CAN COHERENCE GENERATE WARRANT *EX NIHILO*?

PROBABILITY AND THE LOGIC OF CONCURRING WITNESSES

Abstract

Most foundationalists allow that relations of coherence among antecedently justified beliefs can enhance their overall level of justification or warrant. In light of this, some coherentists ask the following question: if coherence can elevate the epistemic status of a set of beliefs, what prevents it from generating warrant entirely on its own? Why do we need the foundationalist's basic beliefs? I address that question here, drawing lessons from an instructive series of attempts to reconstruct within the probability calculus the classical problem of independent witnesses who corroborate each other's testimony.

E. Assorted proofs

1. Weak foundationalism and the coherence theory

Common to all versions of foundationalism is the thesis that there are *basic beliefs—*

beliefs that have a degree of noninferential justification or warrant, warrant that does not

derive from inferential relations to other beliefs. (Throughout this paper I use 'warrant'

simply as a syllable-saving synonym for 'justification', not in Plantinga's technical sense.

[1]) All other justified beliefs are justified in virtue of the inferential support they receive

directly or indirectly from basic beliefs.

Laurence BonJour distinguishes three grades of foundationalism (1985, 26-30).

According to *strong* foundationalism, basic beliefs are "not just adequately justified, but

also *infallible, certain, indubitable*, or *incorrigible*" (26-27). According to *moderate*

foundationalism, the noninferential warrant possessed by basic beliefs need not amount to

absolute certainty or any of the other privileged statuses just mentioned, but it must be

"sufficient by itself to satisfy the adequate-justification condition for knowledge" (28).

Finally, according to *weak* foundationalism,

> basic beliefs possess only a very low degree of epistemic justification on their own, a
> degree of justification insufficient by itself either to satisfy the adequate-justification
> condition for knowledge or to qualify them as acceptable justifying premises for further
> beliefs. Such beliefs are only "initially credible," rather than fully justified. (28)

We must rely on coherence among such initially credible beliefs to amplify their level of

warrant up to the point where it is adequate for knowledge. Views along these lines have

been advocated by C.I. Lewis (1946), Bertrand Russell (1948), Nelson Goodman (1952),

and Roderick Chisholm (1977), among others.[2]

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[1] According to Plantinga, warrant is "that, whatever precisely it is, which together with truth makes the
difference between knowledge and mere true belief" (1993, 3). On many accounts, this will include not
merely justification, but also whatever further condition is required to solve the Gettier problem.
[2] Russell and Chisholm are not across-the-board weak foundationalists, for they hold that there are some
basic beliefs (for example, beliefs about the current contents of sense experience) that are adequately

As BonJour notes, weak foundationalism seems to be a hybrid view, mixing together foundational and coherentist elements. In fact, Susan Haack (1993) prefers to call it "foundherentism," which she illustrates by comparing empirical knowledge to a crossword puzzle. (A beautiful example to compensate for the ugly name!) Experience corresponds to the clues, which give an initial presumption in favor of certain beliefs or entries in the puzzle; the initial beliefs are then either confirmed by the way in which they interlock with other entries or discarded because they do not fit in. Thus does coherence amplify (or its absence erode) the initial warrant possessed by basic beliefs.[3]

But if coherence can amplify warrant in this way, what prevents it from generating warrant entirely on its own, without any need for basic beliefs? This question has been asked by several authors. Thus BonJour:

> The basic idea is that an initially low degree of justification can somehow be magnified or amplified by coherence, to a degree adequate for knowledge. But how is this magnification or amplification supposed to work? How can coherence, not itself an independent source of justification on a foundationalist view, justify the rejection of some initially credible beliefs and enhance the justification of others? (1985, 29).

David Shatz:

> Is there not a difference, it will be said, between allowing coherence to strengthen an existing justification, and allowing a set of beliefs to be "so coherent" that its members are justified by virtue of coherence alone? . . . But surely just to assert the distinction [between strengthening and creating] is arbitrary: if a circular chain is vicious [as critics of coherence allege], its viciousness should deprive it of any epistemic efficacy whatsoever. (1983, 108-109)

Richard Foley:

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justified for knowledge on their own; indeed, such beliefs are certain. But, as we shall see, they also hold that there are other basic beliefs (for example, those based on memory) that come up to the level of justification required for knowledge only with the help of coherence.

[3] Haack summarizes her theory in Haack 2000. She does not herself classify foundherentism as weak foundationalism, since she believes it essential to foundationalism that the foundations not receive support from other elements in the structure.

> If a set . . . can be raised in epistemic stature by relations of coherence, why cannot a person's entire set of beliefs be raised in a similar way without appealing to any foundational claims? (1980, 62)

BonJour and Shatz raise this question in favor of a pure coherentism, in which coherence by itself can generate warrant. Foley raises it in favor of a pure foundationalism, in which coherence plays no essential role. But all parties agree that there is something wrong with the weak foundationalist's or foundherentist's mixed view.

   In what follows, I explore the following closely related questions: Is there any good rationale or defense for the weak foundationalist's position? Can coherence generate warrant on its own, or does it presuppose initially warranted beliefs to operate on? If it can amplify warrant, why can't it generate warrant? Finally, if weak foundationalism is an unstable position, should we move in the direction of pure coherentism or in the direction of a purer and stronger foundationalism?[4] A large part of my investigation will be centered on a classical problem in probability theory—the case of individually unreliable witnesses whose answers are credible because they cohere with one another.

2. Lewis versus BonJour

An excellent example of a weak foundationalist theory is provided by C.I. Lewis's theory of memory knowledge. It has two parts:

> First; whatever is remembered, whether as explicit recollection or merely in the form of our sense of the past, is *prima facie* credible because so remembered. And second; when the whole range of empirical beliefs is taken into account, all of them more or less dependent upon memorial knowledge, we find that those which are most credible can be assured by their mutual support, or as we shall put it, by their *congruence.* (1946, 334)

Lewis defines a congruent set as one in which any member is more probable given the rest than it is on its own:

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[4] As Tomoji Shogenji has pointed out to me, the foundationalist position to which we move would have to be not only stronger (at least moderate), but pure as well, not allowing relations of mutual support to raise the degree of justification of beliefs in the system. (The strength and purity dimensions are distinguished in Haack 2000, 227.)

A set of statements, or a set of supposed facts asserted, will be said to be congruent if and only if they are so related that the antecedent probability of any one of them will be increased if the remainder of the set can be assumed as premises. (1946, 338)

A point on which Lewis repeatedly insists is that congruence cannot generate probability

or warrant on its own. Rather, some of the statements must have initial credibility, which

congruence can then amplify:

If, however, we are not to repeat some of the fallacies of the historical coherence theory, it becomes vitally important to observe that neither mutual consistency throughout, nor what we call congruence of a set of statements, nor even that relation of a system in which every included statement is deducible from others which are included, can by itself assure even the lowest degree of probability for a body of empirical beliefs or suppositions in question. For that, it is absolutely requisite that some at least of the set of statements possess a degree of credibility antecedent to and independently of the remainder of those in question, and derivable from the relation of them to direct experience. (1946, 39; see also 352.)

How *much* initial probability must the congruent items have? Only a "slight" amount,

Lewis says, illustrating his point with the example of individually unreliable witnesses

who tell the same story:

Our previous example [1946, 239] of the relatively unreliable witnesses who independently tell the same circumstantial story, is another illustration of the logic of congruence; and one which is more closely typical of the importance of relations of congruence for determination of empirical truth in general. For any of these reports, taken singly, the extent to which it confirms what is reported may be slight. And antecedently, the probability of what is reported may also be small. But congruence of the reports establishes a high probability of what they agree upon, by principles of probability determination which are familiar: on any other hypothesis than that of truth-telling, this agreement is highly unlikely . . . . (1946, 346)

Lewis says what goes for the reports of witnesses also goes for the "reports" of memory:

they do not fully authenticate what is reported, but what is reported may nonetheless

become highly credible or practically certain through congruence:

[S]omething I seem to remember as happening to me at the age of five may be of small credibility; but if a sufficient number of such seeming recollections hang together sufficiently well and are not incongruent with any other evidence, then it may become highly probable that what I recollect is fact. (1946, 352)

But just how much credibility do we need to start with?

> [I]t is only essential that the fact of present memory afford *some presumption* of the fact which is memorially presented. All that is needed is initial assumption that the mere fact of present rememberings renders what is thus memorially present in some degree credible. For the rest, the congruence of such items with one another and with present sense experience will be capable of establishing an eventual high credibility, often approximating to certainty, for those items which stand together in extensive relations of such congruence. (1946, 354)

That the witnesses may be "unreliable" suggests perhaps that the probability of what they

report given that they report it may be less than 0.5. That is compatible with probability

merely greater than zero, as is perhaps also the following:

> Just what degree of credibility thus attaches initially to the remembered, merely because remembered, we do not need to ask. . . . If, however, there were *no* initial presumption attaching to the mnemically presented; . . . then no extent of congruity with other such items would give rise to any eventual credibility. (1946, 356 and 357)

There is one passage, however, that seems to imply that the level of probability Lewis has

in mind is not merely greater than zero, but greater than 0.5:

> [A]nything sensed as past is just a little more probable than that which is incompatible with what is remembered and that with respect to which memory is blank. (1946, 358)

That implies that what is ostensibly remembered is more probable than its own negation,

which implies in turn a probability greater than 0.5.[5]

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[5] I should acknowledge the possibility of another interpretation of Lewis on this point, favored (in personal communication) by both Michael Huemer and Tomoji Shogenji. On their alternative interpretation, the requirement of initial credibility is the requirement that the probability of a fact reported given the report need not be greater than 0.5, but must be greater than the prior probability of what is reported. In other words, the report must confirm what is reported in the confirmation theorist's incremental sense—$P(X, X$ is reported) must be greater than $P(X)$.

I base my own interpretation partly on this sentence: "anything sensed as past is just a little more probable than that which is incompatible with what is remembered" (AKV, p. 358). Huemer and Shogenji both point out that if X and Y were contrary hypotheses about the past (such as 'the die landed showing 1' and 'the die landed showing 3'), $P(X, X$ is remembered) could be greater than $P(Y, X$ is remembered) without being greater than 0.5. True. But I take Lewis to be implying that anything sensed as past is more probable than *anything* incompatible with it, and that would include its bare negation as well as sundry contraries.

As another point in favor of my interpretation, I note that Lewis states the requirement of initial credibility in these words: "whatever is remembered, whether as explicit recollection or merely in the form of our sense of the past, is *prima facie* credible because so remembered" (1946, 334). If a proposition about the past had an unconditional probability of 0.1 and a probability conditional on its being

I shall take it, then, that Lewis's view is that the congruence of a set of items (for example, memory reports) may raise their probability as close as one likes to 1, but only if they have a level of antecedent probability or credibility (given the fact of being reported) greater than 0.5.

To this contention BonJour has raised an objection. There is no need, he says, for Lewis's requirement that memory reports or other cognitive deliverances have initial credibility:

> What Lewis does not see, however, is that his own example shows quite convincingly that no antecedent degree of warrant or credibility is required. For as long as we are confident that the reports of the various witnesses are genuinely independent of each other, a high enough degree of coherence among them will eventually dictate the hypothesis of truth telling as the only available explanation of their agreement—even, indeed, if those individually reports initially have a high degree of *negative* credibility, that is, are much more likely to be false than true (for example, in the case where all of the witnesses are known to be habitual liars). (1985, 147-48)[6]

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remembered of 0.2, it would have initial credibility in the incremental sense, but it could hardly be called *prima facie* credible.

To reinforce this point, I note that on p. 315 of 1946, Lewis equates the terms 'credible' and 'probable', then notes that to call something 'probable' in ordinary usage implies that it has probability exceeding 0.5, though not by any definite amount. Presumably, being "*prima facie* credible" is not compatible with being *less* than credible!

My interpretation can admit that incremental confirmation is *part* of Lewis's requirement of initial credibility, just so long as probability > 0.5 is another part. A requirement of incremental confirmation can perhaps be extracted from Lewis's full statement: "anything sensed as past is just a little more probable than that which is incompatible with what is remembered and *that with respect to which memory is blank*. (1946, 358, my emphasis). How are we to understand this statement? Is Lewis saying that anything remembered is more probable than *anything whatsoever* with respect to which memory is blank? No, I think he is more charitably interpreted as saying that if you remember something, that thing is more probable given that you remember it than *that thing* would be if you had no memory concerning it. That is, for any proposition X, $P(X,RX) > P(X,\sim RX \& \sim R\sim X)$. Let us make the plausible assumption that $P(X,\sim RX \& \sim R\sim X) \geq P(X,\sim RX)$. The previous two formulas then imply that $P(X,RX) > P(X,\sim RX)$, which is confirmation-theoretically equivalent to $P(X,RX) > P(X)$. So yes, memory reports must incrementally confirm what is reported; but they must also make what is reported more probable than not.

I acknowledge the following as a point against my interpretation: Lewis says the reporters may be "unreliable" (1946, 352); is that compatible with what they report being more probable than not given that they report it?

[6]When BonJour says the witnesses may be known to be "habitual liars," he had better not mean they are *perfect* liars who always speak falsely, else we could use their reports as an infallible guide to the truth. See Bovens and Hartmann, 2003, 14, n.5.

When BonJour says that *no* antecedent degree of credibility is required before coherence can do its work, he does not mean that the reports may have *zero* probability. A probability of zero can never be raised by subsequent evidence. As he says in the material following the dash, the reports may be "much more likely false than true," and that would mean that they have a probability less than 0.5, but still greater than zero.

So now we have a clear-cut issue to investigate: in order for congruence to raise credibility to near 1, what level of antecedent credibility is required? Among the answers I canvass below are the following: the probability of X given that a witness or ostensible memory attests to X must be greater than 0.5 (Lewis's own view, as I read him); it must be greater than zero, but not necessarily greater than 0.5 (BonJour's alternative); it must be greater than chance (Blitstein 2001); it must be greater than P(X), the prior probability of X (Huemer 1997, Olsson and Shogenji 2004).

To investigate this issue, I propose to look at the classical problem of the agreement of independent witnesses—a frequent topic in probability texts from Condorcet in the 18th century through Keynes in the 20th and into the present day, and a topic to which Lewis and BonJour both appeal in support of their disparate views.

3. Hooper's formula

I begin with the oldest formula known to me for computing the probability that concurrent witnesses are correct. A version of it was published anonymously in the *Transactions of the Royal Society* for 1699 (probably by George Hooper, Bishop of Bath), and it seems to have been reincarnated in every century since, other apparent avatars including Diderot (1755), Whately (1859) and Ekelöf (1964).[7]

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[7] Earman 2000 includes an excerpt from the 1699 article on pp. 193-94. Earman cites Dale 1992 for evidence that the author was Hooper (85, n. 73). The next avatar I cite, the article "Probabilité" in the French *Encylopédie*, was unsigned, normally indicating Diderot as author. For discussion of Diderot, see Todhunter 1865, 55, and Daston 1988, 318-19. (For reasons that are unclear to me, Daston does not

Let p and q be the credibilities (or probabilities of telling the truth) assigned to each of two witnesses separately. What is the probability, w, that a proposition is true given that both witnesses attest to it? Though Hooper does not explicitly state the following formula, his calculation proceeds in accordance with it:[8]

$w = p + q - (pq).$

If we assume for the sake of simplicity that the witnesses all have the same credibility level of p, then the formula may be written more generally for an arbitrary number k of witnesses as follows:

$w = 1 - (1 - p)^k.$

The rationale for the formula is apparently something like this: we obtain the probability that both witnesses testify falsely by simple multiplication, $(1 - p) \times (1 - p)$, and we then obtain the probability that both are telling the truth by subtracting the probability of falsehood from one. (Since they agree, both tell the truth if either does.)

Hooper's formula, if correct, makes corroborative testimony an extremely powerful affair. If each witness individually is exactly as likely to be right as wrong, so that $p = \frac{1}{2}$, the probability that all are right is ¾ if there are two of them, 7/8 if there are three, 15/16 if there are four, and so on. Even if each witness's credibility level is well below ½, the probability that what they agree in asserting is true can be very high if there are enough of them. For example if $p = 1/3$ and $k = 3$, $w = 19/27$, which is still more probable than not. Indeed, no matter how low the initial credibility level is (just so long as it is greater

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assimilate Hooper to Diderot, and she attributes to Hooper on p. 315 a formula inequivalent to those attributed to him by Todhunter and Earman.) The 8th edition of Richard Whately's *Elements of Logic* (1859) is quoted in Keynes 1921, 178-79, note 4. Finally, Cohen 1976 cites Ekelöf 1964 and commentary on Ekelöf by Edman 1973.

