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RANDOMNESS AND THE RIGHT REFERENCE CLASS *

THE statistical syllogism has lived a shadowy life in writings on induction and probability for quite a number of decades. It has rarely received much serious attention, although it has been mentioned by most authors, who then go on [after having said that it is (a) trivial, or (b) invalid] to address more serious problems involving probabilities, degrees of belief, and principles of action. Isaac Levi, in his recent article "Direct Inference" † correctly focuses on it as a central problem for the theory of rational credence, and one which has been badly neglected. Much recent writing has concerned personalistic probability or subjective probability, construed as a set of constraints that degrees of belief should satisfy. Most authors, accepting a fashionable relativism, seem to feel that the constraints of coherence are constraints enough. As in aesthetics and art criticism, "it's all a matter of taste." But, as in aesthetics and criticism, there is good taste and bad taste. In the theory of rational credence, for an agent to believe that almost all F's are G's, that a is an F, and yet, *with no reasonable argument,* to have a low degree of belief in a's being a G, seems to be out and out bad taste—i.e., to fly in the face of rationality. Somehow or other, in some way or other, it should be possible to spell out the circumstances under which the direct inference is rationally compelling.

I go further than Levi: I regard the principles governing direct inference as the sole principles we need for a complete theory of

* This paper has benefited from extensive correspondence with Isaac Levi, without whose perseverance it would be far less intelligible than it is. The combined wordage of our correspondence may well exceed the length of the paper. Needless to say, he does not endorse my point of view (nor I his). Nevertheless we agree that the point at issue—at base an epistemological point—is of profound and critical importance for the philosophy of science and for rational decision theory.

† This JOURNAL, LXXIV, 1 (January 1977): 5–29.

rational credence. But Levi and I disagree on these principles. It may, therefore, be a matter of some interest to examine this disagreement, since our positions are quite close in a number of other regards; and it may also be of some interest in view of the fact that any approach to rational credence must, eventually, even if it is not regarded as a central issue, come to terms with the fact that certain statistical syllogisms, under certain circumstances, seem to most scholars (and to most people) to be rationally compelling.

I shall first present some of Levi's conditions for direct inference, together with his interpretation of them. I shall then give my own way of looking at direct inference and suggest the way in which these two viewpoints can be construed as following naturally from the adoption of two different paradigms for probability. This will be followed by a criticism of Levi's view and a defense of mine.

I

An example of the direct inference, or the statistical syllogism, is the following:

Premise: Coin a is fair; i.e., exhibits a chance of 0.5 of landing heads when tossed in a standard manner.
Premise: The next toss of coin a is to be performed in a standard manner.
Conclusion: The appropriate degree of belief in the occurrence of heads on the next toss is 0.5.

The first premise will be called the major premise, the second the minor premise. The argument is compelling only if we accept the premises—in this it does not differ from any conventional deductive argument. On the other hand, it is clear that accepting the premises is not sufficient for the argument to be compelling: the premises may remain true and acceptable if we replace "the next toss" by "the last toss" in the minor premise. Furthermore, the first premise admits of two interpretations, as Levi points out. It may have the form (1):

(1) The chance of an outcome R occurring on a trial of kind S on a chance set up a equals r (p. 6).

Or it may have the form:

(1′) The percentage of trials of kind S which result in events of kind R equals r (p. 7).

Levi expresses the minor premise thus (6):

(2) Event e is a trial of kind S on chance setup a.

A conventional constraint on the cogency of the argument has the form of the codicil: "and that is all the agent knows about *e*." Levi correctly notes that this condition is useless: one always knows a great deal about *e* other than that it is a trial of kind *S* on chance set up *a*. In particular we will generally know when and where and by whom *e* will be conducted; we will know something about the circumstances under which it will be conducted; etc. So in order to understand direct inference, we must get clear about whether the form of the major premise is (1) or (1′); and we must get clear about the codicil to the minor premise that is sufficient to render the argument compelling.

With respect to the major premise, Levi requires that it have the form (1). His argument for this requirement boils down to this: Using (1′) leads to trouble unless some such principles as those I have offered are invoked; the principles I offer lead to a violation of confirmational conditionalization; we cannot allow confirmational conditionalization to be violated; therefore the major premise must be construed as having form (1) rather than form (1′). Although this analysis is flattering, it is not very compelling. I see no reason to suppose that some variation on the principles I invoke will both allow the use of (1′) and preserve confirmational conditionalization. For example, one could simply invoke the preservation of confirmational conditionalization as a third requirement on the "choice of a reference class." Furthermore, as Levi points out himself (23), adopting major premises of the form (1) will not in itself save confirmational conditionalization.

There are considerations, however, that buttress the argument. Levi is one of "those who take chance seriously." The use of chance statements allows a simple reconstruction of probability statements that seem to be about members of very small classes: thus, given the classical coin that will be tossed but once and then destroyed, we can suppose that the chance of a toss of this kind performed on this coin yielding heads is a half, even though the *frequency* of heads on tosses of this coin is either 0 or 1. More importantly, Levi seems to feel that if the major premise is not a genuine chance statement, the direct inference can impose no credal obligations on a rational agent. If *X* knows that 90% of *A*'s are *B*'s, and we suppose (*per impossibile*) that *X* knows of *a* only that it is an *A*, then it seems axiomatic to me that *X* should assign a degree of credence equal to 0.9 to the hypothesis that *a* is a *B*. Levi simply sees "no compelling reason why rational *X* should be obliged to make a credence judgment of that sort on the basis of the knowledge given" (9). In the face of so direct a conflict of intuitions, one cannot expect to resolve the issue by arguments. We can, however, explore both the sources of the conflict of intuitions and the consequences of following out one or the other program.

