; never repeat the same statement twice.

More directly, it seems that the procedural rules can be stated in terms of how the commitment-store should evolve. These seem to be the most obvious such rules:

Rules of Procedure:

* + PR 1: If r(i+1) is of the form ’?Why ri’ then r(i+2) must have the form ‘(P1 & … & Pn)

ri’, where each Pi is a statement in the commitment-store or else is of the form ‘!\ari’.

* + PR 2: If the rth round is of the form (!, !, ..., !) then Cr + 1 = Cr .
  + PR 3: Let {P1, …, Pn) be the set of direct answers to question **Q**. Then **Q** must occur in round r only if (P1 v … v Pn) Cr.
  + PR 4: If P Cr, then ‘?Why P’ is permitted.
  + PR 5: ‘!’ can occur anywhere except after a challenge.
  + PR 6: If ‘!(& β)’ occurs, where and β are iwffs, then both and β must be responded to.
  + PR 7:.If ‘!(v β)’ occurs, then either or β must be responded to.
  + PR 8: If the rth round is of the form (!\P, !,..., !), then Cr + 1 = Cr P.

With this sort of formal system, we can more naturally and more accurately reflect inductive reasoning. We represent such reasoning not as arguments with some weird kind of evidential relation obtaining between premises and conclusions, but rather as dialogues in which probabilistic deductive inferences take place against the backdrop of changing elements of the commitment store.

**Gary James Jason**

**Department of Philosophy**

**Washburn University**

1. Peirce, 2.636. (All citations from the *Collected Works of Charles Sanders Peirce*). See also 2.642, where Peirce says that induction is classification, retroduction is theorization. Again, this confuses broad and narrow induction; theorization involves, after all, theory confirmation. [↑](#footnote-ref-1)
2. Peirce, 2.640. See also 6.3: "The things that any science discovered are beyond the reach of direct observation.

   We cannot see energy, nor the attraction of gravitation, nor the flying molecules of gases, nor the luminiferous ether, nor the forests of the carbonaceous era, nor the explosions of nerve cells. It is only the premises of science, not its conclusions, which are directly observed." [↑](#footnote-ref-2)
3. Peirce, 2.642. [↑](#footnote-ref-3)
4. Brian Skyrms argues that plausibility amounts merely to probability. See pp. 172-3, also p. 182ff of his *Choice and Chance* (Belmont, California: Dickenson Publishing Co., 1975), 2nd edition. [↑](#footnote-ref-4)
5. Norwood Russell Hanson, "Is There a Logic of Discovery," in *Current Issues in the Philosophy of Science*, ed. Herbert Feigl (New York: Holt, Rinehart and Winston, 1961), pp. 20-42. [↑](#footnote-ref-5)
6. *Ibid.,* p. 26. [↑](#footnote-ref-6)
7. This is normally the case. But, of course, occasionally the testimony of others can overrule one's own observation. [↑](#footnote-ref-7)
8. Carmichael made a similar point. See R.D. Carmichael, *The Logic of Scientific Discovery* (Chicago: Open Court, 1930), p. 21. [↑](#footnote-ref-8)
9. Hanson, op. cit., p. 22. [↑](#footnote-ref-9)
10. Robert Ackermann, "Some Recent Problems of Inductive Logic," in *Philosophical Logic*, ed. J.W. Davis, D.J. Hockney and W.K. Wilson (Dordrecht, Holland: D. Reidel Publishing Co., 1969), p. 143. [↑](#footnote-ref-10)
11. Indeed, some have claimed (F.E. Abbott and Dretske, among others), that we directly or immediately *see* general facts. When looking at that particular bar of copper, we just see that copper conducts electricity well, sans inference. Whether this psychological claim is true or not, as Peirce noted, is really unimportant; we can in any case represent non-inferential seeing as an inference. [↑](#footnote-ref-11)
12. Russell, Bertrand, *Problems of Philosophy* (London: Routledge & Kegan Paul, 1909).  [↑](#footnote-ref-12)
13. Gilbert Harman, "Inference to the Best Explanation," *JOP*, 1964. [↑](#footnote-ref-13)
14. For a more complete justification of this point, see K. A. Fumerton, “Induction and Reasoning to the Best Explanation,” *Philosophy of Science* 47 (1981), 589-600. [↑](#footnote-ref-14)
15. Ackermann, op. cit.,p. 142. [↑](#footnote-ref-15)
16. *Ibid*, p. 142. [↑](#footnote-ref-16)
17. Arthur Burks, *Chance, Cause and Reason,* (Chicago: University of Chicago Press, 1977). [↑](#footnote-ref-17)
18. Nuel Belnap and Thomas Steele, *The Logic of Questions and Answers* (New Haven: Yale University Press, 1976). [↑](#footnote-ref-18)
19. See C.L. Hamblin, *Fallacies*, (London: Methuen, 1970), p. 257. [↑](#footnote-ref-19)