[8] This formula is ascribed to Ekelöf by Edman, and the more general formula just below it to Hooper by Earman.

than 0), the final probability can be brought as close as you like to 1 by making the number of agreeing witnesses large enough.

One may suspect that there is something wrong with so powerful a formula, and indeed there is. Earman (2000, 54) and Daston (1988, 319) both note that the rationale for the formula lends itself to inconsistent applications. If we say that the independence of two witnesses with credibility of 0.9 allows us to multiply $(1 - 0.9)$ x $(1 - 0.9)$ to get the probability that both speak falsely (which we then subtract from 1 to get .99 as the probability that both speak truly), then why not say that independence lets us multiply 0.9 times 0.9 directly to obtain the probability that both speak truly? But we get .99 by the first method and .81 by the second.

Worse, the formula itself may be use to generate inconsistent results (as pointed out in Cohen 1976). Suppose that three witnesses with credibility ½ each testify that the criminal was male. The formula yields the result that the probability that the criminal was male given the conjoint testimony is 7/8. But what now of the probability that the criminal was female? That, too, is presumably ½ given any one witness's assertion of maleness, and thus by applying the formula again, the probability that the criminal was female given the conjoint testimony is also 7/8.

Despite its Phoenix-like properties, then, the Hooper formula should be laid to rest.[9]

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[9] George Boole, in criticism of Whately, offers this diagnosis of the error behind the formula: "A confusion may here be noted between the probability that a conclusion is proved, and the probability in favour of a conclusion furnished by evidence which does not prove it" (quoted in Keynes 1921, 179, n. 4). Apparently Boole thinks Whately is giving the former, whereas what is wanted is the latter. If there is a probability p that Alice has proved a certain theorem and a probability q that Bert has proved it, we would indeed compute the probability that it has been proved by one or the other of them as $p + q - (pq)$. That is different, however, from the probability that the theorem is true given combined nonprobative evidence from each of them.

4. Boole's formula

I turn now to George Boole, who advocated a widely received solution to our problem in

the mid-19th century (1857). Boole addressed the problem as part of a more general

problem, sometimes known as the combination of evidence: given that we know the

probability of C given A and the probability of C given B, what is the probability of C

when both A and B are known to be present? Here A and B could be two symptoms of

disease C, or two testimonies attesting to fact C. In the case where C is a fact attested to

and A and B the facts that two witnesses each attest to it, here is what Boole has to say

(1857, 364):

> Let p be the general probability that A speaks the truth, q the general probability that B
> speaks the truth; it is required to find the probability that, if they agree in a statement,
> they both speak the truth. Now agreement in the same statement implies that they either
> both speak truth, the probability of which beforehand is pq, or that they both speak
> falsehood, the probability of which beforehand is $(1 - p)(1 - q)$. Hence the probability
> beforehand that they will agree is $pq + (1 - p)(1 - q)$ and the probability that if they
> agree, they will agree in speaking the truth is accordingly expressed by the formula

$$w = \frac{pq}{pq + (1 - p)(1 - q)}$$

How does Boole arrive at this formula? By a familiar version of Bayes's theorem,[10]

we have

$$P(\text{both speak truly, they agree}) = \frac{P(\text{both speak truly}) \times P(\text{they agree, both speak truly})}{P(\text{they agree})}$$

Now Boole assumes that agreement may be analyzed as "either both speak truly or both

speak falsely." This makes P(they agree, both speak truly) = 1, so the right multiplicand

in the numerator drops out. It also lets us rewrite the denominator so that we obtain

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[10] The simplest version of Bayes's theorem is P(H,E) = P(E&H)/P(E), sometimes offered as the definition
of conditional probability, but also obtainable from the general multiplication rule P(H&E) = P(H,E) x P(E)
if we divide both sides by P(E). To get the version of Bayes's theorem I am using now, rewrite the
numerator of the first version using the general multiplication rule. For another common version, to be
encountered below, rewrite the denominator of the second version using the theorem on total probability,
P(E) = [P(H) x P(E,H)] + [P(~H) x P(E,~H)].

P(both speak truly, they agree) = $\dfrac{\text{P(both speak truly)}}{\text{P(both speak truly v both speak falsely)}}$

We may rewrite the denominator again using the special addition rule:

P(both speak truly, they agree) = $\dfrac{\text{P(both speak truly)}}{\text{P(both speak truly)} + \text{P(both speak falsely)}}$

Finally, Boole assumes that the independence of the witnesses' testimony means that we can use the special multiplication rule to compute the probability that both speak truly (or falsely, as the case may be). Letting p = the probability that the first witness speaks truly and q = the probability that the second witness speaks truly, this gives us

$$\frac{pq}{pq + (1 - p)(1 - q)}$$

just as Boole says.

I shall comment presently on two of the assumptions entering into Boole's derivation. First, however, let us note its implications for the issue before us. The reader may readily verify the following claims by plugging various numbers into the formula.

Setting p and q each equal to 0.6, we get w = .69. That is, if we set A's credibility and B's each equal to 0.6, the probability that X is true given that they each testify to it is .69. More generally, if we plug in any numbers greater than 0.5 as p and q, w will be greater than the mean of p and q.   So far, we have an illustration of Lewis's contention that the agreement of witnesses can boost credibility.

What if we plug in values of p and q equal to or less than 0.5? If 0.5 goes in, 0.5 comes out; and if p and q are each less than 0.5, the output value is less than their mean. For example, if p and q are each 0.1, w is approximately .01. In other words, if the witnesses have what BonJour calls "negative credibility" (credibility less than 0.5), the probability of X given that they both testify to it is not enhanced, but diminished!

If Boole's formula is correct, then, Lewis is vindicated and BonJour refuted. In order for coherence to have any amplifying effect, we need initial credibilities or input probabilities greater than 0.5. With credibilities less than that, coherence makes things worse rather than better.

But *is* Boole's formula correct? I have two qualms about it.

First, "both speak truly or both speak falsely" is a faulty analysis of agreement. Agreement would normally be taken to mean that the witnesses say the same thing, or at least logically equivalent things. If they do say the same thing, it will of course follow as Boole says that both speak truly or both speak falsely, but the converse is by no means assured. One witness could be speaking truly about events in New York and the other about events in Ouagadougou without their "agreeing" in any sense however stretched. "Both speak truly or both speak falsely" is a sufficient condition for agreement only in the case where the witnesses are answering the same true-false question. That restriction may be a condition of the applicability of Boole's formula.[11]

Second, I suspect that Boole has made an incorrect assumption about what the independence of the witnesses amounts to. Why does he equate the probability that A and B both speak truly with the simple product *pq* (i.e., P(A speaks truly) x P(B speaks truly)) rather than with the product P(A speaks truly) x P(B speaks truly, A speaks truly), which would be dictated by the general multiplication rule? The answer, presumably, is that the two probabilities are independent, so that we need only invoke the special

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[11] In a perplexing discussion of this point on pp. 365-67, Boole acknowledges that agreement in truth value is not sufficient for sameness of fact attested to. But rather than adopting an assumption of true-false questions to the witnesses, as I have proposed, he says that his reasoning is meant to address only the case of agreement in his artificially wide sense. He says that that his formula "would express the true solution of the problem originally proposed, if it were permitted to neglect the circumstance that it is to the *same* fact that the testimonies have reference, and so to regard their agreement as merely an agreement in being true or in being false, but not in being true or in being false about the same thing."

multiplication rule. But the sense of independence required for use of the special

multiplication rule would be this: P(B speaks truly, A speaks truly) = P(B speaks truly). I

take it that when we speak of "independent" or noncollusive witnesses we mean

something different: something like P(B says X, A says X) = P(B says X), or perhaps

something more complicated (as I advocate in section 6 below). How does independence

in the "no collusion" sense lead to independence in the sense presupposed by Boole?[12]

In sum, in deriving his formula Boole has assumed a notion of "agreement" that is

faulty except under the unrealistic assumption of true-or-false questions to the witnesses,

and he may also have used an improper conception of what the independence of the

witnesses amounts to.

There is a further and larger problem with Boole's formula, which it shares with some

of the formulas discussed below: it presupposes an equal distribution of prior

probabilities among the alleged fact attested to and its alternatives. I say more about this

problem in section 8.

5. Blitstein's formula

Michael Blitstein has proposed a modification in Boole's formula that would get around

the true-false constraint (2001). The true-false assumption allows agreement to come too

cheaply: if both say "no" to the question "Did the defendant do it?", they will count as

agreeing, even if under further questioning they would give very different accounts of

who did it and how.   More impressive than agreement in their answers to true-false

questions would be agreement in their answers to multiple-choice questions, which in

effect is what Blitstein's formula reflects. More impressive still, of course, would be

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[12] I address a further worry about independence in Boole in note 00 (approx 27).

substantial agreement in answers to an essay question (provided it were not such as to raise suspicions about independence!), but that is harder to capture in a formula.

Blitstein actually generalizes Boole's formula in two ways: he allows for more than two witnesses (which Boole could easily have done all along),[13] and he allows for more than two choices in answer to questions. At the same time, he simplifies Boole's formula by assuming that all the witnesses have the same level of credibility, so $p = q$. This assumption can be dispensed with, but making it enables a more perspicuous presentation of Blitstein's ideas. Let k be the number of witnesses, each of whom has credibility p, and let n be the number of choices in the multiple-choice question that is put to them. Blitstein's formula can then be written as follows:

$$ w = \frac{p^k}{p^k + (1 - p)^k / (n - 1)^{k-1}}. $$

As in Boole's formula, the numerator and the left-hand summand in the denominator represent the probability that the witnesses will all give the same correct answer. (If $k = 2$ and $p = q$, then $pq = p^k$.) The difference comes in the right-hand summand in the denominator, which is meant to represent the probability that the witnesses will all give the same *incorrect* answer, whether owing to incompetence (mass cretinism) or mendacity (mass Cretanism). We arrive at this summand as follows. First, the chance that a given witness will choose a given false answer is $(1 - p)/(n - 1)$—the probability of his giving a false answer divided by the number of false answers there are to choose from. In effect, this is to assume that if the witness gives a false answer, he is randomly guessing from among the available false answers. Next, the probability that *all k* of the witnesses will choose a given false answer (for example, answer *b*) is our previous value

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[13] In fact, Boole does present a formula for an arbitrary number k of witnesses, which in the three-witness case would be $w = pqr/[pqr + (1 - p)(1 - q)(1 - r)]$.

raised to the kth power, $[(1 - p)/(n - 1)]^k$ (assuming independence in some sense that lets us use the special multiplication rule). Finally, the probability that all k witnesses will give the *same* false answer, though not any particular one—that is, the probability that all will say *b*, or all will say *c*, or all will say *d*, etc.—is by the addition rule the sum of (n − 1) terms of the form $[(1 - p)/(n - 1)]^k$ or, equivalently, $[(1 - p)/(n - 1)]^k(n - 1)$. This simplifies to $(1 - p)^k/(n - 1)^{k-1}$, which is the right-hand summand in Blitstein's formula, giving us

$$w = \frac{p^k}{p^k + (1 - p)^k/(n - 1)^{k-1}}.$$

This formula reduces to Boole's in the special case where n and k are each equal to 2.

In other cases, however, Blitstein's formula has dramatically different results from Boole's. In particular, we can let the input credibilities be significantly less than 0.5 and still get high final probabilities, provided either the number of choices or the number of witnesses is great enough. For example, if the witness credibility level is only 0.3, but there are five witnesses all giving the same answer from among five choices, the probability that they will be giving the correct answer is .79. It can be shown that for any initial credibility level no matter how low (just so long as it is greater than zero), the final probability of a claim (that is, the probability of the claim given that all the witnesses agree in making it) can be brought as close as one likes to 1 by choosing high enough values for n and k. In fact, we need only manipulate n or k alone: for any p and any k, the final probability of a claim can be brought as close to 1 as one likes by choosing a high enough value for n; and for any p and any n (just so long as p > 1/n), the final probability of a claim can be brought as close as one likes to 1 by choosing a high enough value for k.

If Blitstein's formula is correct, then, BonJour is vindicated and Lewis refuted. Initial credibilities need not be high; they can be arbitrarily close to zero just so long as they exceed 1/n. They certainly need not have probabilities higher than their own negations.

The only constraint is that the witness credibility level must be *greater than chance* or the random guessing level. If there are ten answers to choose among and the witnesses are randomly guessing, their credibility level would be 0.1. Blitstein's formula lets coherence or agreement do its work just so long as the credibility level exceeds the chance level. With credibility less than chance, agreement has the effect of diminishing rather than raising final probability over its initial level, just as with the Boole formula.

6. Huemer's formula

By a somewhat different route, Michael Huemer has arrived at a formula equivalent to Blitstein's for computing the probability that what independent witnesses agree to is correct (1997). Since Huemer is quite explicit about all the assumptions entering into his derivation, it will be worth looking at his formula if only for the sake of scrutinizing some of these assumptions.

Huemer writes:[14]

For the sake of simplicity, let us suppose that there are two witnesses, Alice and Bert, and that they are reporting on the value of a certain variable, x. x can take on a number of different values—suppose that there are *n* possible values that x can assume, and that one of these possible values is x = 2. Assume that each witness is equally credible and that Alice's level of credibility is such that the probability that Alice will report the value of x correctly is p. Now let "X," "A," "B" . . . stand for the following propositions:
  X = The value of x is 2.
  A = Alice reports that the value of x is 2.
  B = Bert reports that the value of x is 2.

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[14] 1997, 465-66. I have changed Huemer's variable 'r' to 'p' for the sake of uniform notation throughout this essay.

What we need to determine is P(X,A&B), the probability of X given that Alice and Bert each report it. Huemer begins his derivation with the following instance of Bayes's Theorem:[15]

$$P(X,A\&B) \;=\; \frac{P(X) \times P(A\&B,X)}{P(A\&B,X) \times P(X) + P(A\&B,\sim X) \times P(\sim X)}$$

He then makes a number of assumptions that enable him determine the various values on the right-hand side above. I shall highlight three in particular.

Credibility. Suppose for a moment that the correct value of x is indeed 2, i.e., that X is true. What is meant by the English expression "the probability that Alice will report the value of x correctly"? Do we mean P(X,A), the probability that X is true given that Alice says so, or do we mean P(A,X), the probability that Alice will say so given that X is true? English is perhaps simply ambiguous on this point.[16] For now I simply note that although what Boole and some others have meant by "the credibility of the witness" is the former of these quantities, what Huemer initially denotes by it is the latter, P(A,X).[17]

Independence. Now is the time to inquire just what independence of the witnesses should mean. Earlier I made the preliminary suggestion that it should mean that the probability that one witness says what he does is not affected by the fact that the other says what she does, which is to say P(B) = P(B,A). But this is not quite right. What we really want (as pointed out in Cohen 1976) is that the fact that one witness says X and the

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[15] This is the third form of Bayes's theorem mentioned in n. 00 (now n. 10).

[16] As pointed out by Sobel (1987,168), Owen (1987, 198-99), and Earman (2000, 40-41), among others.

[17] On p. 465 Huemer says "Alice's level of credibility is such that the probability that Alice will report the value of x correctly is r," and on p. 467 he says that r = P(A,X). That is why I originally took him to identify the probability that Alice will report correctly with P(A,X). However, Huemer tells me that what he really meant by the phrase was an *un*conditional probability, namely, P(x = 1 & Alice says x = 1 **or** x = 2 & Alice says x = 2 **or** . . .**or** x = n & Alice says x = n). That this was Huemer's meaning was also conjectured to me by Tim Chambers. Huemer and Chambers have both shown me how to prove that under this meaning of the probability in question, it is *equal* to P(A,X). The main point for now is that "the probability that Alice will report the value of X correctly" is *equal* to P(A,X) for Huemer, even though that is not what he *means* by the phrase.

fact that the other also says X should not be connected with each other *other than*

through the truth of what is testified (66). It would be all right if Alice's saying Zeke did

it raised the probability of Bert's saying the same thing provided it came about as

follows: Alice and Bert are both credible witnesses in such a way that Alice's testimony

raises the probability that Zeke did it, which in turn raises the probability that Bert will

testify that Zeke did it.[18] To accommodate this possibility, we must add X as a

background condition in our definition of independence, defining it as $P(B,X) =$

$P(B,A\&X)$, that is, Bert's probability of testifying truly is the same as Bert's probability

of testifying truly given that Alice does. For parallel reasons, we must add a second

clause in which ~X is a background condition: $P(B,\sim X) = P(B,A\&\sim X)$, that is, the

probability that Bert will testify falsely is the same as the probability that he will testify

falsely when Alice testifies falsely. These are the two independence conditions used by

Keynes in his treatment of our problem, to be discussed below. They are also the

conditions used by Huemer and many other writers.[19,20,21]

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[18] For further discussion of this point, see Olsson 2005, 28, and Jeffrey 1987. Jeffrey puts it this way (392): "Independence of the two witnesses regarding H means that any dependency between [Alice's saying H and Bert's saying H] is accounted for by the dependency of each upon H." He shows that the simpler independence definition $P(B) = P(B,A)$ is out of place if either of the witnesses is somewhat reliable.