With respect to the minor premise, Levi claims that rational X should use the strongest description of the event e that he knows. This is a sensible counsel of total evidence, with which it is hard to see how anyone could disagree. Nevertheless it is worth spelling out, if only as an antidote to the informal proviso sometimes encountered that "X knows nothing else about Petersen than that he is a Swede." We can never, of course, be in a position to know of a no more than that it is an F: we will know, of course, that it is identical with itself, that its name is a, that it was selected in such and such a manner, specified in such and such a way, that it exists at such and such a time and such and such a place, that it was selected or specified by such and such a person, and so on. The important issue—and one that is related to the treatment of the major premise—is how we are going to treat this information. Levi imposes a strong requirement as the codicil that will legitimize the direct inference: we must *know* of each of the elements of the strongest description of the event e that it is stochastically irrelevant to the chance of an outcome R on a trial of kind S. In some circumstances we do indeed have this kind of information: that a coin is tossed by me is stochastically irrelevant to its landing heads; the time and place of operation, we know generally, are stochastically irrelevant to the outcomes of chance mechanisms. But in a later section we shall look more closely at Levi's codicil.

Direct inference, for Levi, is to be examined in a framework of conditions for partial belief. Let $K_{X,t}$ be a set of statements representing X's rational corpus at t; it contains just those sentences which X is committed to accepting at t. Let $C_{X,t}$ be X's confirmational commitment at t, i.e., a function from *potential* rational corpora to credal states. Let $B_{X,t}$ be X's credal state at t. Given a potential corpus K, a credal state is a *set* of functions $Q(h;e)$ from pairs of statements to real numbers, satisfying:

(1) *Consistency:* B is nonempty if and only if K is consistent.
(2) *Coherence:* Every Q-function in B is a probability measure relative to K.

Since Q takes two arguments, it is a conditional probability measure. Where t is any sentence entailed by K, we may define an absolute probability function in the obvious way: $Q(h) = Q(h;t)$. Since a major issue we shall come to involves conditional probabilities, we will express Levi's condition of coherence in the following pair of propositions:

(2a) Every absolute Q-function is coherent: i.e., for every t in K, $Q(h,t)$ is a probability measure relative to K.

(2b) Every conditional Q-function is coherent: i.e., for every e not in K, but consistent with K, $Q(h,e)$ is a probability measure relative to K.

In thus dividing the requirement of coherence, I mean to focus attention on the multiplication axiom. Of course it is true that, given an absolute Q-function, we can *define* a conditional Q-function satisfying (2b) [for e such that $Q(e) \neq 0$, $Q(h,e) = Q(h \wedge e)/Q(e)$], but this may not be at all like the conditional Q-function that Levi takes as primitive. That Q-function may be taken to represent "conditional degrees of belief," as expressed, for example, in conditional bets—i.e., bets that are called off if the event on which they are conditional fails to occur. Thus the coherence of the conditional Q-function is intended to include the multiplication axiom,

$$Q(h \wedge e;f) = Q(e;f) \cdot Q(h;e \wedge f)$$

as a constraint on degrees of belief. Note that this constraint does not require us to consider regularity.

Note also that the multiplication axiom merely reflects a constraint on X's *present* degrees of belief. By itself it reflects no commitment on X's part were he in fact to add e to his body of knowledge. This is a separate matter; that X's confirmational commitment does entail that he ought to modify his beliefs in accordance with the conditional probabilities $Q(h;e \wedge f)$, *were* he to add e to his body of knowledge f, is embodied in Levi's principle of confirmational conditionalization, to which we shall turn shortly. Here it suffices to note that this principle is not the same as the principle that X's conditional degrees of belief should satisfy the multiplication axiom.

Let B_e be the set of functions $Q(h;e)$. Levi imposes the condition that this set be convex:

(3) *Convexity:* For any h, every weighted average of a finite subset of the set $B_e(h)$ is in $B_e(h)$.

Finally, Levi imposes on confirmational commitments the condition of confirmational conditionalization.

(4) *Confirmational Conditionalization:* Let K be a rational corpus, and let K' be obtained from K by adding a statement e, consistent with K, and forming the deductive closure. X's confirmational commitment at t, $C_{X,t}$ specifies what X's credal state *would* be, *were* he to endorse the corpus K'. It specifies that for every Q-function Q in his credal state based on K, there would be a Q-function Q' in his credal state based on K' such that $Q(h,e \wedge f) = Q'(h,f)$ and conversely.

Note that this does not specify what X's credal state will be—or should be—at some other time when he has in fact endorsed the corpus K'. According to Levi, X may change his confirmational commitment at any time. Thus temporal credal conditionalization need not obtain, though it will, if X does not change his confirmational commitment. Levi has yet to characterize the grounds on which a rational agent might justifiably change his confirmational commitment (if there are any); but we may leave this point aside, since no change in confirmational commitment is involved in direct inference.

Given this framework, how does direct inference work? It becomes simply a special case of the general determination of degrees of belief by confirmational commitment and total body of knowledge. We require that the major premise be a statement about chance, rather than a statement about frequencies. This may not be as simple a it sounds, for it may not be obvious when a statement is a chance statement and when it is merely a frequency statement. Clearly the word 'chance' is not a clear indicator, for often people use such expressions as 'objective probability', 'likelihood', and even 'frequency' to express what Levi would want to call a "chance" statement. (The objective probability of heads on a toss of coin a is a half; the likelihood of heads on a toss of coin a is a half; the odds are even that a toss of a will yield heads; a toss of a is as likely to yield heads as tails; tails are as frequent as heads, etc.) Furthermore, people use 'chance' to express what Levi would regard as a mere frequency. The chance of a Swede's being a Protestant is 0.9; the chance of an assistant professor being promoted is slim nowadays; the chance of a manufacturing defect in radios coming off a certain production line is 0.01; the chance of being mugged in New York is greater than the chance of being mugged in Vershire; the chance of a card in an ordinary deck being black is a half; etc.) Thus in order to follow the advice to take as the major premise in direct inference only a statement concerning chances, and never one concerning frequencies, we would have to find a usable criterion.

Furthermore, even if the major premise of the direct inference is a chance statement, that is not sufficient to ensure the validity of the inference. Levi points out that the rules for direct inference that I offer lead to "conflict with confirmational conditionalization" even when they are modified so as to require chance statements. Therefore, "in order to avoid such conflict, X [must be] required to employ the strongest description he knows to be true of the trial" (23). It is totally unclear to me why employing a strongest description should enable X to avoid conflict with the principle of confirmational conditionalization. Of course, given Levi's principles (1)–(4), conflict with confirmational conditionalization will be avoided—principle (4), after all, is just the principle of confirmational conditionalization. Furthermore, since my rules require X to take account of everything he knows to be true of the trial on a—isn't this the same as employing a "strongest description"?—it is difficult to see how the "strongest description" requirement could possibly serve to eliminate conflict with confirmational conditionalization.

The gloss of this requirement shows what Levi has in mind, however, and it is indeed something (as we shall see) quite different from what I have in mind. Let e be a strongest description of a trial of kind S. It is also a trial of kind S that is identical with e; it is a trial of kind S performed at place p and time t, it is a trial of kind S also satisfying a large number of other predicates; let the total set of predicates satisfied by the event e be $P_1, \ldots P_n, \ldots$ (There will, of course, be an infinite number of them.) There is only one event—the event e in question—that satisfies all these predicates. However, we may still be able to make a *chance* statement about events of that kind, i.e., events that are trials of kind S satisfying the predicates $P_1, \ldots P_n, \ldots$ (We could not make a *frequency* statement about events of this kind—unless we knew the result of the trial—since there is only one event of this kind.)

Presumably, then, the conditions for the validity of the direct inference are the conditions under which we have grounds for accepting a chance statement about events that are trials of kind S satisfying predicates $P_1 \ldots P_n, \ldots$ When we begin with a statement to the effect that trials of kind S yield result R with chance p, we can obtain the statement that trials of kind S that are also trials of kind $P_1 \ldots P_n \ldots$ yield result R with chance p, just in case we know that the properties $P_1 \ldots, P_n, \ldots$ are stochastically irrelevant to the result R among trials of kind S in general. That this is what Levi has in mind seems clear from the following statement: "Thus if 'e' describes the event by identifying it as the toss at place p and time t, X's conviction that he can ignore the information that the event is identical with e may be a consequence of his general conviction that the spatiotemporal location of trials of kind S is stochastically irrelevant to their resulting in events of kind R" (24). Again: "If X knows that the fact that the toss is by Morgenbesser is stochastically irrelevant, he can ignore that information in direct inference and base the inference on his knowledge of the chance of heads on a toss of coin a" (25).

Now this is obviously imposing pretty severe restraints on direct inference. Of each of the infinite number of predicates we know e to satisfy (in addition to "is a trial of kind S") we must *know* that it is stochastically irrelevant to the outcome R. Levi talks of "conviction," but surely he must mean more than bare conviction; it must in some sense be grounded or justifiable conviction. Sometimes (as in the case of spatiotemporal location of tossings of coins) we may have general theoretical grounds for our assertions of irrelevance. But often we do not have any such grounds. Often the only grounds we have for believing that P_i is irrelevant to result R on a trial of kind S are directly statistical. We examine a large number of instances of trials of kind S, and we find no significant difference in the frequency with which R occurs among those that satisfy the predicate P_i and those that fail to satisfy it. When we lack both this statistical background and the background that would provide general theoretical justification for an assertion of irrelevance, then the direct inference should (on Levi's view) be frustrated.

In fact, the situation is even worse. Not only must we have grounds for asserting the stochastic irrelevance of each of the infinite number of properties that e has, but we must have grounds for asserting the irrelevance of every *combination* of them. (This in view of the fact that it is possible for both A and B to be irrelevant to the production of R on a trial of kind S although the combination of A and B is not irrelevant to the production of R on a trial of kind S.) And when the grounds for asserting irrelevance are not general and theoretical, but require to be based on actual statistical evidence, it is clear that few, if any, instances of direct inference are warranted.