[19] That Alice's and Bert's answers are independent of each other is Huemer's assumption 4 on p. 466; what he takes this to mean in terms of the probability calculus emerges on p. 467.

Others who use the same independence assumption are Olsson and Shogenji (2004), Olsson (2005), and Bovens and Hartmann (2003).

An equivalent formulation of the two independence conditions (given the general multiplication rule) is this: $P(A\&B,X) = P(A,X)P(B,X)$ and $P(A\&B,\sim X) = P(A,\sim X)P(B,\sim X)$. This is Jeffrey's formulation.

[20] In a theorem about testimonial corroboration proved in Cohen 1976, the standard equalities are weakened to 'greater than or equal to' statements: $P(B,X)$ must be less than or equal to $P(B,A X)$ and $P(B,\sim X)$ must be greater than or equal to $P(B,A\&\sim X)$. In other words, there must be no negative influence between the testimonies if X is true and no positive influences between them if X is false.

In his own remarks on this topic, C.I. Lewis cites only one of these independence conditions: in effect, that $P(B,A\&\sim X)$ not be greater than $P(B,\sim X)$. See 1946, p. 344, p. 349, and p. 349n6.

[21] For discussion of the concept of independence in the related context of Condorcet's jury theorem, I refer the reader to Estlund 1994. Estlund states the import of the jury theorem (first published in 1785) thus: "On a dichotomous choice, individuals who all have the same competence (or probability of being correct) above 0.5, can make collective decisions under majority rule with a competence that approaches 1 (infallibility) as either the size of the group or the individual competence goes up." The isomorphism of Condorcet's results to Boole's will be obvious. The jury theorem presupposes that the voters satisfy an independence condition, which Estlund formulates as follows: the probability of A's voting *correctly* (not

Since probabilistic independence is symmetrical, our two conditions are equivalent to

P(A,X) = P(A,B&X) and P(A,~X) = P(A,B&~X).

Prior probabilities. What value should be assigned to P(X), the prior probability of X

or the probability that X is true antecedently to the fact that Alice and Bert testify to it?

Huemer in effect uses the Principle of Indifference, saying that if there are n possible

values for x to take, then each of them is equally likely to be the actual value. That

makes P(X) = 1/n.[22] For example, if there are ten possibilities for the final digit in a

license plate, the prior probability that it was 2 is 1/10. I return to this assumption below.

Other assumptions. Huemer also assumes, as Blitstein does, that Alice and Bert have

equal levels of credibility or reliability (his Assumption 1) and that if they answer

incorrectly, they choose randomly from the (n − 1) incorrect answers (his Assumption 3).

Finally, he assumes that "the chances of Alice or Bert reporting incorrectly are

independent of what the true value of x is" (his Assumption 5). I take this to imply that

the chances of Alice's saying anything other than 2 when the correct value is 2 are equal

to the chances of her saying anything other than 3 when the correct value is 3 and so on.

This together with Assumption 3 enables him to compute P(A,~X) as (1 − r) x (1/n − 1).

These assumptions enable Huemer to assign values to all the quantities in the instance

of Bayes's theorem above.[23] By the assumption about prior probability, P(X) is 1/n, and

P(~X) therefore 1 − 1/n. By the independence and other assumptions, P(A&B,X) = p^2

and P(A&B,~X) = [(1 − p) x 1/(n − 1)]^2. When we plug these values in and simplify, we

arrive at the following formula:[24]

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simply his voting as he does) equals the probability of A's voting correctly given that B votes correctly.
[22] This is his Assumption 2 on p. 466.
[23] I have noticed two typos in Huemer's derivation. On p. 467, line 2, 'assumption 2' (the independence assumption) should be 'assumption 4'. On p. 468, the rightmost plus sign in the denominator of the first equation should be a multiplication sign.
[24] Olsson (2005) proves the same formula on p. 26.

$$P(X,A\&B) = \frac{np^2 - p^2}{np^2 - 2p + 1}$$

In some ways, the strategy of Huemer's derivation is very like Blitstein's. By allowing n to be greater than two, Huemer allows the witnesses to be asked multiple-choice questions, not just true-false questions. He also computes the probability of a witness offering a given false answer in the same way as Blitstein—by assuming that all false answers are equally likely to be selected.

In other ways, Huemer's strategy is not obviously the same as Blitstein's. Huemer explicitly assumes that witness credibility = $P(A,X)$. He assumes the two independence conditions we have identified above. He assumes a value for the prior probability of X using the Principle of Indifference. Blitstein assumes none of these things explicitly, but he may assume some of them tacitly, as shown below.

In any case, the formulas of Blitstein and Huemer turn out to be equivalent in the two-witness case, which is the only case Huemer considers. They always yield the same output values for given input values of n (number of choices) and p (witness credibility), as the reader may confirm for himself. (A general proof of the equivalence of the Blitstein and Huemer formulas has been provided to me by Tim Chambers.)

Thus Huemer's formula, like Blitstein's, should vindicate BonJour and refute Lewis. Yet Huemer advertises his result as doing just the opposite. He says it vindicates Lewis-style foundationalism and refutes BonJour-style coherentism. How is this possible?

The answer, I think, is that Huemer has misconstrued BonJour's position, or at least adopted a construal of it different from mine. Huemer takes BonJour to be saying that "even if neither witness has any credibility at all, enough coherence will eventually render a belief in X justified" (469). He then proposes that Alice's having some initial

credibility amounts to this: "when Alice says X, *the probability of X goes up*" (469). In other words, the probability of X given Alice's testimony is greater than its prior probability: P(X,A) > P(X). In the language of confirmation theory, A incrementally confirms X. Putting these two things together, he takes BonJour's claim that "no antecedent degree of warrant or credibility is required" to imply that P(X,A) need be no greater than P(X). Now we have already seen that when there are n choices, Huemer sets the prior probability P(X) = 1/n. From these last two assumptions—P(X,A) = P(X) and P(X) = 1/n—Huemer is able to derive that P(A,X) = 1/n. If we now plug 1/n into Huemer's formula as the value of p (= P(A,X)), we get as output 1/n, the same as our input. Huemer concludes:

> Thus, we see that if X receives no confirmation at all from either A or B individually, then X receives no confirmation at all from A and B together. If neither witness has any independent credibility, then the correspondence of the two witnesses' testimony provides no reason at all for thinking that what they report is true. (469)

Why do I think that Huemer has misinterpreted BonJour? As quoted above, BonJour expands on his claim that "no initial credibility is required" by saying that the reports may initially have *negative* credibility:

> [Lewis's own example shows] that no antecedent degree of warrant or credibility is required. For as long as we are confident that the reports of the various witnesses are genuinely independent of each other, a high enough degree of coherence among them will eventually dictate the hypothesis of truth telling as the only available explanation of their agreement—even, indeed, if those individually reports initially have a high degree of *negative* credibility, that is, are much more likely to be false than true. (147-48)

This makes it clear that he means initial credibility may be *less than 0.5*. (Significantly, when Huemer quotes BonJour, he omits the lines following the dash.) To say that a witness has negative credibility in the sense of credibility less than 0.5 does not imply that her testimony does nothing to raise the probability of X above its prior level, for

P(X,A) can be less than 0.5 and still greater than P(X). On the point where I am taking Lewis and BonJour to disagree, then—must initial credibilities exceed 0.5?—it remains the case that Huemer's formula vindicates BonJour's answer of *no*.[25]

Before going on, I must address a feature of Huemer's exposition that may at first strike the reader as a curious inconsistency. In deriving his formula, Huemer equates Alice's credibility with the probability that Alice says X given that X is true, P(A,X). But in his paragraphs criticizing BonJour, he treats Alice's credibility as the inverse of this, P(X,A). For example, he says "let r [my p] be sufficiently low so that P(X,A) = P(X)" (469). Now a conditional probability and its inverse need not in general be equal, so it may seem that Huemer is equivocating on what witness credibility is to mean. It turns out, however, that under all the assumptions now in play, P(A,X) and P(X,A) must have the same value. Huemer assumes that P(X,A) = P(X) as a rendering of BonJour's "no credibility" assumption; he assumes that P(X) = 1/n; and he assumes in deriving his formula that P(A,~X) = (1 − p)/(n − 1), where p [Huemer's r] is set equal to P(A,X). It may be shown that under these three assumptions, P(A,X) = P(X,A).[26]

The upshot is that an apparent sleight of hand on Huemer's part—the substitution of P(X,A) for P(A,X) as the measure of witness credibility—is actually the manifestation of an equality that holds under special conditions.[27]

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[25] In note 4 on p. 67 of his 2005, Olsson says that what BonJour *should* have meant by "no credibility" is what Huemer took him to mean: P(X,A) is no greater than P(X). However, he interprets BonJour's actual words as I have. Huemer has suggested to me in correspondence that BonJour would have to construe "no credibility" as P(X,A) no greater than P(X) in order to distinguish his position from weak foundationalism.

[26] I first learned this from Tim Chambers, who provided me with the proof. I have discovered subsequently that the equality of P(A,X) and P(X,A) may be derived from just *two* assumptions. One is the already mentioned P(X) = 1/n; the other (I simplify for the moment by assuming that there are only two possible values of x) is P(A,~X) = P(~A,X), which holds in Huemer scenarios. See Appendix E for further details.

[27] This equality may explain something that would otherwise seem puzzling in Boole. It is to be assumed in Boole that "the probability that A speaks the truth" and likewise the probability that B speaks the truth are the conditional probabilities that X is true given that A reports it and given that B reports it—in our notation, P(X,A) and P(X,B). (The witness problem is a special case for him of the problem of the

Let us sum up what we learn if the formulas of Huemer and Blitstein are correct. In order for amplification by coherence to take place (that is, in order for final probabilities or probabilities given the conjunction of convergent testimonies to be higher than initial credibilities), initial credibilities need not be greater than 0.5. They need only be greater than chance (for Blitstein) or (what is the same thing for Huemer) greater than the prior probability of the hypothesis attested to. Moreover, amplification can indeed take final values as close as one likes to 1, even with initial credibilities below 0.5, just so long as either the number of choices or (in Blitstein's case) the number of witnesses is high enough.[28] So if the formulas of Blitstein and Huemer are correct, BonJour is right and Lewis is wrong.[29]

7. Olsson and Shogenji's model

I turn now to important recent work on our topic by Olsson and Shogenji (2004). They distinguish three theses stated or suggested in Lewis's discussion of memory and coherence. The *negative* thesis says that "if there is no initial credibility pertaining to reports, then congruence is not capable of amplifying probabilities" (22). The (bare) *positive* thesis says roughly the converse of this: if there is *some* degree of initial

combination of evidence, the two pieces of evidence being A and B.) He calculates the probability that *both* witnesses are speaking the truth by simple multiplication, taking himself to be authorized in so doing by an unarticulated independence assumption. But how are we to understand the independence of probabilities that are conditional on different conditions, such as P(X,A) and P(X,B)? This puzzle would not arise if he were multiplying the inverse probabilities P(A,X) and P(B,X). So perhaps we should assume—not implausibly—that in Boole scenarios, the conditions hold under which the probabilities P(X,A) and P(X,B) are equal to their inverses.

[28] Huemer does not discuss how high P(X,A&B) can go; he is mainly concerned to show that it can go higher than P(X) if the witnesses have some initial credibility. But his examples suggest that he is aware that it can go arbitrarily high if the number n of possible answers is high enough, and although he states his formula only for the two-witness case, he notes that amplification by coherence is greater with a larger number of witnesses.

[29] To be fair, I repeat that this verdict presupposes that the issue between Lewis and BonJour is what I take it to be: must the initial credibilities that are to be amplified by coherence be greater than 0.5? The verdict would be reversed—Lewis right and BonJour wrong—if the issue were what Huemer takes it to be: must the initial credibilities of witness reports be greater than the prior probability of what is reported, that is, must P(X,A) be greater than P(X)?

credibility, congruence *is* capable of amplifying probabilities. O&S distinguish two

further developments of the positive thesis. The *weak* positive thesis says

For any level of initial credibility, no matter how low just so long as P(X,A) exceeds P(X),	there is an extent of congruence, taken as going up with the number k of witnesses, such that	congruence of that extent will raise the posterior probability P(X,A₁&…& Aₖ) beyond any desired threshold for knowledge or action (short of certainty).

The *strong* positive thesis says

There is an extent of congruence, taken as going up with the number k of witnesses, such that	for any level of initial credibility, no matter how low just so long as P(X,A) exceeds P(X),	congruence of that extent will raise the posterior probability P(X,A₁&…& Aₖ) beyond any desired threshold for knowledge or action (short of certainty).

The boxes make it clear that the weak and strong positive theses differ just in the order of

the existential and universal quantifiers (as in 'everyone loves someone' and 'someone is

loved by everyone'). According to Olsson and Shogenji, the negative and weak positive

theses are both demonstrably correct. The strong positive thesis is *not* correct, but also

not essential for Lewis's purposes if one is willing to make certain other assumptions in

its place.

　　As we saw in the previous section, there is sometimes a question whether the

conditional probability that measures witness credibility should be P(X,A) or P(A,X).

O&S take it to be P(X,A), and they say that a witness report A has some positive initial

degree of credibility iff P(X,A) > P(X).[30] With these things understood, they represent

Lewis's negative thesis as follows:[31]

  If P(X,A) and P(X,B) are each = P(X), then P(X,A&B) = P (X).

In other words, if the testimonies of Alice and Bert (and any number of others we wish to

add) do nothing for X individually, then the conjunction of their testimonies does nothing

either. This result is the same as that reached by Huemer, as discussed in the previous

section, but O&S reach it by a simpler route. Huemer derives the general formula

$P(X,A\&B) = (np^2 - p^{2)}/ (np^2 - 2p + 1)$, then shows that Lewis's negative thesis follows

from it when for the value p of witness credibility we plug in 1/n, his value for the prior

probability of X. O&S derive the negative thesis directly from Bayes's theorem and the

same pair of independence assumptions used by Huemer.[32] A version of their proof is

given in Appendix E.

    I turn now to the weak positive thesis, which O&S state thus: "For any value n

[number of possible answers to the question put to the witnesses], any i [my p] > 1/n, and

any threshold of reliance t < 1, there is a finite number of independent reports [my k]

such that the posterior probability of X given the convergence of the reports is beyond t"

(2004, 31). In other words, for any level of initial credibility, no matter how low just so

long as it is greater than chance, there is an extent of congruence (which O&S take to go

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[30] On p. 21, they explicate "reports . . . have some positive initial degree of credibility" as "the existence of a report makes it somewhat more likely that what is reported be true." In other words, Alice's report has positive initial credibility iff P(X,A) > P(X). This could, perhaps, be taken as a holophrastic definition of what it is for a report to have initial credibility without any implication that credibility is to be identified with P(X,A). On pp. 30 and 33, however, they use *i* as a term for the initial credibility of a report and identify it with P(X,A).
    Elsewhere in the paper, O&S identify *i* or initial credibility as Huemer does with the inverse probability P(A,X)—see p. 29. This is potentially confusing to the reader. As we saw in the previous section, P(X,A) and P(A,X) are equal only under certain special assumptions, and O&S do not say that they are making these assumptions.
[31] 2004, p. 26. As always, I rewrite the variables for the sake of uniformity throughout this essay.
[32] 2004, pp. 26-27. They also assume that P(X) is neither zero nor one.

up as the number of concurring witnesses goes up (28)) that will raise the posterior

probability beyond any desired threshold. This is one of the theses we noted as a

consequence of Blitstein's formula in the antepenultimate paragraph of section 5.

O&S offer two ways of proving the weak positive thesis. In the first, they simply take

over the proof of Huemer's formula, endorsing the strategy of the proof while

generalizing the formula to cover any finite number of witnesses rather than just two.