II

Let me now review my own approach to direct inference. In place of Levi's four conditions on rational credence, I take direct inference to be fundamental to all credibilities. Every (epistemic) probability derives, more or less directly, from direct inference. As opposed to Levi, I take the major premise to be (usually) a mere frequency statement. Sometimes the frequencies involved may be frequencies in somewhat indefinite populations (for example, when we talk of the distribution of weights in a certain biological population) and sometimes they may even be hypothetical (as when we talk of frequencies of heads in tosses of an *ideal* coin), but in general I want to hew as closely as possible to actual frequencies. The reason for this is that, in performing direct inferences, we are generally concerned with guiding our actions in the actual world. Actual frequencies are better guides in the actual world than are hypothetical frequencies or chances. If I am concerned about what to believe about the result R occurring on a trial of kind S, and I know both that the chance of R on a trial of kind S is p and that the frequency *in the actual world* is q, I would be well advised to base my behavior on the number q rather than on the number p.

As does Levi, I require that the minor premise contain as complete a description of the individual event in question as is available to the agent X. Or, to put the matter more simply, I require that the agent X take account of his total body of knowledge.

But the codicil that warrants the direct inference is completely different. Where Levi required that every property and combination of properties known to be possessed by the individual in question be *known* to be stochastically independent of the result R, what I require is that the individual be a *random member* of the set mentioned in the frequency statement constituting the major premise. Now randomness, for me, is an epistemological notion and involves negative criteria: that is, roughly speaking, I would say that, relative to my rational corpus, a is a random member of F with respect to belonging to G, just in case there is nothing in my body of knowledge which would put a into some other reference class that would be better as a guide for my credal state. To spell out the criteria of randomness in full generality is tedious, lengthy, and unnecessary here (but has been done elsewhere).[1] The general

[1] See my *The Logical Foundations of Statistical Inference* (Boston: Reidel, 1974).

idea, however, can be characterized by means of two principles which, in simplified form, go somewhat as follows:

(A) If I know that a belongs to H and to F, and I know the frequency of G's in H, and this frequency differs from the frequency with which I know G's occur in F, and I do *not* know that F is a subset of H, then I should reject the claim that a is a random member of F with respect to G.

(B) If I know that a belongs to H, and no knowledge of mine requires that I reject the claim that a is a random member of F with respect to G according to principle (A), then if I know the frequency of G's in H more precisely than I know the frequency of G's in F, I should reject the claim that a is a random member of F with respect to G.

These two principles are not adequate as they stand (is it obvious?), but they can be refined to yield a definition of randomness. With this definition of randomness, the direct inference takes the form:

(1) X knows that the frequency of G's among F's is p (more generally: between p and q).
(2) The individual a (fully described) is known by X to belong to F.
(3) Relative to X's rational corpus, a is a random member of F with respect to belonging to G.

Therefore,

(4) X's degree of belief that a is a G is constrained to be (or to lie in) the interval (p,q).

Probabilities in general are based on direct inferences in the following way. Given a rational corpus, we may consider equivalence classes of statements such that each member of the equivalence class is known in that rational corpus to be equivalent to all other statements in that equivalence class. (If we eschew deductive closure, we obtain a similar result by considering biconditional chains.) The probability of a statement is the interval (p,q) just in case that statement belongs to an equivalence class at least one member of which obtains the probability (p,q) by means of a direct inference.

We have the following results for a rational corpus that is consistent and deductively closed:

(a) Every statement has a probability relative to that body of knowledge.
(b) There is no reason to suppose that any of those probabilities are

represented by the maximally vague interval [0,1]. (Contrary to what Levi seems at times to suggest.)

(c) Every probability is based on a *known* frequency.

(d) There exists a function P, whose domain is the set of sentences of the language, and whose range is the interval [0,1] with the property:

(i) It is a coherent probability function.

(ii) For every statement in the language, if the probability of the statement is the interval (p,q), the value of the function P for that statement falls within the interval (p,q).

Property (d) corresponds to Levi's principle (2a). However, Levi's principle (2b) fails: if we add e to the corpus K, the new probability of h, relative to that rational corpus need not (though it may, and often will) satisfy the principle of confirmational conditionalization. We will return to the consideration of this question later.

Leaving the failure of the principle of confirmational conditionalization to one side, Levi objects to this formulation of direct inference on the grounds that the major premise is only a frequency statement. He writes, for example: "suppose the strongest information X knows about a given trial is that it is a toss of a coin a by Sidney Morgenbesser... X may be convinced that Morgenbesser has and will toss coin a once and only once. Hence, from X's point of view, the reference class of tosses of a by Morgenbesser contains exactly one event and the percentage of such tosses landing heads must be either 0 or 100. Yet the chance of heads on such a trial can be some value between 0 and 1 and, indeed, can be equal to the chance of heads on a toss of coin a" (25). Thus the direct inference employing a major premise involving chance goes through all right, but the direct inference employing a major premise involving frequency is doomed to emit the useless conclusion that X's degree of belief in the occurrence of heads on the toss in question should be constrained to lie between 0 and 1.

But this is simply not true of my view. The toss in question is a member of a lot of classes other than the class of tosses performed by Sidney Morgenbesser with coin a, and it is precisely the function of the rules governing randomness to single out the most appropriate class for determining X's credal state with regard to that toss's landing heads. The toss is a member of the set of tosses performed by Sidney Morgenbesser; it is a member of the set of tosses of coin a; it is a member of the set of tosses performed on such and such a date; it is a member of the set of tosses performed with a coin of such and such a denomination; and so on. Levi focuses his attention on the tosses of coin *a*, and supposes we know the chance that a toss of coin *a* will yield heads, and supposes that we know that the time, the place, the fact that the toss was performed by Morgenbesser, etc., are all stochastically irrelevant to the occurrence of heads. He does so, presumably, because he knows that some coins are biased: some other coin than *a* might have a different chance of yielding heads. But on what grounds does he conclude that *a* is unbiased? The picture one forms is of Levi meeting Morgenbesser in the hallway, reaching into his pocket, pulling out a quarter, and asking Morgenbesser to toss it once and once only. But if that is coin *a*, it is very unlikely indeed that it either has been subjected to careful physical scrutiny concerning its distribution of mass or has been subjected to a long series of test tosses that have been subsequently subjected to statistical analysis and have supported the hypothesis that its chance of heads is a half.

I can think of only one way in which *X* can have a justified conviction that the chance of heads on a toss of coin *a* is a half (or close to a half) and that is this: *X* may, on the basis of a long history of social lore and gambling tradition, be fully convinced that practically all coins are practically fair, i.e., have a chance close to a half of landing heads when tossed. He may, therefore, by direct inference take it to be practically certain that *a* is fair, since *a* is one of those coins. But of course this is precisely the form of direct inference that Levi will not admit. The analogous inference on Levi's principles would involve the curious notion that the coin in his pocket came to be there through some process of random selection. Levi would claim that ordinary commercial transactions are such as to ensure this random selection. I am not sure that this notion is either intelligible or necessary; and, given that it is intelligible, I don't see how we can have non-question-begging warrant for it. And even if this were the way the story went, *X* would have to pass from having a credal state in which he is practically certain that *a* is fair, to the *conviction* that *a* is fair: the fairness of *a* must become part of his rational corpus. Levi does have an account of this transition though it is not entirely unproblematic.

On the view that I advocate this problem does not arise. We may employ the social lore and gambling tradition directly. The appropriate reference class for the toss in question is *not* tosses performed with *a* by Sidney Morgenbesser (about which we know nothing); *nor* tosses performed with *a* (about which we have only inferential knowledge); but rather tosses of coins in general, concerning which we have good grounds for believing that half of these tosses, more or less, yield heads. The general principle is that the direct inference uses as major premise a statement concerning that class about which we have the most precise information, subject to the proviso that no other class to which the event in question is known to belong gives rise to *conflicting* information.

III

Levi can reply that the question of how we come to know, or think we know, the chance that a toss of coin *a* will yield heads is irrelevant to the analysis of the direct inference concerning a toss of *a* by Morgenbesser. He will insist that the direct inference seems cogent to him only when its major premise is a chance statement. As I indicated earlier, it seems obvious to me that the closer we can stick to frequencies in the actual world as guides to our credal states, the better off we are. What we have here is a divergence of intuitions so fundamental that it is very likely to be unaffected by arguments and examples. We have all the symptoms of a divergence in paradigms, and that is just what underlies the conflict. For Levi the paradigm for probability is the toss of a coin; for me it is the urn of black and white balls. The coin has the chance of landing heads that it has, regardless of whether or not it is ever tossed. (Factor in the tossing apparatus, if you will.) There is no "reference class" that determines the probability. The urn, on the other hand, has its objective composition, and it is this composition that determines the probability. If we are concerned with the probability that a ball in the urn is white, what can it be other than the proportion of white balls in the reference class—i.e., the set of balls in the urn?

As in the case of any two healthy conflicting paradigms, each can handle the situation the other takes as paradigmatic. For Levi, to talk about the probability that a ball in the urn is white is, if intelligible at all, elliptical for talk about the probability that a ball *selected* by a certain method from the urn is white. It is, of course, only certain special methods of selection that have the curious and useful property that the chance of a white ball being selected by that method is numerically the same as the proportion of white balls in the urn. For me, to talk about the probability that a particular coin will yield heads on a toss, if interesting at all, is interesting only because that particular coin has been extensively tested so that there is in fact a large population of its tosses that has been sampled. We need not know exactly how many times the coin will be tossed (we need not know exactly how many balls there are in the urn), and we cannot empty the set of tosses out on the rug and count the total number of heads. But often we must infer the proportion of white balls in an urn on the basis of a sample drawn from the urn, and we may suppose that we are doing the same thing with the tosses of the coin. Of course, as I said earlier, when we talk about the probability that a particular toss of a particular coin will land heads, we have in mind (or should have in mind) not the set of tosses of that coin, but rather the very large set of tosses of coins in general, from which we (and friends and neighbors for a thousand years) have observed a very large sample, and about which we have concluded, with perfect justification, that the proportion of heads is very nearly a half.

Now, although I am convinced that there are rational grounds for choosing between paradigms, in both science and philosophy, it seems clear that there are no quick and simple knock-out arguments that will settle the matter in this case. There are nonetheless some considerations that may bear on the relative attractiveness of the two paradigms.