After some algebraic transformations of the formula, they obtain[33]

$$P(X, A_1 \& \ldots \& A_k) = \frac{1}{1 + (n-1)[(1-p)/p(n-1)]^k}.$$

Inspection of this formula reveals that if p is greater than 1/n, the bracketed term in the

denominator is a fraction less than 1; hence, as k, the number of witnesses, increases, the

right hand factor in the denominator approaches zero; therefore $P(X, A \& \ldots \& A_k)$

approaches one.

O&S also show that there are numbers to plug into the formula just given to show that

the *strong* positive thesis false (31). I will say more about the significance of this

presently.

O&S then go on to offer their own second way of demonstrating the weak positive

thesis, which they believe superior to Huemer's. They propose, in effect, a two-stage

model for evaluating the probability of a purported fact X in light of concurring witness

reports. In the first stage, we evaluate the probability of various hypotheses about the

reliability level of the witnesses, given that they all agree. To simplify for the moment,

assume that there only two such hypotheses, R (= the witnesses always tell the truth, so

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[33] 2004, p. 30; the same formula also occurs in Olsson 2005, 32. Kenny Easwaran has pointed out to me that if you multiply the numerator and the denominator in the O&S formula both by p^k, you arrive at Blitstein's formula—further proof that the O&S/Huemer and Blitstein formulas are equivalent.

P(X,A&R) = 1) and U (= the witnesses are random guessers. so P(X,A&U) = P(X).). In

the second stage, we evaluate the probability of the attested fact X itself, given that the

witnesses agree and taking our probabilities for R and U into account. If Alice and Bert

were the sole witnesses, in the first stage we would calculate P(R,A&B) and its

complement, P(U,A&B). In the second stage, we would calculate P(X,A&B) by

calculating P(X&R,A&B) + P(X&U,A&B), which is an equivalent probability on the

assumption that R and U are exhaustive. A more fine-grained approach would consider

as well various hypotheses about the reliability levels of the witnesses intermediate

between R and U, but sticking just with R and U will more readily convey the gist of

their approach.[34]

O&S claim to find precedent for the two-stage approach in Lewis himself, basing their

attribution on the following passage:[35]

> For any one of these reports taken singly, the extent to which it confirms what is
> reported may be slight. . . . But congruence of the reports establishes a high probability
> of what they agree upon, by principles of probability determination which are familiar:
> on any other hypothesis than that of truth-telling, this agreement is highly unlikely.
> (1946, 346)

To generate their reading of this passage, they must take the hypothesis of "truth-telling"

to be the general hypothesis that the witnesses are fully reliable in their sense. For my

part, I think it more likely that Lewis simply meant that the testimony of the witnesses is

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[34] O&S call their scheme *dynamic*, because it allows estimates of witness reliability to be revised upward as
concurring testimonies accumulate. They characterize Huemer's framework as *static*, since its assignments
of reliability are fixed. I find the terminology of two stages versus one more suggestive of the differences
between O&S and Huemer.

[35] Bovens and Hartmann (2203) also attribute to Lewis an interest in the two-stage approach. In their
terminology, the two-stage approach is a matter of taking witness reliability to be an "endogeneous"
variable (Chapter 3).

true in the case at hand. In other words, by the hypothesis of "truth-telling," I think he only meant X and not R.[36]

Regardless of whether Lewis himself envisioned the two-stage approach, O&S tout the advantages of it. They claim that as the number k of witnesses increases, $P(X,A_1\&\ldots\&A_k)$ approaches one much faster on their "dynamic" model than on Huemer's "static" model (33-34).

There is still no guarantee that the *strong* positive thesis is true. Why would one want the strong positive thesis to be true? Well, as O&S understand Lewis, he holds that the degree of initial credibility attaching to reports of memory (and perhaps also to reports of witnesses) is *unassignable*.[37] It may be presumed to be positive (that is, greater than the prior probability of the fact attested), but its exact level is undeterminable. Therefore, O&S point out, with only the weak positive thesis at our disposal, we could know that there is *some* extent of congruence that would raise final probabilities above the chosen threshold for knowledge, but we would not know what that extent is or whether we had reached it. By contrast, with the strong positive thesis at our disposal, there would be an identifiable level of congruence that would raise final probabilities above the threshold no matter *what* the level of initial credibility, just so long as it is positive. So we would not need to know the initial credibility level beyond knowing that $P(X,A)$ exceeds $P(X)$ by some unspecified and possibly very small amount.

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[36] This would be implied in the quoted passage if by a high probability of truth telling Lewis meant a high probability of what the witnesses agree upon. A passage clearly favoring my interpretation of Lewis occurs at 1946, 352, where he says that a feature of corroboration through congruence is that the concurrence of the witnesses "would be improbable coincidence on any other hypothesis than that which is corroborated." That which is corroborated is the hypothesis on which all the witnesses agree—that is, X, not R. Moreover, when applying the witness model to ostensible memories, Lewis says that what emerges as more probable through congruence is the remembered content; he does not make a preliminary pass through the general reliability of memory.

[37] O&S cite 1946, 356-57.

Alas for Lewis, the strong positive thesis is false, as noted above. O&S therefore look for a way to assign initial levels of credibility after all. To do this, they need to assign a prior probability to the hypothesis that the witnesses are reliable—prior, that is, to hearing their testimony and learning to what extent they agree. For this purpose, they invoke the Principle of Indifference. "According to this principle, we should assign, in the face of ignorance, equal probabilities to all alternatives" (35). In a simple version of this strategy, they assume the witnesses are either perfectly reliable (R) or are randomly guessing (U), and assign to each of these two alternatives a probability of 0.5: $P(R) = P(U) = 0.5$.[38] They are then able to derive an initial credibility level by plugging in 0.5 for $P(R)$ where it occurs in the formulas of their two-stage model to obtain[39]

$$P(X, A_1 \& \ldots \& A_k) = 1/n + \frac{(n-1)n^{k-1}}{n(n^{k-1}+1)}$$

and then setting $k = 1$ in the formula just given to obtain

$$P(X, A_1) = 1/n + [(n-1)/2n].$$

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[38] In a more refined version of the strategy, they divide the interval between $1/n$ (the random-guessing level) and 1 (perfect reliability) into several equal-sized segments, then assign equal probability to each of these segments. They say that doing so "makes the model more realistic, but does not affect any of our results qualitatively" (36).

[39] To see where the formula that follows in the text comes from, recall that in the two-stage model, one first calculates $P(R, A_1 \& \ldots \& A_w)$, then calculates $P(X, A_1 \& \ldots \& A_w)$ as a function of $P(R, A_1 \& \ldots \& A_w)$. (For the space of this footnote, I have reverted to O&S's variable 'w' for the number of witnesses in order to facilitate comparison of what I say here with their own presentation.) O&S show on pp. 33-34 that

$$P(X, A_1 \& \ldots \& A_w) = 1/n + (n-1)/n \times P(R, A_1 \& \ldots \& A_w)$$
$$= 1/n + (n-1)/n \times \frac{r}{r + (1-r)/n^{w-1}}$$

where $r = P(R)$. Plugging in 0.5 for r, we get

$$P(X, A_1 \& \ldots \& A_w) = 1/n + (n-1)/n \times \frac{0.5}{0.5 + (1-0.5)/n^{w-1}}$$
$$= 1/n + (n-1)/n \times \frac{1}{1 + 1/n^{w-1}}$$
$$= 1/n + \frac{(n-1)n^{w-1}}{n(n^{w-1}+1)}$$

There is a typo in O&S's presentation of the last formula; they mistakenly have just 'w' as the exponent in the denominator. (Thanks to Lewis Powell for pointing this out to me.)

They note that this implies that $P(X,A_1)$ will be greater than $P(X)$ whenever n is greater than one, and that it gives us an exact value for $P(X, A_1)$. With an initial credibility level thus determined, they can then compute the value of k required to bring the posterior probability $P(X,A_1\&\ldots\&A_k)$ above any desired threshold. (That for any threshold there is such a value is what the weak positive thesis assures us.)

There are some things I find surprising about O&S's two-stage approach. In the formula just given, if we plug in n = 2, we get back $P(X,A_1) = 0.75$.[40] That means we should assign a probability of ¾ to the testimony of a witness of unknown reliability on a true-false question. Is that not overly optimistic?

O&S invoke the Principle of Indifference explicitly only as applied to the reliability and randomness hypotheses R and U in their derivation of initial credibility values. But they also make crucial use of the Principle of Indifference elsewhere without calling attention to the fact: they apply it to the attested fact, X, in order to establish the weak positive thesis. This comes out in several places in which they set $P(X) = 1/n$, where n is the number of possible answers to the question the witnesses are reporting on. In proving the weak positive thesis by way of Huemer's formula, they assume $P(X) = 1/n$ just as Huemer had. In proving the thesis by way of their own two-stage strategy, they again assume $P(X) = 1/n$ in several key places: at p. 33 in line 16, at p. 34 in the transition from proof line 3 to proof line 4 in the proof at the top of the page, and at p. 36 in line 2 (though here the context is not proof of the thesis but explication of positive initial credibility). I take the trouble to point out these additional instances of reliance on the

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[40] What is going on here stands out more clearly in Olsson 2005, 44 and 217. There he proves that in his model, $P(X,A) = P(R) + P(X)P(U)$. With $P(R) = P(U) = 0.5$ and $P(X)$ also $= 0.5$, we get $P(X,A) = .75$.

Principle of Indifference because the principle is controversial, as discussed in the next section.

Like Huemer, O&S take their results to favor Lewis over BonJour. Their negative thesis does indeed favor Lewis if the issue is whether coherence must operate on initial probabilities $P(X,A)$ greater than $P(X)$, but not if (as I have taken it) the issue is whether we require initial probabilities greater than 0.5. Their weak positive thesis also vindicates a further contention of Lewis's, namely, that enough coherence can bring final probabilities arbitrarily close to one. However, their proof of the thesis assumes something Lewis would repudiate—the Principle of Indifference.[41]

8. Keynes's formula

The Boole formula favors Lewis over BonJour; the formulas of Blitstein, Huemer, and O&S arguably favor BonJour over Lewis. Who is right? For further light on this question, I turn to the discussion of our issue by Keynes in his magisterial *Treatise on Probability* (1921). Keynes did not know, of course, about Blitstein, Huemer, and O&S, and none of them make any reference to him. But Keynes offers a critique of Boole that is applicable as well to Blitstein and Huemer. As noted above, Boole's formula is a special case of Blitstein's, and Keynes has criticisms that apply in the special case. So if Keynes's criticisms of Boole are correct, there must be something questionable about Blitstein's more general formula and the equivalent formulas of Huemer and O&S, even though they improve upon Boole. Moreover, Keynes offers a formula of his own for treating the witness question that is not equivalent to those of our contemporary writers.

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[41] Lewis's negative attitude toward the Principle of Indifference is perhaps best summed up in the following sentence: "[T]he real ground for assigning equiprobability of the alternatives is not the mere matter of logical division and symmetrical relation to the whole body of the data . . . but is the extensive body of past experience [concerning frequencies] which bears upon the point" (1946, 313).

It may therefore be that the outcome of the Lewis-BonJour dispute is decided only by a formula we have yet to consider.

Keynes begins by criticizing Boole's assumptions about independence, as I did above. In effect, he says that the conditions for using the special rather than the general multiplication rule to compute the probability that both witnesses speak the truth are not guaranteed to obtain simply by "causal independence" or absence of collusion between the witnesses (181).

Keynes then attacks the problem of the witnesses head-on. He develops his own formula for the probability of a statement X attested to by independent witnesses with credibilities measured by $P(X,A)$ and $P(X,B)$,[42] and he shows that this formula will yield the same results as Boole's only under an assumption he regards as dubious.

I give an expanded version of Keynes's derivation of his formula in Appendix E. Here is the penultimate line, translated into our notation:[43]

$$P(X,A\&B) = \frac{P(B,A\&X) \times P(X,A)}{[P(B,A\&X) \times P(X,A)] + [P(B,A\&\sim X) \times (1 - P(X,A))]}$$

With independence in the form I discuss above (implying $P(B,A\&X) = P(B,X)$ and $P(B,A\&\sim X) = P(B,\sim X)$), this takes the simpler form

$$P(X,A\&B) = \frac{P(B,X) \times P(X,A)}{[P(B,X) \times P(X,A)] + [P(B,\sim X) \times (1 - P(X,A))]}$$

Assuming that Keynes's derivation is correct (and I do not think there can be any doubt about that), Boole's formula can be correct only if it gives the same result as Keynes's. But Keynes goes on to show that under the correct assumption about what independence is, his formula and Boole's agree only under a certain highly dubious

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[42] That Keynes means $P(X,A)$ by the credibility of the witness is explicit on p. 181.
[43] Keynes writes 'a/h' for 'the probability of a given h'; I have rewritten this in the form 'P(A,H)' and suppressed his ubiquitous reference to background information.

assumption. Once again, I relegate the proof to Appendix E. The assumption is this:

$P(X) = P(\sim X) = \frac{1}{2}$. As Keynes puts it,

> This then is the assumption which has tacitly slipped into the conventional formula—
> that a/h = ~a/h = ½. [Translating into our notation and suppressing the background
> evidence h, this becomes : $P(X) = P(\sim X) = \frac{1}{2}$.] It is assumed, that is to say, that any
> proposition taken at random is as likely as not to be true, so that *any* answer to a given
> question is, *a priori*, as likely as not to be correct. Thus the conventional formula ought
> to be employed only in those cases where the answer which the "independent" witnesses
> agree in giving is, *a priori* and apart from their agreement, as likely as not. (182)

We can see that the assumption to which Keynes objects is indeed made by Huemer—

he explicitly sets the probability P(X) equal to 1/n, and thus in the two-choice case

discussed by Keynes, equal to ½. It is also made by O&S in their proof of the weak

positive thesis, though not in their proof of the negative thesis. Blitstein does not make

this assumption explicitly, but we may infer that (as Keynes says) it has slipped in tacitly,

since Blitstein's formula agrees exactly with Boole's in the two-choice case and with

Huemer's in the more general n-choice case.

How plausible or implausible is the assumption that P(X) = 1/n? This assumption is

an application of the classical Principle of Indifference, which is generally put in some

such way as this: if you do not have any evidence favoring some possible outcomes over

others, you should regard all the outcomes as equally probable. There are two worries

about relying on this principle in constructing a formula for the witnesses. First, the

condition for the applicability of the principle may not be met in the cases in which we

would like to use the formula. Before we hear the witnesses, we may indeed have more

evidence for X than we do for the competing answers (or conversely, more for one of

them than we do for X). In that case, we should not assign all the possible answers equal

prior probability.[44] Second, it has proved notoriously difficult to come up with a

formulation of the Principle of Indifference that does not lend itself (in the situations in

which it *is* applicable) to contradictory applications. Van Fraassen's cube factory is the

latest in a long line of examples illustrating the difficulty. Suppose our only relevant

knowledge about a certain cube is that it came from a factory that produces cubes with

edge length ≤ 2 inches. What is the probability that the cube has edge length ≤ 1 inch? If

we let the equiprobable ranges be $0 \leq L < 1$ and $1 \leq L \leq 2$, the Principle of Indifference

gives us an answer of ½. But a cube has edge length ≤ 1 iff it has face area ≤ 1, and

Indifference lets us treat as equiprobable the four area ranges $0 \leq A < 1$, $1 \leq A < 2$, $2 \leq A$

< 3, and $3 \leq A \leq 4$. So the Principle also gives us an answer of ¼. Contradiction.[45]

Keynes himself hoped that a contradiction-free formulation of the Principle of

Indifference could be found,[46] but he refused to take its applicability for granted in

witness scenarios. His objection to Boole might be put this way: Boole's formula makes

no provision for possible differences in the prior probabilities of the events being attested

to.[47] Of course, there is a sense in which Huemer does take account of prior probabilities,

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[44] Russell once observed, "When you meet a stranger, there are exactly two possibilities: on the one hand, he may be called Ebenezer Wilkes Smith; on the other hand, he may not. But in a long life, during which I have met a great many strangers, I have only once found the former possibility realized" (1948, 366). When we have statistical knowledge bearing on a question, it trumps any toting up of possibilities and assigning them equal probabilities.

[45] Van Fraassen 1989, Chapter 12. Van Fraassen seems to regard von Mises's problem about the ratio of water to wine in a certain glass as an even more intractable problem for the Principle of Indifference.

[46] He endeavored in Chapter IV of his *Treatise* to come up with an acceptable form of the Principle of Indifference. In his version, the alternatives to which we are to assign equal probabilities must not be divisible into further alternatives "of the same form" as the original alternatives. It is not clear to me how this stipulation would block contradictory applications in the cube factory case.

[47] Cohen (1976) mentions two of the objections I have raised against Boole, but for reasons that are unclear to me, he treats them as though they were one: "The specific trouble with Boole's formula is that it envisages a situation in which the domain of possibilities is a binary one. . . . The underlying trouble with the formula is that it takes no account of the relevant prior probabilities" (68). The formulas of Blitstein, Huemer, and O&S avoid the first objection to Boole, but they still incur the second.

since they occur in his formula, but by setting them all equal to chance, he allows them no real role.

Let us return now to our main question: must initial credibilities (the various probabilities that X is true, given that this or that witness says so) be greater than 0.5 before coherence can work its wonders? Blitstein, Huemer, and company say no (they would say yes only in the true-false case), but their formulas rest on the assumption of equal prior probability for X and its alternatives, which we have just called into doubt. What is the bearing on our question of Keynes's own formula, which makes no such assumption?

Despite being critical of the approach of Huemer, Blitstein, and company, Keynes delivers the same negative answer they do to our question—antecedent probabilities need not exceed 0.5 for coherence to amplify them. Here is his formula again:

$$P(X,A\&B) = \frac{P(B,X) \text{ x } P(X,A)}{[P(B,X) \text{ x } P(X,A)] + [P(B,\sim X) \text{ x } (1 - P(X,A))]}.$$

Suppose for the sake of example that P(X,A) = 0.4 and P(B,X) = 0.2 In that case, Alice's credibility in the Boole sense and Bert's credibility in the Huemer sense are both below 0.5. Suppose also that P(B,~X) is equal to 0.1. In that case, P(X,A&B) will come in above 0.5 at .57. Under the same assumptions about Alice and Bert, if P(B,~X) goes down to .05, P(X,A&B) will go up to .73, and if P(B,~X) goes down to .01, P(X,A&B) will go up to .93.

More generally, we can see that no matter how low P(X,A) is, the probability of X given that A and B both testify to it may still be very high. The right-hand side of the Keynes formula has the form ab/(ab + cd), and this may be brought as close as we like to

1 if we make c small enough. The role of c is occupied by P(B,~X)—the probability that the second witness would assert X if it were false.

So is Lewis wrong? There is a point to be made in his defense. To say that P(B,~X) is low is to say that the complementary probability P(~B,~X) is high. So although we assigned low credibility to the first witness (Alice), it may seem as though we are assigning high credibility to the second (Bert). It is not credibility as measured by P(X,B), for to say that P(~B,~X) is high is not to say that P(X,B) is high—conditional probabilities do not contrapose in that way.[48] Yet a high value for P(~B,~X) may still seem to represent *some* sort of favorable credit rating for the second witness.[49] So although Keynes's formula (the only one discussed so far in which I have complete confidence) seems to support BonJour, it does so only with the reservation just noted.

9. Taxicabs and miracles

Before I get to the point of this section, I ask any readers who have not encountered it before to test their probability intuitions on the following question:

> You have been called to jury duty in a town where there are two taxi companies, Green Cabs Ltd. and Blue Taxi Inc. Blue Taxi uses cars painted blue; Green Cabs uses green cars. Green Cabs dominates the market, with 85% of the taxis on the road. On a misty winter night a taxi sideswiped another car and drove off. A witness says it was a blue cab. The witness is tested under conditions like those on the night of the accident, and 80% of the time she correctly reports the color of the cab that is seen. That is, regardless of whether she is shown a blue or a green cab in misty evening light, she gets the color right 80% of the time. You conclude, on the basis of this information:
> ___(a) The probability that the sideswiper was blue is 0.8.
> ___(b) It is more likely that the sideswiper was blue, but the probability is less than 0.8.
> ___(c) It just as probable that the sideswiper was green as that it was blue.

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[48] That is one reason why conditional probability is not the same thing as the probability of a material conditional.

[49] A high value for P(X,B) is the probabilistic counterpart of what Sosa calls *safety*—X would (probably) be true if the witness said so. A high value for P(~B,~X) is the probabilistic counterpart of what Sosa calls *sensitivity*—the witness (probably) wouldn't say X if it weren't true. See Sosa 1999 for the distinction between safety and sensitivity and Roush 2005 for discussion of the probabilistic versions of these two virtues.

___(d) It is more likely than not that the sideswiper was green.[50]

This was one of the questions put to subjects in famous work by Tversky and Kahneman. T&K found that most people give either answer (a) or (b); in effect, they disregard the high prior probability that the cab would have been green. According to T&K's own reckoning, the correct answer is (d), and subjects who do *not* give this answer are committing the so-called "base-rate fallacy" or fallacy of ignoring prior probabilities.

An earlier well-known plea for the relevance of prior probabilities lies at the heart of Hume's case against belief in miracles. "I should not believe such a story were it told to me by Cato," he says, citing the Roman proverb as an illustration of his view that the prior improbability of an event may outweigh even a highly reputable authority (1748, 75).

I go into this issue because, as we have seen, Keynes's main criticism of Boole is precisely that he ignores prior probabilities in assessing the probability of a fact given concurrent testimony. If Hume and T&K are correct in their criticism of ordinary believers, we may have all the more reason to distrust Boole's formula and the others that imply it.

Yet the advocates of taking prior probabilities into account are not without their critics. In Hume's own day, his position against believing in miracles was vigorously opposed by Richard Price. To Price we owe the objection that if Hume's strictures were correct, it could never be rational to believe a newspaper's report of the winning lottery number, since the chances of an error or misprint in the paper are greater than the chances that

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[50] I take the problem in this wording from Hacking 2001, xvi, where I first encountered it. For the original authors' formulation of the problem, see Tversky and Kahneman 1977.

ticket #97 out of a million tickets actually won. Price maintained that the improbability

of a fact attested to (for example, that a ferry with a previous record of safe crossings has

sunk, or that a certain ticket was drawn in the lottery) should have no bearing on our

regard for testimony. He says that in the case of a newspaper "supposed to report truth

twice in three times, the odds of *two* to *one*, would overcome the odds of *thousands* to

one" (in Earman 2000, 164). He goes on to articulate the following thesis: "A given

probability of testimony communicates itself always entire to an event," undiminished by

the improbability of the event itself (167). In Appendix B I return to Price's lottery

example and his "communication" thesis; suffice it to say for now that Price would

apparently have given answer (a) to the quiz above.

In our own day, the judgments of ordinary folk on the blue-green question and other

similar questions (for instance, about the probability that you have a disease given a

certain diagnostic test) have been defended against T&K's allegations of fallaciousness in

Cohen 1981. According to Cohen, the 85% base rate for green cabs should not prevent

you from believing a witness who is correct only 80% of the time; nor should the low

antecedent probability that you have a certain rare disease prevent you from believing

you have it when an 80% accurate test says you do.

Who is right—Hume, Keynes, and T&K on the one side, or Price, Cohen, and the

average person on the other?[51]

 For many years, I considered myself a good Humean about miracles—I took it that

the improbability of a miracle would generally outweigh any testimony given in its favor.

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[51] The parallels between the Hume-Price debate and the T&K-Cohen debate are well brought ought in
Owen 1987.

Yet when I first encountered the taxicab question, I was one of those who thought the taxi more likely blue than green. Was I being unfaithful to my Humean principles?

Here (from Hacking, 72-23) is the Bayesian reasoning in support of T&K's answer of (d): Let G = x is green, B = x is blue, and *says B* = the witness says x is blue (where the variables range over taxis in the town). We represent the given information as P(G) = . 85, P(B) = .15, P(says B, B) = 0.8, and P(says B, G) = 0.2. What is the probability that a taxi is blue, given that the witness says so? By Bayes's theorem,

$$P(B, says\ B) = \frac{P(B) \times P(says\ B, B)}{P(B) \times P(says\ B, B) + P(G) \times P(says\ B, G)}$$

$$= \frac{(.15 \times .8)}{(.15 \times .8) + (.85 \times .2)} = 12/29 = .41$$

Correlatively, P(G, says B) = 1 − .41 = .59. "In spite of the witness's report, therefore, the hit-and-run cab is less likely to be Blue than Green, because the base-rate is more extreme than the witness is credible" (T&K 1977, 175).

There can be no faulting Bayes's theorem or that calculation employing it. I must say, however, that when I saw the justification for T&K's answer, I felt cheated by their construal of "gets the color right 80% of the time" as P(says B, B) = 0.8. I would have taken it instead to mean P(B, says B) = 0.8, and under that understanding, my answer is obviously more defensible.[52]

In the statement of the problem, the witness "gets the color right 80% of the time." Does that mean (1) 80% of the time when a taxi is blue, she says blue, or (2) 80% of the

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[52] Diaconis and Freedman (1981) accuse Cohen of the "fallacy of the transposed conditional"—the fallacy of switching from P(says B, B) to P(B, says B). I myself may have taken "gets it right 80% of the time" in the wrong or unintended way, but I did not switch from one reading to the other in midstream. Cohen, however, seems to have used P(says B, B) as the 0.8 item in doing the Bayesian calculation and P(B, says B) as the 0.8 item in saying why the calculation should be disregarded. See Cohen 1981, 328-29, and D&F, 333.

time when she says blue, the taxi is blue? Compare this with the case of a medical test

marketed as 80% accurate. Does that mean (1) that concerning people who have the

disease, the test is right (in indicating the disease) 80% of the time, or (2) that concerning

people whom the test indicates as having the disease, it is right 80% of the time? As

David Owen has trenchantly observed, "Any company that marketed such a test, and

claimed that it was 80% accurate, had better mean, by 80% accuracy, claim (2), or they

would, I suspect, be deluged by lawsuits."[53]

That point does not really settle which direction of probability ought to be used in our

calculations, however. As Owen also notes, there is a problem with taking P(B, says B)

as the given: if that is the given, what is the sought? What we are asked to *determine* is

P(B, says B), in which case that very probability is presumably not a datum of the

problem.

In discussing Hume on miracles, Keynes explicitly raises the following question:

"If a witness's credibility is represented by *x*, do we mean that, if *a* is the true answer, the

probability of his giving it is *x*, or do we mean that if he answers *a* the probability of *a*'s

being true is x?" (183) His answer is the former—in the notation I have been using,

P(A,X) in the Alice case and P(says B,B) in the cab case. He states no reason for this

verdict, but it appears to be simply this: if what is sought is P(X,A), as it is when we are

deciding whether to believe the report of a miracle, then what we need to plug into

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[53] Owen 1987, 198. Owen also observes (199) that it is charitable to assume that subjects in the T&K experiment took the 80% figure the first way. That was true in my own case, though in rereading Hacking's formulation of the problem—"regardless of whether she is shown a blue or a green cab in misty evening light, she gets the color right 80% of the time"—I can see a hint of the second way.

Here is the wording T&K presented to their subjects: "When presented with a sample of cabs (half of which were Blue and half of which were Green) the witness made correct identifications in 80% of the cases and erred in 20% of the cases" (p. 175). That adds a bit of information omitted in Hacking's statement of the problem—that the test sample was divided 50/50 between blue and green cabs. It follows that in the test situation, the probabilities P(says B, B) and P(B, says B), as measured by the frequencies, were *both* equal to 0.8. So given their own description of the background information, T&K were justified in setting P(says B, B) = 0.8 in their calculation.

Bayes's theorem is the inverse probability, P(A,X). Curiously, in discussing Boole's multiple witness problem just a few paragraphs earlier, Keynes explicitly takes witness credibility to be measured by probabilities running in the other direction—P(X,A) and P(X,B). Why this difference? Perhaps the answer is that in the multiple witness problem, what is sought is the probability of X conditional on the conjunction of two pieces of evidence, P(X,A&B), and in computing *that* probability by means of Bayes's theorem (see the previous section), one of the called-for inputs is P(X,A).

   I have gone into the debates about taxicabs and miracles because they highlight two questions that are important in this essay: (1) should the prior probability of a fact be taken into account in assessing the probability of the fact given testimony in its favor, and (2) what should be understood by such locutions as "the probability that a witness is telling the truth"—which direction of conditional probability is appropriate? My answer to (1) is yes, we should take priors into account.[54] My answer to (2) is that it depends on the problem. P(A,X) is the relevant input when we are trying to determine P(X,A), but P(X,A) may itself be a relevant input when we are trying to determine P(X,A&B). Certain English locutions, such as "Alice is right 80% of the time," are ambiguous, but perhaps more strongly suggestive of P(X,A) than P(A,X) as being 0.8; that may explain some people's apparent neglect of priors in the taxicab problem.

10. Real coherence

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[54] Incidentally, what is the proper Bayesian thing to do—taking differential prior probabilities of hypotheses into account, or distributing one's credences equally among the possibilities in accordance with the Principle of Indifference? You could perhaps do both (the former when your evidence favors one of the hypotheses and the latter when it does not), but which policy would make you a good Bayesian? According to some authors (e.g., Niiniluoto (1981, 349) and Daston (1988, 329 and 331)), Bayesians assume uniformly distributed prior probabilities. According to other authors (e.g., Sobel (1987,181ff.) and Williamson (2000, 196)), Bayesians base their calculations on differential prior probabilities. Such is the elasticity of the Bayesian label.

Although the agreement of witnesses on the stand has been called a paradigm of

coherence (Olsson 2005, 16 and 23), it is in one respect a drastically oversimplified

model of coherence. The agreement of the witnesses is literal identity, or at least logical

equivalence, of content: Alice says X and Bert says X, too.[55] But the coherence that

figures in epistemology is typically a matter of hanging together in a looser sort of way.

The coherence of ostensible memories is not their all being memories that P, for the same

P or something logically equivalent. Nor is the coherence of beliefs or cognitions more

generally like that. Rather, it is a type of coherence that is exemplified by the following

list of items:

  I seem to remember hearing a commotion last night.

  I seem to remember smelling a skunk last night.

  I seem to remember that the lid was on the garbage can when I went to bed.

  I seem to remember reports of a skunk in the neighborhood.

  I now see that the can has been knocked over and trash strewn about.

  There was a skunk here last night.

and so on. In other words, it is not identity or even equivalence of content, but rather

something like the relation Lewis calls congruence: a matter of each of the contents

being more probable given the rest than it is on its own.

    An unfortunate feature of congruence as defined by Lewis, however, is that it is an all

or nothing affair—a set is either congruent or not, period—whereas coherence is

normally thought to admit of degrees.[56] It is fortunate for our purposes that there has

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[55] Actually, in the derivations of our various formulas, it is not required that A = Alice says X and B = Bert says X. A and B could be any statements on which X has some conditional probability. But if B = Bert says Y, B's positive relevance to X will be unclear.
[56] This defect of Lewis's notion is pointed out by O&S on p. 25. One could, however, define within Lewis's framework in any of several ways a sense in which one set is more *nearly* congruent than another.

been a freshet of recent work devoted to developing probabilistic measures of coherence that do let coherence admit of degrees.

   One such measure is presented in Shogenji 1999. Although it has features some find counterintuitive, I shall discuss it as an example of the sort of thing that is needed to advance the discussion. Shogenji measures the coherence of a set of beliefs $\{B_1, \ldots, B_n\}$ as follows:

   $C\{B_1, \ldots, B_k\} = P(B_1 \& \ldots \& B_k)/P(B_1) \times \ldots \times P(B_k)$.

In other words, the coherence of a set of beliefs is the ratio of their conjoint probability to the product of their individual unconditional probabilities. To see how Shogenji arrives at this measure, consider first the case where $k = 2$. Shogenji assumes that $\{B_1, B_2\}$ is neutral—neither coherent nor incoherent to any degree—if and only if B_1 and B_2 are probabilistically independent, that is, neither does anything to raise or lower the probability of the other: $P(B_1, B_2) = P(B_1)$. (Since probabilistic independence is symmetrical, it is already implied and thus unnecessary to add that $P(B_2, B_1) = P(B_2)$.) If we choose to let the neutral point between coherence and incoherence be represented by the number 1, we may then define $C\{B_1, B_2\}$ as the ratio $P(B_1, B_2)/P(B_1)$. This ratio will be equal to 1 in the case of probabilistic independence; it will be greater or less than one in proportion to the degree to which B_1 and B_2 support or undermine each other. Shogenji's measure for $C\{B_1, \ldots, B_k\}$ is a generalization of this two-belief case.[57]

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[57] Shogenji points out that congruence in Lewis's sense is definable as minimal coherence in his: a set is Lewis congruent iff removing any member makes it less Shogenji coherent than it was before. A set can be Shogenji coherent without being Lewis congruent.
   I note that the converse is also true: a set can be Lewis congruent without being Shogenji coherent. For instance, consider the set {h1, h2, j1, j2}, where the h's and the j's are logically equivalent and so are the j's , but the h's and the j's are irrelevant to each other. In general, any union of Lewis congruent sets whose members are irrelevant to the members of the other sets will also be a Lewis congruent set, even though it may not be coherent by intuitive standards or by Shogenji's measure.