First of all, there are the advantages, already alluded to, of sticking close to the actual frequencies in the world. If, for example, we could know of a certain coin that it would be tossed exactly n times and would yield exactly m heads, surely the appropriate credal stance to adopt toward the statement that one of these tosses of the coin will yield heads is represented by the number m/n, regardless of what the "chance" of heads on a toss of that coin may be. If an insurance company could know exactly how many of its policy holders of a certain category will die during the ensuing year, it could establish its rates more competitively than it can when it must rely on statistical inferences based on past data. If we know the exact distribution of some random quantity in a population, we are better off using that exact distribution, rather than supposing that the population is generated by a chance mechanism and inferring the character of the chance mechanism from samples of the population. Why are we better off? Because, as is easy to demonstrate and easier to see, the expected error of our estimates concerning future samples from the population will be less.

Second, there are disadvantages to the coin paradigm. One of them is that it invites an infinite regress. Given that X knows that 90% of Swedes are Protestants, and that Petersen is a Swede, Levi sees "no compelling reason" (9) why rational X's credal state should be represented by the number 0.9. This is because X does "not know whether the way in which Petersen came to be selected for presentation to him is or is not in some way biased" (9). (An odd locution: "selected for presentation to him.") In order for X to be obliged to have this degree of belief in Petersen's Protestantism, he should "know that Petersen has been selected from the Swedish population according to some procedure F and also know that the chance of obtaining a Protestant on selecting a Swede according to procedure F is equal to the percentage of Swedes who are Protestants" (10). But although we know that in general applications of F will yield Protestants with a chance of 0.9 (just as we knew that the proportion of Protestant Swedes was 0.9) we are concerned with a *given* application of F—that application which yielded Petersen for presentation to X. Thus in order that X should have a degree of belief that that application of F will yield a Protestant, why should he not be obliged to know that that application of F was selected from the applications of F according to some procedure F′, and also know that the chance of obtaining an application of F that will yield a Protestant is the same as the chance that F will yield a Protestant in general, which is also equal to the percentage of Swedes in the population? And of course we are now confronted with a particular application of F′, and in order for that to be rationally compelling, should we not need to know that that particular application of F' is selected by a particular method F'' which is such that...?

I imagine the answer is that, once one has got a chance mechanism, one no longer has to make any selections: the chance mechanism does it. As soon as you have converted the urn into a chance mechanism by considering, not balls in the urn, but balls drawn from the urn, you have an honest-to-goodness chance statement to use for direct inferences. But a further difficulty looms: the method of selection must have a special property if the parameter of the chance statement is to be the same as the parameter characterizing the population. It must be such that the chance that it selects any one member of the population is the same as the chance that it selects any other member. This is an empirical chance hypothesis, and surely should not be accepted without evidence. But what kind of evidence can we have for it? Indirect evidence is not hard to come by: the method of selection may have been shown to be fair in other applications, so that we can infer (by direct inference? What is the method of selecting *this* application?) that the method will be fair in this application. But at some point the evidence that the method is fair must be direct: we must have evidence that the method picks each member of some population equally often, or with equal chance. And it is hard to imagine that any method has been tested in this direct way.

Finally, it is relatively rare that the probabilities we are really concerned with—the probability of rain, the probability of death, the probability of accident, the probability of an increase in the Dow Jones average, the probability of the breakdown of a piece of machinery—can be related to chance mechanisms. Surely whether it rains or not is not dependent on the outcome of a trial on a chance setup. Surely an insurance company does not select its policyholders by means of a method of selection F, nor does the dark angel himself select those whom he visits by means of a chance setup. It is quite true, as Levi says, that I do not take chance seriously. But not only do I regard it as metaphysical moonshine (to use Levi's happy phrase), I regard it as generally quite useless. In the relatively rare cases in which it is useful (see my "Chance" [2]) it is useful because it provides a way of characterizing hypothetical populations when the real ones are in some way inaccessible.

IV

Of course things are not all one-sided. What Levi regards as the most serious argument against my treatment of direct inference—although he recognizes that it would apply to my treatment even were I to believe in objective chances—is the fact that I am obliged to violate confirmational conditionalization. It is, at least indirectly, due to the fact that I adopt the urn paradigm for probability that I formulate rules for direct inference in the way that I do, and so the violation of confirmational conditionalization may plausibly be seen as an indirect consequence of my preference for frequencies over chances.

Levi's example (20–22) to illustrate the violation of confirmational conditionalization is the following: Suppose that X's rational corpus contains:

(5) 90% of Swedes are Protestants.
(6) Either 85% or 91% or 95% of Swedish residents of Malmö are Protestants.
(7) Petersen is a Swedish resident of Malmö.

Using f_r to abbreviate '100 r % of Swedish residents of Malmö are

[2] *Journal of Philosophical Logic*, v (August 1976): 355–393.

Protestant', (6) may be written '$f_{0.85} \vee f_{0.91} \vee f_{0.95}$'. Let h assert that Petersen is a Protestant.