There is one feature of the Shogenji measure that some find objectionable. Suppose

we have a set of propositions in which each is logically equivalent to each of the others—

the ideal of coherence in older-style coherence theories, such as were espoused by the

British Idealists. Suppose in addition that the probability of each of these propositions is

1. In that case, the Shogenji coherence measure of the set will also be 1—the neutral

point between coherence and incoherence. And yet it seems to some that logical

equivalence ought to be the maximum of coherence, not (as in this case) the absence of it.
[58,59]

In their discussion of Lewis on memory, O&S cite Shogenji's measure of coherence

(though they do not presuppose it) and note that it captures two intuitions about what

congruence amounts to in the witness case. First, "congruence increases with the number

of agreeing witnesses;" second, "the degree of congruence is inversely related to the prior

probability of the supported hypothesis" (28). The idea behind the second point is that

agreement is more impressive when it is agreement on something more specific and thus

less probable, as in the multiple-choice case as contrasted with the true-false case. O&S

explain as follows why these two intuitions are captured by Shogenji's measure: if B_1

through B_k are all the same proposition, then $C\{B_1, \ldots, B_k\}$ as defined above is

$P(B_1)/P(B_1) \times \ldots \times P(B_k)$, which reduces to $1/P(B_1)^{k-1}$. Clearly, the value of this fraction

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[58] For a measure of coherence that makes mutual logical equivalence the maximum of coherence, as well as further criticisms of Shogenji, see Fitelson 2003. For criticisms of Fitelson's measure, however, see Meijs 2006. Meijs claims that no measure of coherence can satisfy both the desideratum that equivalence is the maximum of coherence and the desideratum that coherence is a matter of mutual support, as Fitelson's measure is meant to do. If this is right, one must either explicate coherence in terms of mutual support at the expense of the equivalence desideratum (as Shogenji does) or explicate coherence as a generalization of equivalence at the expense of the mutual support desideratum (as Meijs himself sets out to do).
[59] If the probability of each of the equivalent propositions is *less* than one, then the set will be the more Shogenji coherent the lower these individual probabilities are. This runs counter to a recurrent intuition of mine that coherence should be a function of relations among propositions that do not vary with the individual probability values of the propositions. But that intuition may be at odds with the very idea of trying to define coherence and other epistemic relations in terms of the probability calculus.

goes up as k increases (jibing with the first intuition); it also goes up as $P(B_1)$ decreases

(jibing with the second intuition).

From my point of view, to secure O&S's two congruence intuitions by identifying all

the B_i is precisely to forfeit the advantages of the Shogenji measure. What is involved in

real coherence (as opposed to literal agreement in the courtroom) is not identity, but

looser webs of probabilistic relations between disparate propositions. It would be nice to

see an application of the probability calculus to a witness problem in which the

"agreement" of the witnesses is their telling different stories that nonetheless somehow

reinforce one another.[60] Here lies a mostly uncultivated field.

What I am calling "real coherence"—probabilistic relations short of equivalence—

could, of course, be said to be exemplified in witness scenarios by the report-facts

themselves, even if the contents of the reports are identical. That is, one could take as

one's coherent set the set {A(lice says X), B(ert says X), C(harlie says X), etc}, perhaps

along with X.[61] Despite occasional ambiguities, I think that is how Lewis himself

understood the matter.[62] But what I would like to see investigated further is whether the

rationale for the credibility-boosting power of coherence among report-facts can be

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[60] Olsson 2005 takes a step in this direction with the example analyzed on p. 113-16: one witness attests that Robert was at the crime scene, and another attests that Robert had a million in cash the next day. However, nothing in the analysis seems to turn on how the testimony contents are related; everything seems to turn on whether certain specially defined independence relations hold.

[61] Branden Fitelson has informed me of the following fact (gleaned from his program PrSAT) about the numerical example I use in discussing the implications of Keynes's formula on p. 00, now 36: if we keep the values of $P(X,A)$ at 0.4, $P(X,B)$ at 0.2, and $P(B,\sim X)$ at .10 as in the example, then the extent to which A&B confirms X (as measured by the likelihood-ratio measure of confirmation) is proportional to the extent to which A and B are coherent according to either Shogenji's measure of coherence or his own measure of coherence.

[62] The ambiguities concern whether phrases like "items exhibiting these congruent relationships" refer to the contents of witness reports or the facts that the witnesses make the reports.

extended from the case in which the witnesses literally agree to cases in which the

contents of their reports merely cohere.[63]

11. An argument for moderate foundationalism

As discussed in the preceding section, the type of coherence of interest in epistemology is

typically not identity of content among various reports, but a tissue of looser relations of

probability. It is time to ask: what are these coherence-constituting relations of

probability founded upon, and how do we know that they obtain?

One answer was given by Russell: "It is only by assuming laws that one fact can

make another probable or improbable" (1948, 188). Perhaps Russell went too far in

requiring strict *laws* in order for one fact to make another probable, but it is plausible that

we at least require empirical statistical generalizations.[64] Where do these generalizations

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[63] Lewis gives the probabilistic rationale for the credibility-boosting power of coherence as follows: "[C]ongruence of the reports establishes a high probability of what they agree upon, by principles of probability determination which are familiar: on any other hypothesis than that of truth-telling, this agreement is highly unlikely; the story any one false witness might tell being one out of so very large a number of equally possible choices" (346). Earlier (238, n.8) he shows how the underlying principle is a consequence of Bayes's theorem (or the principle of Inverse Probability, as he calls it), rewriting the theorem to show that $P(X,E)$ approaches certainty as $P(E,\sim X)$ approaches zero. In the witness scenario, E is the conjunction of 'A(lice says X) & B(ert says X) & . . .'; Lewis claims this conjunction has low probability if X is false. The question I am recommending for further investigation is whether the same Bayesian rationale can be given when the report contents are coherent by some measure without being identical.

Lewis claims that strong confirmation of a hypothesis by some conjunction of its consequences takes place (in accordance with Bayes's theorem) iff the hypothesis and its consequences form a congruent set in his sense. Both sides of this biconditional are plausibly true in his example of the playing cards on p.345, but I cannot see that they must always stand or fall together.

[64] In Lewis's own theory of probability as developed in chapter 10 of 1946, all probabilities must be based on empirically given frequencies. Even if he is wrong about this and some probabilities are assignable *a priori*, it seems that the probabilities at issue in cases of real coherence—e.g., the probability that a skunk was here given the scattered garbage—could only be assigned on the basis of empirically given frequencies.

Bovens and Hartmann give the following as an example of a coherent information set: {The culprit is French, the culprit drove away in a Renault}. In calculating the coherence level of that set according to the probabilistic measure of coherence they are discussing, they assume such things as that most Frenchman drive Renaults (13). Nothing could better illustrate my contention that the probabilistic relations definitive of real coherence must rest on empirical generalizations

In one respect, however, Russell's principle—that only with the backing of laws can one fact make another probable—may go too far. For how would anything make the laws themselves probable? (Thanks to Michael Huemer for asking this question.) Here it may be necessary to assume that the premises of enumerative induction can make a generalization probable without the help of any prior empirical generalizations.

come from? Presumably, they are inferred inductively from particular facts gathered

together in memory. And now the following difficulty emerges: ostensible memories

give rise to knowledge only with the help of coherence; coherence depends on laws or

empirical generalizations; and such generalizations can be known only with the help of

memory. In short, we cannot get coherence without the help of laws, and if memory does

not suffice on its own to give knowledge of particular facts from which laws are inferred,

we cannot get laws without the help of coherence. It appears to follow that we cannot

have any knowledge from memory unless the occurrence of ostensible memories is

prima facie sufficient for knowledge. Such was Russell's own conclusion:

> [M]emory is a premise of knowledge. . . . When I say memory is a premise, I mean that
> among the facts upon which scientific laws are based, some are admitted solely because
> they are remembered. (188-89)

Note the word 'solely'. Russell is saying that individual memories must be capable of

giving rise to knowledge on their own, without benefit of coherence. This is compatible,

of course, with allowing that the warrant provided by memory is defeasible, as Russell

did allow. But the resulting view is nonetheless a foundationalism of memory knowledge

stronger than that of Lewis, who required only an initial "slight" presumption in favor of

the truth of any ostensible memory. Russell's view accords to memory greater epistemic

powers than that: ostensibly remembering that p is a source of *prima facie* warrant that,

if undefeated (and if p is true) is strong enough for knowing that p. In BonJour's terms,

we have advanced from weak to moderate foundationalism.[65]

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[65]In his 1977, Chisholm takes a position intermediate between Lewis and Russell. For Chisholm, having an ostensible memory of h confers upon h the status of "being beyond reasonable doubt," which is higher than "having some presumption in its favor" (the level correlating with Lewis's probability greater than 0.5), but lower than "being evident" (the level required for knowledge). On Chisholm's view, it takes *concurrence* (his brand of coherence) to boost memories to the level of warrant required for knowledge.

I anticipate three challenges to the Russellian argument. The first runs thus: "You are saying we need knowledge of laws before we can get knowledge of particular facts and knowledge of particular facts before we can get knowledge of laws, but so what? Aren't such circles what coherentism is all about?" Well, a plausible coherentism should not really endorse vicious circles; it should say instead that it avoids vicious circularity by going holistic and denying the relations of epistemic priority affirmed in the quoted speech. We believe particular facts and we believe general laws; when we have enough beliefs of each type, they morph into knowledge together. Light dawns magically over the whole.

I am not sympathetic to this objection, as it seems to me that it provides too easy a recipe for making circular reasoning legitimate. "Don't say you come to know P by knowing Q first and Q by knowing P first; say you know them both at a stroke once you believe them both." But in any case, the argument I am adapting from Russell is not targeted against the thoroughgoing coherentist. It is targeted against the weak foundationalist like Lewis, who accepts that knowledge of particular facts is prior to knowledge of laws, while holding at the same time that knowledge of particular facts is not delivered by ostensible perceptions and memories alone. Knowledge of particular facts arises only from ostensible perceptions and memories supplemented by coherence. But if coherence is a matter of probabilistic connections, and if such connections are based on laws, it follows that knowledge of laws is prior to knowledge of particular facts. And that *is* a problem.

The second challenge, to which I am also not sympathetic, invokes a subjectivist theory of probability. Where do the probabilities definitive of coherence come from? No

problem; they are simply a function of the agent's degrees of belief in the various propositions making up his doxastic system. They are there for the believing.[66] Here I can only report that I find the subjectivist theory too, well, subjective.

The third challenge, to which I *am* sympathetic, comes from externalist epistemology. Russell's argument assumes that coherence and the laws that underlie it contribute to our knowledge only if they are themselves known. One could challenge this assumption by holding that coherence is an *external* factor of knowledge—a factor that does its work regardless of whether the subject knows it to obtain. The most familiar example of an external factor is reliability in an epistemology like Goldman's (1979): if a subject comes to believe p as the result of a reliable process, his belief can thereby be justified and amount to knowledge regardless of whether he knows anything about the reliability of the process. What I am suggesting is that coherence might function in this externalist fashion, contributing to knowledge even if not itself known. Not many coherentists embrace this option, as most of them are moved to be coherentists in the first place by internalist considerations, but it is nonetheless an option to be considered. I take up an analogous externalist suggestion at the end of the next section.

12. Another requirement of initial credibility

The requirement of initial credibility we have discussed up until now is Lewis's requirement that what is reported must be more probable than not (and thus greater than 0.5) given that a witness (or an ostensible memory) reports it.[67] We have discussed the

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[66] This is not to say that you must have beliefs *about* probabilities; probabilities *are* your degrees of belief in various things, provided they satisfy the probability calculus.

[67] On Huemer's interpretation of Lewis, his requirement is that what is reported must have higher probability given the occurrence of the report than its prior probability. On Huemer's interpretation as well as mine, though, the requirement concerns how probable the content of a report must be given that the report occurs.

bearing of several probability formulas on this issue, some of them supporting Lewis's

position and others supporting BonJour's dissenting position. But however that debate is

resolved, there is another requirement of initial credibility we should consider now—a

requirement not about the probability of the *contents* of witness reports given that the

reports occur, but concerning instead the very *facts of their occurrence*. Even if Lewis

were judged wrong about the first requirement, there is a good case to be made that he is

right about the second.

Unfortunately, Lewis does not distinguish the two requirements, which threatens to

confuse the overall dialectic. Throughout most of his chapter on probable knowledge and

the validity of memory, his requirement of initial credibility is the former requirement—

that report contents be probable given report facts.[68] However, when we get to section 9

of his chapter, entitled "Congruence by itself is not sufficient to validate belief," he

enunciates the second requirement (concerning the report facts themselves) without

noting that it is a requirement distinct from the first. Here is a passage in which I have

numbered the sentences:

> (1) The feature of such corroboration through congruence that should impress us, is the
> requirement that the items exhibiting these congruent relationships must—some of them
> at least—be *independently given facts* or have a probability which is antecedent. (2)
> There must be *direct* evidence of something which would be improbable coincidence on
> any other hypothesis than that which is corroborated. (3) The root of the matter is that
> the unreliable reporters do make such congruent reports without collusion; that we do
> find ourselves presented with recollections which hang together too well to be
> dismissed as illusions of memory. (4) The indispensable item is some direct empirical
> datum; the actually given reports, the facts of our seeming to remember; and without

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[68] See the litany of quotations in section 2. The passages from pp. 334, 346, 354, and 356-57 of 1946 say
explicitly that the requirement of initial credibility pertains to the probability of remembered contents given
that they are remembered. The passages from pp. 39 and 352 say that what has initial credibility are the
contents themselves. However, the latter two passages are consistent with the larger group if the
probability of a content is determined by conditionalization on the fact that it is remembered. It is Lewis's
view (to be discussed shortly) that the fact that an ostensible memory occurs is to be assigned probability 1,
and what has probability p conditional on anything with probability 1 thereby has an unconditional
probability of p.

that touchstone of presentation, relations of congruence would not advance us a step
toward determination of the empirically actual or the validly credible. (1946, 352-53)

The first sentence of this passage is perhaps ambiguous; what is required to have

antecedent probability might be either the report contents or the report facts.[69] But in the

second, third, and fourth sentences, Lewis is clearly saying that the requirement of

antecedent probability holds for the report facts—that the witness *does* say such and such,

or that I *do* ostensibly remember so and so. It is the second of these requirements that I

wish to discuss now.

Whatever requirement there may be for the probability of a report content given the

fact of the report, there must surely be at least a presumption that the report itself has

occurred. The fact that a group of testimonies cohere would hardly give them high

probability if there were reason to doubt that the testimonies had, in fact, been given. If

we had reason to suspect that the courtroom and all its proceedings were happening only

in a dream or a novel or an elaborately programmed Mission Impossible scam, the fact

that the ostensible reports hang together would count for little.[70]

Here, then, is one place where it seems a foundational requirement is clearly in order:

before coherence can elevate the epistemic standing of the contents of various testimonies

(be they the testimonies of witnesses or the testimonies of memory or the senses), there

must be good reason to believe that the various testimonies have in fact been given.

Indeed, Lewis himself famously maintained that nothing can be probable unless

something is certain, and among the certainties he included the facts that I do have this or

that presentation of sense or memory (1946, 186 and 333). His insistence on certainty is

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[69] What would be "independently given facts" are such facts as that the witness or memory attests such-and-such, which suggests requirement two. But the items "exhibiting these congruent relationships" could arguably be the contents of the reports, suggesting requirement one.

[70] In the scam scenario, are the reports really independent? Well, it is as true to claim that they are independent as it is to claim that they occur at all: both claims are part of the deception.

controversial,[71] but it seems to me that a good case can be made that there must at least be high intrinsic credibility attaching to the facts that such-and-such cognitive states, be they experiences, ostensible memories, or beliefs at large, are actually occurring in us. Surely it must be more probable than not that the reports actually occurred! I would be tempted to go further: we must *know* that the reports have occurred, with whatever consequences that implies for how probable the fact of their occurrence must be.[72]

I see only one plausible alternative to an assumption of high initial credibility (or even warrant sufficient for knowledge) at the foundational level, and that is the view that the promptings of sense and memory function as *external* conditions of knowledge. An external condition of knowledge is a condition that makes knowledge possible regardless of whether it is itself known. *Some* of the conditions of knowledge must be external in this sense or else Kp would require KKKKKp with an endless string of Ks—a point that internalist epistemologies are in perpetual peril of denying. Perhaps the facts that I have such-and-such ostensible perceptions or memories could function in this external way, contributing to my knowledge even if not themselves known. The idea would be that my ostensible perceivings and rememberings are facts whose mere obtaining confers credibility on their contents (and higher credibility when they concur). They are not

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[71] For discussion of Lewis's reasons for insisting that ultimate evidence must be certain, see Van Cleve 1977. In a nutshell, Lewis thinks that merely probable (or uncertain) evidence would always have to be evidence based on further evidence, so it is only *certain* evidence that can put an end to the regress of evidence based on evidence based on evidence *ad infinitum*. For an alternative view, see the chapter entitled "Probability Kinematics" in Jeffrey 1983. Jeffrey proposes a method ("Jeffrey conditionalization") for assigning probabilities in relation to newly acquired evidence without assuming that the evidence is certain (or has probability 1): we set $P_{new}(h) = P_{old}(h,e)P_{new}(e) + P_{old}(h,\sim e)P_{new}(\sim e)$.

[72] Williamson 2000 develops an account according to which propositions serving as evidence must be propositions both known to be true (E = K) and possessed of evidential probability 1. However, the requirement of probability 1 for the evidence seems to have no substantive implications for Williamson; it is merely a trivial consequence of his defining evidential probability as probability conditional upon one's evidence.

pieces of evidence on which I conditionalize, or from which I draw inferences, when and only when their own epistemic status is high enough.[73]

Coherentists no less than foundationalists may wish to consider "going externalist" at this juncture. A belief of mine is justified, according to the pure coherentist, iff it coheres with the other beliefs I have. Well, how do I know that *p*, *q*, and *r* do stand in coherence-constituting relations? And how do I know that *p*, *q*, and *r* are among the things I believe? An externalist coherentist will say that I need not know these things; they need merely be so in order for *p*, *q*, and *r* to be things that I know. But a coherentist who is uncomfortable with letting important conditions of knowledge be thus external may well feel pressure to move closer to a Lewis-style foundationalism, in which the facts that I believe this or remember that have a privileged epistemic status.[74]

13. Conclusions

Weak foundationalists maintain that coherence can amplify warrant or credibility, but only if some items have initial credibility from another source. Thoroughgoing coherentists maintain to the contrary that if that coherence can amplify warrant, it ought to be able to generate warrant on its own, without any requirement of initial credibility.

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[73] For purposes of the present contrast, what I mean by 'conditionalizing' is drawing the following inference: $P(h,e) = n$; $P(e) = 1$; therefore, $P(h) = n$. But it is also possible to understand conditionalizing in a noninferential way (as perhaps by Jeffrey himself)—as simply a matter of changing one's degrees of belief under the impact of new experiences.

[74] If I understand the evolution of BonJour's own thinking correctly, this is what happened in his case. In *The Structure of Empirical Knowledge* (1985), he espoused a thoroughgoing coherentism. Realizing, however, that it is crucial to internalist epistemology that the subject know which propositions he believes, and not seeing a satisfactory way to account for such knowledge within a coherentist framework, he moved in subsequent writings over to the foundationalist camp.

Why can't the coherentist say that beliefs to the effect that one has certain beliefs are themselves justified by their coherence? BonJour answers as follows: "[I]t is beyond any doubt viciously circular to claim that the metabeliefs which constitute the believer's grasp of his system of beliefs are themselves justified by virtue of their coherence with that system—even if the nonlinear view of justification articulated earlier is accepted in its entirety" (102). He goes on to explain that this is so because a grasp of one's system of beliefs is prior to the justification of those beliefs by their coherence relations to one another.

Who is right? Do we need initial credibility before coherence can work its wonders, and if so, how much warrant, measured in terms of probability?

C.I. Lewis's answer, as understood here, is that the reports of witnesses, memory, or the senses must have initial credibility greater than 0.5—that is, the probability of X (given that X is reported) must be greater than that of its own negation. Of the various formulas considered here, only Boole's supports Lewis on this point, but Boole may be faulted for restricting attention to true-false questions and assuming that any hypothesis the witnesses report on is just as likely to be true as false. Some of the other formulas considered here belie Lewis's claim, implying that with initial credibility less than 0.5, final probabilities can still go arbitrarily close to certainty. But these formulas, too, can be criticized for assuming that the prior probabilities of hypotheses are distributed equally. The only formula that makes no assumption about equal priors is that of Keynes. Under his formula, the agreement of several witnesses can substantially raise the probability of what they agree on even though some of the witnesses have initial credibility less than 0.5; however, this is possible only if others of the witnesses are unlikely to attest to X if it is false, and this seems to be a concession of sorts to Lewis.

In whatever way the foregoing issue is resolved, we must at least require initial credibility in the following sense: the probability of X given that X is reported must be higher than the antecedent probability of X. That is the moral of the "negative thesis" espoused by Lewis and variously proved by Huemer, Olsson, Shogenji, and others. Without initial credibility in this incremental sense, the agreement of several reports would do nothing to make the final probability of X higher than its probability given any of the reports singly.

The upshot of our investigation of the witness problem within the framework of the probability calculus, then, is that weak foundationalism is a stable and well-motivated position; it has no built-in propensity to collapse into pure coherentism. In allowing that coherence can amplify warrant, one does not thereby concede the possibility that coherence can generate warrant wholly on its own.[75]

We considered two reasons, however, for thinking that weak foundationalism must gravitate in the other direction, toward what BonJour calls *moderate* foundationalism. The first is Russell's argument that relations of coherence can be determined only if memory on its own is a source of warrant at a level adequate for knowledge. The second is Lewis's observation that the coherence of various reports will count for little unless we have warrant—arguably, at a level adequate for knowledge—for thinking that the reports have actually occurred. These considerations could be resisted by externalists, but if nothing else, they show that something is wrong with internalist versions of coherentism or weak foundationalism.

I will close by answering a question I have been asked by several readers or hearers of previous versions of this essay: why should independence be a condition for coherence to do its work? The question provides an occasion for renewing my suggestion in section 10 for the direction of future research.

Whether independence should be required depends on which of two coherence models is in play. In one model, coherence is a relation between contents, examined without regard to the provenance of the reports that carry the contents, and one wants coherence

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[75] Could any coherentists have been been influenced by the following line of reasoning? "If coherence can give you more warrant from less, it can also give you some from none, because every case of more from less *is* a case of some from none. The *difference* between the more and the less must be coming from nothing." No, it could be coming from the lesser amount! The quoted reasoning begs the question in the same way as Descartes's argument that the principle *there cannot be more reality in the effect than in the cause* follows from the principle *ex nihilo, nihil fit*.

to boost their overall level of warrant. This is what is involved in Chisholm's notion of concurrence.[76] Here independence is *not* wanted; indeed, independence of content is the antithesis of coherence. In another model, figuring in Lewis and most of the authors discussed here, we have coherence when a number of reports all have the same content, and we need to assume that the reports are independent if we are to draw conclusions about the enhanced likelihood of the agreed-upon content. What is needed at this juncture, I believe, is a model that combines features of the foregoing two: an independence requirement for the reports and a real coherence measure for their contents. This combination is clearly what we need for analyzing typical witness scenarios, and it may also prove useful in considering analogous scenarios involving the reports of memory and the senses.

A final note: although we have found no rationale within probability theory for the pure coherentist's position, there would be nothing to prevent a coherentist from saying that quite apart from the machinations of the probability calculus, it is simply *self-evident* that coherence is a self-sufficient source of warrant—nothing to prevent it, at any rate, unless our coherentist were of a mind to repudiate the very notion of the self-evident.

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[76] Chisholm defines concurrence in a manner reminiscent of Lewis's congruence: a concurrent set is one in which each proposition is confirmed by the conjunction of the rest. He then lays down two principles about the power of concurrence. Principle H says that a set of propositions that have some presumption in their favor can be raised by their concurrence with one another to the status of being beyond reasonable doubt; principle I says that a deliverance of perception or memory can be raised by its concurrence with a set of beyond-reasonable-doubt propositions to the status of being evident—the status required for knowledge (1977, chapter 4).

APPENDICES

A. A farrago of formulas

Let X be a statement attested to by two witnesses (Alice and Bert). Let A = Alice says X and B = Bert says X. We want to compute P(X,A&B), the probability that X is true given that both witnesses independently attest to it. In some of the formulas below, this probability is denoted by 'w'. Some of the authors make provision for more than two witnesses, in which case 'k' is used for the number of witnesses. The level of witness credibility (identified by some of our authors with P(X,A) or P(X,B)) is p for the first witness and q for the second; sometimes these are assumed to be the same, in which case 'q' does not appear in the formula. Some of the formulas contain an 'n' denoting the number of possible answers to the question put to the witnesses.

Hooper's formula (1699)

$w = p + q - (pq)$. More generally (and assuming $p = q$), $w = 1 - (1 - p)^k$.

No matter how low witness credibility is, just so long as it is **greater than 0**, w can be brought as close to 1 as you like by making the number of witnesses (k) high enough. Supports BonJour.

Boole's formula (1857)

$$w = \frac{pq}{pq + (1 - p)(1 - q)}$$

The value of w may again be brought arbitrarily close to 1 by increasing the number of witnesses, but only if p and q are **greater than 0.5.** Supports Lewis.

Blitstein's formula (2001)

$$w = \frac{p^k}{p^k + (1 - p)^k / (n - 1)^{k - 1}}$$

Just so long as witness credibility is **greater than chance** (1/n), w may be brought arbitrarily close to 1 by choosing high enough n or k. Supports BonJour when n > 2.

Huemer's formula (1997)

$$P(X,A\&B) = w = \frac{np^2 - p^2}{np^2 - 2p + 1}$$

$P(X,A\&B)$ can be higher than 0.5 even if p is less than 0.5; p need only be **greater than the prior probability of X**. By increasing n, the value of w can be brought arbitrarily high. Supports BonJour (even though Huemer took it to support Lewis).

Keynes's formula (1921)

$$P(X,A\&B) = \frac{P(B,X) \times P(X,A)}{[P(B,X) \times P(X,A)] + [P(B,\sim X) \times (1 - P(X,A))]}$$

$P(X,A\&B)$ can be very high **even if P(X,A) is low**—provided $P(B,\sim X)$ is low. Supports BonJour, but only equivocally, given that assigning a low value to $P(B,\sim X)$ is, in a way, assigning a high level of credibility to the second witness.

B. Condorcet, Hume, Price, and Boole

The following formula, sometimes attributed to Condorcet's work of 1785, is discussed by several authors in connection with Hume's argument against miracles:[77]

$$P(\text{e occurred, w says e occurred}) = \frac{pt}{pt + (1 - p)(1 - t)}$$

Here p = the prior probability of the event and t = the probability that the witness is telling the truth.

On first encountering it, I was struck by the fact that the *pt* formula has exactly the same form as Boole's formula, which I criticized in the text. Condorcet's formula may be shown (under certain assumptions) to follow from Bayes's theorem. Is the formula therefore correct? If so, does that vindicate Boole after all?

Schlesinger (1991) notes that we may obtain the *pt* formula if we start with Bayes's theorem in the form

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[77] They include Todhunter 1865, Venn 1888, Niiniluoto 1981, Sobel 1987, Owen 1987, and Schlesinger 1991. Sobel refers to the formula as the Hume-Condorcet formula—not because Hume offers any formulas, but because he insists that the probability of an event given testimony in its favor is a function of the antecedent probability of the event (*p*) as much as of the veracity of the witness (*t*).

$$P(e, w \text{ says } e) = \frac{P(e)P(w \text{ says } e, e)}{P(e)P(w \text{ says } e, e) + P(\sim e)P(w \text{ says } e, \sim e)}.$$

and then introduce *p* as an abbreviation for 'P(e)' and *t* for 'P(w says e, e)'. We also need

to assume that P(w says e, ~e) can be represented as $(1 - t)$—an assumption I shall

examine in a moment.

Before we discuss the derivation of the formula, let us consider an objection to the

formula itself, mentioned by Todhunter and echoed by several others, including Cohen

(1981, 329). Suppose a witness of considerable reliability, such that t = .99, announces

that ticket #267 has won in a fair lottery of 10,000 tickets. In that case, t = .99 and p =

1/10,000. Plugging these numbers into the *pt* formula yields a value of 1/102 for the final

probability of the witness's claim. Is it not intolerable that the testimony of a reliable

witness should in this way be "enormously depreciated"?[78] In effect, this is Price's

objection to Hume: that Hume's principles imply that it could never be rational to

believe a newspaper's report of a lottery drawing.

Let us now return to the derivation of the *pt* formula. It turns out that there is an error

in the formula as applied to lottery cases: we should not have equated P(w says e, ~e)

with $(1 - t)$ or .01. What is equal to .01 is the probability that the witness gives *some*

false answer when the winning ticket is not #267.[79] But P(w says e, ~e) is not *that*

probability; it is the probability that he will give the *particular* false answer he does (i.e.,

that ticket #267 won) when he gives a false answer. If we assume that he is as likely to

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[78] The phrase is Venn's on p. 415, summarizing Todhunter.

[79] "The event that indeed has a probability of 0.01 is that the witness is reporting falsely," says Schlesinger (97). Schlesinger does not explain his reasons for this, but we can see that he is right if we make two assumptions. By stipulation P(w says e, e) = t, so by the negation rule P(~w says e, e) = $(1 - t)$. In the case at hand, we may assume that the witness must attest to one of the possible answers, so ~w says e is equivalent to his reporting some number other than #267. Thus P(~w says e, e) is the probability of his reporting some number other than #267 when the number drawn in in fact #267. Let us also assume that the probability of his reporting incorrectly when #267 is drawn is the same as the probability of his reporting incorrectly when any other number is drawn. We then get that the probability of his giving *some* wrong answer when ~e is $(1 - t)$ or .01, just as Schlesinger says.

give one false answer as another, then to compute P(w says e, ~e) we must divide the

probability that he gives some wrong answer, which is 1/100, by the number of possible

wrong answers, which is 9999. So P(w says e, ~e) = 1/100 x 1/9999, or 1/999999.

When we plug *this* number into Bayes's theorem instead of (1 – t), the value we obtain

for P(e, w says e) is 0.99. So there has been no depreciation of the witness's testimony

after all; in fact, the final probability of the event is equal to the witness's initial level of

reliability.[80]

The foregoing analysis shows that under certain circumstances, Price's

"communication thesis"—"A given probability of testimony communicates itself always

entire to an event"—is exactly correct. By the same token, however, it may be used to

show that he was wrong to elicit from Hume's view the contrary consequence that one

could never rationally believe a newspaper report about the outcome of a lottery. The

calculation above, yielding P(e, w says e) = .99, is arguably just what Hume's implicit

Bayesian principles authorize in such cases.[81] Moreover, there are other circumstances in

which Price's communication thesis is *not* correct. Suppose the witness is reporting on

whether a black ball or a white ball has been drawn, those being the only possibilities. In

such scenarios, Price's communication thesis fails and Hume's depreciation thesis holds.
[82]

Let us return to the *pt* formula. Can anything be done to salvage it? Sobel has shown

that it can indeed be derived from Bayes's theorem, but under a different interpretation of

its variables from what we used above. He lets *p* be the prior probability of the

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[80] Similar analyses are given in Venn 1888, chapter 17, Sobel 1987, Earman 2000, and Diaconis and
Freedman 1981.
[81] So argues Sobel (179-80); see also Earman (49-50).
[82] See Venn, 411-12 and 415, and Earman, 50-53, for analyses of black-or-white ball scenarios. Earman
argues that miracles may be (though need not always be) assimilated to black-or-white cases.

hypothesis (as above), but for *t* he uses something more complicated than P(w says e, e), namely, P(w says e, e)/P(w says e, e)+ P(w says e, ~e), which Sobel calls a measure of the witness's reliability relative to *e*. Under this more complicated interpretation of t, Condorcet's formula is correct (169-71).

   And now we get to the rub: although Boole's formula has the same form as Condorcet's formula, it cannot be justified in Sobel's way. It cannot be supposed that Boole had anything so complicated as Sobel's *t* in mind as his *q*. Moreover, Boole's *p* and *q* are parallel variables, whereas Sobel's *t* is very different from his *p*.

   There are special conditions under which we may obtain a valid *pt* formula using the simpler P(w says e, e) value for *t*. In the lottery scenario discussed above, we could not justify setting P(w says e, ~e) = (1 – t). We *could* justify that move, however, if we had the equality P(~w says e, e) = P(w says e, ~e), for then P(w says e, ~e) = P(~w says e, e) = 1 – P(w says e, e) = 1 – t. Moreover, the equality P(~w says e, e) = P(w says e, ~e) holds under the following three assumptions identified by Sobel:

  (i) e and ~e are the only alternatives,

  (ii) the witness must testify to one of them (he cannot remain silent), and

  (iii) the witness is equally accurate about e and ~e.