According to my rules, X's degree of belief in h should be 0.9, on the grounds that we have more precise information regarding the proportion of Swedes in general that are Protestants than we have concerning residents of Malmö, and what information we do have about Malmö does not really conflict with what we know about Swedes in general.[3]

It is provable that my system of direct inferences satisfies Levi's condition (2a), i.e., that there is an absolute Q-function that satisfies both my constraints and the requirements of coherence. Any such Q-function then, will assign to h the measure 0.9. If we now proceed in the ordinary way to *define* a conditional Q-function, we will encounter no inconsistency:

$$Q(h;f_{0.85}) = \frac{Q(h \wedge f_{0.85})}{Q(f_{0.85})}$$

But this is not the way Levi goes, for he takes the conditional Q-functions as basic. He thus takes them to impose constraints on absolute Q-functions (in accordance with the multiplication theorem), whereas I take the constraints to work the other way around and am thus led to reject Levi's interpretation of $Q(h;f_{0.85})$. Levi construes $Q(h;f_e)$ as a permissible fair betting quotient on h for cases where bets are called off if e is false. If we endorse, as Levi recommends, the principle of confirmational conditionalization, this will also represent the fair betting quotient on h that X should adopt when he actually adds e to his body of knowledge. Suppose that X adds to his rational corpus the statement $f_{0.85}$. Thus, according to Levi, on my principles $Q(h;f_{0.85})$ should have the value 0.85. Interpreting the two-place conditional Q-function in this way, it is easy enough to show that there is no Q-function satisfying the constraints:

(d) $Q(h;f_{0.85}) = 0.85$

(e) $Q(h;f_{0.91}) = 0.91$

(f) $Q(h;f_{0.95}) = 0.95$

[3] This may not be clear in view of the peculiarity of our knowledge about the proportion of Swedes in Malmö who are Protestants. Ordinarily, of course, our information regarding the subclass would simply be vaguer, so that if we know that between 0.90 and 0.92 of Swedes in general are Protestants, we might also know that between 0.85 and 0.95 of Swedes in Malmö are Protestants. It is hard to see where we could get the strange information that we are supposing X to have. And Levi remarks that my examples designed to support the plausibility of rejecting confirmational conditionalization are bizarre!

(b) $$Q(h;f_{0.85} \vee f_{0.91}) = 0.9$$
(c) $$Q(h;f_{0.85} \vee f_{0.95}) = 0.9$$
(a) $$Q(h) = 0.9$$

We cannot impose confirmational conditionalization on my principles of direct inference. One thing or the other must bend.

But let us look a little more closely at these two-place Q-functions. One justification for the requirement that one-place Q-functions satisfy the axioms of coherence is that we can imagine the agent X being forced to post odds on a number of propositions. If he posts odds, and accepts all bets, on a set of propositions, where the odds reflect his degrees of belief, then, if his degrees of belief are incoherent, he will be in a position to have a book made against him. Of course the rational agent will not post odds that enable a book to be made against him. It is not obvious that in any epistemic state the agent will be able to post coherent odds that *also* reflect his degrees of rational belief. That it turns out to be the case is interesting and important and gratifying. Similarly, one of the arguments that is intended to justify the requirement that the two-place conditional Q-function be coherent is that the agent may be forced to post odds not only on straight bets, but on conditonal bets. Again, he will post odds that will not allow a book to be made against him. Again, it is not obvious that in any epistemic state it should be possible for him to use his degrees of belief to determine such a set of odds. If we define the two-place Q-function in terms of the one-place Q-function, in the conventional way (as I did above), then that Q-function will determine a set of odds such that X will not be in a position to have a book made against him. Let us call the principle thus satisfied the principle of *credal conditionalization*. Let us see what emerges from Levi's example with respect to credal conditionalization. Clearly, the two-place Q-function defined on a coherent one-place Q-function, for conditions whose Q-values are not 0, will be coherent. Let us look more closely at $Q(h;f_{0.85})$.

$$Q(h;f_{0.85}) = \frac{Q(h \wedge f_{0.85})}{Q(f_{0.85})}$$

An intuitive way of approaching the value of the numerator is to consider the dependence between h and $f_{0.85}$. That Petersen is a Protestant has very little to do with what proportion of residents of Malmö are Protestants, other things being equal. Thus

$$Q(h \wedge f_{0.85}) = Q(h) \cdot Q(f_{0.85};h) \approx Q(h) \cdot Q(f_{0.85}).$$

The two propositions (still speaking intuitively and informally) are relatively independent. Thus $Q(h \wedge f_{0.85})$ will be very close indeed to $Q(h) \cdot Q(f_{0.85})$, and $Q(h;f_{0.85})$, far from being equal to 0.85, will be (close to) 0.9.

This shows that the two-place function cannot be construed as a conditional degree of *confirmation*, but only as a reflection of the odds and conditional odds that are consonant with my present credal state, based upon my present *actual* body of knowledge. It does not reflect the change in my credal state that should be induced by my actually *accepting* one of the propositions considered.

Let us call the operation of moving to a new Q-function as a result of adding an item to the rational corpus *epistemic conditionalization*, and represent it by a two-place function $QE(h;e)$; $QE(h;e)$ will be the Q-value of h, relative to the rational corpus obtained by enlarging the present rational corpus by the addition of e.

Conditions (a)–(f) above (see p. 517/8) are indeed all satisfied by QE, on my view. It is quite true, for example, that if $f_{0.85}$ is added to my rational corpus, my new credal state with regard to h will be represented by 0.85. What Levi's argument shows, then, is that, if we are talking about epistemic conditionalization (QE) rather than credal conditionalization (Q), Bayes' theorem fails. QE is not coherent.

One response might be that, although we have certain intuitions about credal conditionalization and, indeed, dutch-book arguments to the effect that it would be nice if credal conditionalization yielded coherent odds, we simply have no such clear-cut intuitions concerning epistemic conditionalization. If perfectly clear, orderly, and intuitive principles of direct inference lead to a violation of the supposition that epistemic conditionalization is coherent, so much the worse for the coherence of epistemic conditionalization.

Another response might be to cite the many authors (among the best known, Popper and Cohen) who have argued that epistemic conditionalization is not and cannot be a matter of probabilities, and thus that it should not satisfy the requirement of coherence.

But we can do even better, with Levi's own example. Suppose, contrary to fact, that QE is a coherent conditional probability function. $QE(h)$ is to be just $QE(h,t)$, where t is any proposition already in our rational corpus. From Bayes' theorem it follows that

$$QE(h;f_{0.85}) = \frac{QE(h) \cdot QE(f_{0.85};h)}{QE(f_{0.85})}$$

$QE(f_{0.85};h)$ represents our degree of belief in $f_{0.85}$ when we add h to our body of knowledge. Under ordinary circumstances, however, coming to accept that Petersen, the Swede who lives in Malmö, is a Protestant will have no significant effect on our degree of belief in the demographic hypothesis that 85% of the Swedish residents of Malmö are Protestants. Thus $QE(f_{0.85};h)$ should realistically be taken as equal to $QE(f_{0.85})$, and it will follow that $QE(h,f_{0.85}) = QE(h) = 0.85$, and that our original degree of belief in h was 0.85. But, by considering $f_{0.91}$, it would follow equally that $QE(h) = 0.91$. Clearly something is fishy; Bayes' theorem cannot hold for QE.

It might be thought, since ratios are tricky things, that what is happening is that a very tiny effect is imposed on our degree of belief in $f_{0.85}$ by the discovery that Petersen is a Protestant and through the magic of ratios this effect is magnified. But the solution is not to be sought in these forests: the value of $QE(h;f_{0.85})$ will change only by a factor equal to the ratio of $QE(f_{0.85};h)$ to $QE(f_{0.85})$, and if $QE(f_{0.85})$ is reasonably large, as it may well be, (say 0.5), it cannot plausibly be supposed to change by a large factor.

Suppose we do regard all this as an optical illusion: after all, if Petersen is a Protestant, perhaps that does have a small bearing on our credal state with regard to the proposition that 95% of the Swedes in Malmö are Protestants. Then perhaps nothing stands in the way of imposing coherence on the epistemic conditional Q-function as I interpret it? I suspect (as does Levi) that the attempt would lead to difficulties. In any event, the constraint seems gratuitous. After all, we have already assured the coherence of the absolute Q-function. We have already ensured that the principle of *credal* conditionalization is satisfied. Our agent can post odds—both on straight bets and on conditional bets—on all the statements in his language that (a) reflect his credal state and (b) do not allow a dutch book to be made against him.

We have here a phenomenon which is both intuitively plausible and common and to which I drew attention in my book. It can easily be the case that e is highly relevant to h (in the sense of epistemic conditionalization), but h is irrelevant to e. Thus, to turn the equation around, $QE(h;f_{0.85})$ can be very different from $QE(h)$ —showing that $f_{0.85}$ is highly relevant epistemically to h—but $QE(f_{0.85})$ is essentially or actually indistinguishable from $QE(f_{0.85};h)$ —showing that data concerning one individual is not highly relevant to (or may even be irrelevant to) a general statistical hypothesis concerning the population to which that individual belongs.

So much the worse for Bayes' theorem as applied to epistemic conditionalization.[4] Of Levi's principles, it is the principle of confirmational conditionalization—what I have lately called "epistemic conditionalization"—that must go. I have drawn attention to Levi's condition (2b), since that embodies the multiplication axiom, and it is the combination of the multiplication axiom and the principle of confirmational conditionalization that, on my view, leads to trouble. Confirmational conditionalization imposes a certain interpretation on conditional *Q*-functions, and, on this interpretation, I claim, (2b) fails. Condition (2b) holds when conditional *Q*-functions are defined as ratios of absolute *Q*-functions; credal conditionalization is satisfied. But, if we interpret conditional *Q*-functions that way and absolute *Q*-functions my way, the principle of epistemic confirmational conditionalization no longer seems to hold of them.

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CHARITY, INTERPRETATION, AND BELIEF

IT is generally agreed that a principle of charity should play some part in regulating the project of radical interpretation. But it is a question what status such a principle enjoys. Donald Davidson has urged that charity with respect to the beliefs and sayings of others is a *sine qua non* of successful translation; more, that unless we see to it that veracity preponderates in a creature's attitudes and utterances we cannot construe its behavior as that of a rational agent or psychological subject. Thus he says:

> Since charity is not an option, but a condition of having a workable theory [of radical interpretation], it is meaningless to suggest that we might fall into massive error by endorsing it. Until we have successfully established a systematic correlation of sentences held true with sentences held true, there are no mistakes to make. Charity is forced on us;—whether we like it or not, if we want to understand others, we must count them right in most matters.[1]

[4] The distinction between two sorts of conditionalization has also made its appearance in statistical literature. See James S. Williams, "An Example of the Misapplication of Conditional Densities," *Sankhya*, XVIII (1966): 297–300, and R. J. Buehler, "Some Validity Criteria for Statistical Inference," *Annals of Mathematical Statistics*, XXX (1959): 845–863.

[1] "On the Very Idea of a Conceptual Scheme," *Proceedings of the American Philosophical Association*, XLVII (1973/74): 5–20, p. 19.