The first two conditions secure the equivalence of *~w says e* and *w says ~e*. The third condition may be expressed as P(w says e, e) = P(w says ~e, ~e) or, equivalently under (i) and (ii), P(w says ~e, e) = P(w says e, ~e). The desired equality may now be obtained as follows:

  P(~w says e, e) = P(w says ~e, e)—by (i) and (ii).

  P(w says ~e, e) = P(w says e, ~e)—by (iii).

Therefore, P(~w says e, e) = P(w says e, ~e)—by transitivity.

Assumptions (i)-(iii) are not all met in the lottery scenario, but they are met in cases of the sort for which Boole's formula may have been intended—cases in which a witness must give answers to true-false questions and in which he is as likely to call a false proposition true as a true proposition false. So is the Boole formula right under these special assumptions?

I say *no*, for the formula may still be faulted for Keynes's reason: it takes no account of prior probabilities. The *p* variable in Condorcet's formula is the prior probability of the event attested to, whereas in Boole's formula, it is the credibility of the first witness. Prior probabilities do not enter into Boole's formula at all (or if you prefer, they drop out, having being assumed equal).[83]

* * * * *

I end with some observations on the bearing of Condorcet's formula on belief in miracles. Let us assume that you should believe in a miracle on the strength of testimony if and only if the probability of the miracle given the testimony is greater than the probability of the nonoccurrence of the miracle given the testimony—in other words, iff P(m, w says m) > 0.5. If we use the *pt* formula that holds under special conditions (with *p* for P(m) and *t* for P(w says m, m), we get the result that the break-even point (where P(m, w says m) = 0.5) occurs when $p = (1 - t)$.[84] For example, if the reliability of the witness is 0.9 and the probability of the event is 0.1, then the final or overall probability

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[83] In his n. 13, Sobel cites the 1911 *Encyclopedia Britannica* article "Probability" for what is apparently an attempt to derive Condorcet's formula (for the probability of an event given the testimony of a single witness) from a formula like Boole's (for the probability that two concurring witnesses have told the truth). To achieve this, the author treats nature herself as one of the concurring witnesses! In effect, the author is reinterpreting Boole's *p* variable as the prior probability of the event being attested to, which would remove my principal objection to Boole.

[84] See Owen, 191-92. He says in n. 7 that the *pt* formula is derivable from Bayes's theorem provided P(w says e, e) = P(w says ~e, ~e). I think he must also be assuming Sobel's conditions (i) and (ii).

is 0.5 and you should withhold judgment. But leaving t at 0.9 and letting p rise to 0.2, we get a final probability of .18/.26, which greater than 0.5, so you should believe the testimony. Again keeping t at 0.9 but letting the probability of the event drop to .05, we get .45/.995, which is less than 0.5, so you should disbelieve the testimony.

This finding might be thought to vindicate Hume, since it shows that the improbability of an event can outweigh the authority of a witness. But so far as possible numerical inputs go, it also shows the converse—that a great enough authority can outweigh the improbability of an event. Perhaps the only general moral that can be advanced is that of Earman: for any degree of witness credibility, there is a miracle story too improbable to be established by a witness with that degree of credibility; but at the same time, for any miracle story, no matter how improbable, there is a degree of witness credibility such that a witness with that degree of credibility would establish the story (52-53). A similar thesis presumably holds if we replace 'degree of witness credibility' by 'number of concurring witnesses'.

C. Three theorems on corroboration and confirmation

It is demonstrated in Cohen 1976 that under four assumptions to be stated below, the following conditional is derivable within the standard probability calculus:

   If $P(X,A) > P(X)$ and $P(X,B) > P(X)$, then $P(X,A\&B) > P(X,A)$.

This tells us that if each of two testimonies individually confirms X (in the sense of raising its probability over its antecedent level), then the conjunction of the two testimonies confirms X more than either of them does alone. As Cohen puts it, genuine corroboration (of one witness by another) is possible.

Cohen's conditional clearly implies the following conditional, which we might call the *bare positive thesis*:

  If P(X,A) > P(X) and P(X,B) > P(X), then P(X,A&B) > P(X),

This says that if each of two testimonies individually confirms X, so does their conjunction. It is roughly the converse of the "negative thesis" advanced by Huemer and also by Olsson and Shogenji, which says if there is *no* confirmation provided by reports singly, then there is none provided by the reports conjointly. The bare positive thesis is weaker than O&S's "weak positive thesis" because its consequent says only that P(X,A&B) will exceed P(X,A), telling us nothing about high P(X,A&B) might go absolutely.

If we include the two conditions in the antecedent of Cohen's conditional among his assumptions, we have the following six assumptions in all:

C1. P(X,A) > P(X)
C2. P(X,B) > P(X)
C3. P(A&B) > 0
C4. P(B,X) ≤ P(B,A&X)
C5. P(B,~X) ≥ P(B,A&~X).
C6. P(X,A) < 1

C3 tells us that there is some chance that Alice and Bert both testify as they do, while C6 tells us that there is room for the probability of X to be raised beyond the probability it has given Alice's testimony. C4 and C5 are weaker versions of the two independence assumptions discussed in section 6, with '=' replaced by '≤ ' in C4 and by '≥' in C5. Together they tell us that there must be no negative influence between the testimonies if X is true and no positive influence between them if X is false.

The consequent Cohen derives from C1-C6 is

CC. P(X,A&B) > P(X,A)

which is what Cohen means by saying that the testimony of the second witness corroborates that of the first.

The question naturally arises whether all six conditions are *necessary* for confirmation (i.e., $P(X,A\&B) > P(X)$) and corroboration (i.e., $P(X,A\&B) > P(X,A)$) to take place. Huemer 2007 argues that not all of them are necessary for confirmation; Schlesinger 1991 argues that not all are necessary for corroboration.

According to the negative thesis of Huemer and O&S, we cannot drop both C1 and C2, since if neither A nor B by itself confirms X, their conjunction cannot do so.[85] The negative thesis is demonstrable, but only if we make the standard independence assumptions $P(B,X)= P(B,A\&X)$ and $P(B,\sim X) = P(B,A\&\sim X)$. In his more recent work (2007), Huemer suggests that coherentists might block the negative thesis by rejecting these independence assumptions. We want to rule out collusion among the witnesses, to be sure, but Huemer suggests that this can be done merely by retaining the condition $P(B,\sim X) \geq P(B,A\&\sim X)$, which is Cohen's C5.[86]

Huemer also proves a theorem according to which testimonies that do not confirm X separately may nonetheless do so in conjunction, provided certain assumptions collectively weaker than Cohen's hold. The assumptions are

H1. $P(X,A) = P(X,B) = P(X)$
H2. $P(X)$, $P(A)$, and $P(B)$ are each > 0
H3. $P(B,A\&X) > P(B,A\&\sim X)$
H4. $P(\sim X) > 0$

and the consequent derivable from them is

HC. $P(X,A\&B) > P(X)$.[87]

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[85] Cohen himself notes that we could drop *one* of the two, say $P(X,A) > P(X)$, replacing it with $P(X,A) > 0$.
[86] Interestingly, when Lewis formulates independence conditions, he gives only C5. He never discusses the fuller conditions, nor gives any reason for not using them. See the earlier footnote on this, approximately n. 20.

Here it is stipulated (in H1) that A and B on their own do not confirm X, but are neutral with respect to it. Cohen's independence conditions and the stronger standard independence conditions have been replaced by the single condition H3, P(B,A&X) > P(B,A&~X), which says that the probability of Bert's agreeing with Alice about X must be higher if X is true than if X is false. Add the nonzero assumptions H2 and H4 and the consequent follows: Alice's and Bert's testimonies do together confirm X, even though neither does so separately.

Another alternative to Cohen's theorem using weaker assumptions than Cohen's has been proved by Schlesinger (1991, 145-62).[88] Schlesinger's hypotheses are

S1. P(X,A) > 0
S2. P(X,B) > 0
S3. P(B,A&X) > P(B,A&~X)
S4. P(X,A) < 1

and his consequent is

SC. P(X,A&B) > P(X,A) (and similarly > P(X,B)).

Note that Schlesinger's S3 is the same as Huemer's H3; both have hit upon the same weaker condition to use in lieu of independence of the witnesses. (As far as I know, Huemer is unaware of Schlesinger's work.) All Schlesinger assumes about P(X,A) and P(X,B) is that they be greater than zero, which is compatible with their being no higher than P(X) by itself. Schlesinger therefore comments on the import of his theorem as follows: "corroborated evidence [can provide] greater support to a hypothesis than

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[87] See the appendix to Huemer 2007 for the proof. Huemer actually has just three of the assumptions I list and a biconditional consequent, P(X,A&B) > P(X) iff P(B,A&X) > P(B,A&~X). Since I am presently interested only in the right-to-left direction of his biconditional, I have moved its right hand side out of the consequent and into the list of assumptions.

[88] The proof is given on pp. 155-57. Lewis Powell has called my attention to the following typo: line α on p. 155 should read (in Schlesinger's notation) 'P(R₂/R1&S) > P(R₂/S)'.

uncorroborated evidence [even when] uncorroborated evidence provides no support whatever" (158).

Schlesinger's theorem is stronger than Huemer's, since Huemer's may be derived from Schlesinger's, though not (as far as I can see) conversely. To see how the derivation would go, note that we can get all of Schlesinger's hypotheses from Huemer's: S1 and S2 from H1 and H2, S3 from H3 (they are identical), and S4 from H4 and H1. Therefore, if we assume Schlesinger's theorem and then assume Huemer's hypotheses for a conditional proof of Huemer's theorem, we can derive Schlesinger's consequent, which we may then rewrite as Huemer's consequent in view of H1. Q.E.D.

Nonetheless, Huemer's theorem may be the more interesting, since his conditions yield confirmation and not just corroboration. Schlesinger's consequent says that the two pieces of evidence A and B do more in combination for X than either does separately, but the combination might still fall short of confirming X (in the sense of raising its antecedent probability).[89]

Does Huemer's theorem offer any aid and comfort to BonJour? It does vindicate one of BonJour's contentions (as Huemer interprets him)—that reports that individually have no credibility (doing nothing to raise the probability of the fact reported) may combine to confirm a fact when enough of them agree. But to get this possibility, we must assume that the reports are *not* independent in the standard sense—else the negative thesis would be in play—and that they satisfy the special condition H3 (=S3) identified by Schlesinger

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[89] Let A = normal die *a* is thrown, B = normal die *b* is thrown, and X = a side with six dots showing lands uppermost. Assume that there are four possible setups that occur with equal frequency: (i) *a* alone is thrown, (ii) *b* alone is thrown, (iii) both are thrown, and (iv) neither is thrown, but a die with all six faces marked with six dots is thrown instead. In this scenario, I believe all of Schlesinger's hypotheses and his consequent would be true, but it would not be true that $P(X,A\&B) > P(X)$.

and Huemer. Huemer says he knows of no reason to believe that the special condition

holds in matters of epistemological interest.

Moreover, merely to show that a modicum of confirmation by the conjunction of

individually neutral pieces of evidence is possible is not yet to say anything about how

high the level confirmation might eventually go. Recall some of BonJour's more

ambitious pronouncements:

> It is simply not necessary in order for such a [coherentist] view to yield justification to suppose that cognitively spontaneous beliefs have some degree of initial or independent credibility. (147)

> What Lewis does not see, however, is that his own example shows quite convincingly that no antecedent degree of warrant or credibility is required. For as long as we are confident that the reports of the various witnesses are genuinely independent of each other, a high enough degree of coherence among them will eventually dictate the hypothesis of truth telling as the only available explanation of their agreement. (147-48)

It seems clear from such passages (especially in their dialectical context) that BonJour

thinks coherence on its own, without any initial credibility for reports, can eventually

provide a high degree of warrant—enough for knowledge. Nothing in the results of

Huemer or Schlesinger bears out that contention.

D. Concurring witnesses and the principle of the common cause

If several witnesses independently tell the same story, is it reasonable to assume that their

story is true simply because that is the likeliest cause of their agreement?

Wesley Salmon has used Reichenbach's *principle of the common cause* to offer

rational reconstructions of common patterns of reasoning in science and everyday life

(Salmon 1984). For example, in the early years of the twentieth century, Jean Perrin

argued that nothing but the hypothesis of the reality of molecules (which was not at that

time generally accepted even in the scientific community) could explain why scientists in

different countries using different methods to determine the value of Avogadro's number

(the number of molecules in a mole of any substance) all came up with similar values.

According to Salmon, Perrin's argument uses the principle of the common cause: "when

apparent coincidences occur that are too improbable to be attributed to chance, they can

be explained by reference to a common causal antecedent"—in this case, that there really

are molecules that the various investigators were all counting (158). He also illustrates

the principle by citing the testimony of independent witnesses: the agreement of

witnesses with no opportunity for collusion is too improbable to have happened by

chance, so it constitutes strong evidence that the witnesses are reporting a fact that all of

them have indeed observed (220-21).

Following Reichenbach, Salmon spells out the common cause principle using the

probability calculus. He says that A and B together with C form a *conjunctive fork* when

the following four conditions obtain:[90]

(1) $P(A\&B,C) = P(A,C) \times P(B,C)$

(2) $P(A\&B,\sim C) = P(A,\sim C) \times P(B,\sim C)$

(3) $P(A,C) > P(A,\sim C)$

(4) $P(B,C) > P(B,\sim C)$

He then formulates the principle of the common cause as follows: when events A and B

satisfying conditions (1)-(4) occur, we should assume that A and B have a common cause.

A and B might be bouts of gastrointestinal distress in members of a touring troupe and C

a shared meal of mushrooms, or A and B might be determinations of similar values for

Avogadro's number and C the actual molecular constitution of physical reality.

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[90] Pp. 159-60; it is also assumed that none of the probabilities involved are 0 or 1.

Could it be that our reasoning about witness reports is simply an application of the common cause principle? And is that principle vouchsafed by standard principles of probability? Though I was initially hopeful on these points, I now have doubts.

To apply Salmon's framework to the witness case, let A = Alice says X and B = Bert says X, as before, and let C be the truth of what they both attest to, X.

We may begin by noting that conditions (1) and (2) are equivalent to the independence conditions for witness testimony that we have used throughout this paper, $P(B,X) = P(B,A\&X)$ and $P(B,\sim X) = P(B,A\&\sim X)$.[91] We may note next that conditions (3) and (4) say that Alice and Bert are each individually more likely to assert X when it is true than when it is false. This is an assumption of initial credibility for the witnesses in the incremental sense, since $P(A,X) > (A,\sim X)$ iff $P(X,A) > P(X)$.

So far, so good, but now for my reservations. First, the principles of Boole, Keynes, and the others discussed in this paper all purport to be sheer principles of probability, derivable within the standard calculus. Salmon uses notions of probability to *formulate* the antecedent of his common cause principle, but the principle itself is evidently put forth simply as a posit; it is not offered as a theorem of the probability calculus. Second, upon looking closely at how the principle is formulated and reading the small print in Salmon, we see that his principle does *not* say that when A and B together with C satisfy conditions (1)-(4), we may assume that *C* is the common cause of A and B. We may only infer that *there is* a common cause of A and B, which might be something other than C (167-68). In the case at hand, this means that we cannot infer, using the common cause

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[91] This is because (1) and (2) are instances of the special multiplication rule, which can be used only when A and B are independent assuming C and also assuming ~C. We take for granted the nonzero assumptions mentioned above.

principle alone, that the truth of X is the common cause of the witnesses' independently attesting to it.

We *can* infer from (1)-(4) using the probability calculus alone that P(C,A&B) is higher than P(C), but we cannot infer that it is high enough to justify our believing it outright.

E. Assorted Proofs

1. Conditions under which P(X,A) = P(A,X)

To prove: if (i) P(X) = 1/n and (ii) P(A,~X) = P(~A,X), then P(X,A) = P(A,X).

For simplicity, I give the proof just for the case in which n = 2 case (i.e., the case in which there are just two possible answers, X and ~X).

$$P(X,A) = \frac{P(A,X)P(X)}{P(A,X)P(X) + P(A,\sim X)P(\sim X)} \qquad \text{by Bayes's theorem}$$

$$= \frac{P(A,X)P(X)}{P(A,X)P(X) + P(A,\sim X)P(X)} \qquad \text{when } P(X) = \tfrac{1}{2}, \text{ i.e., } P(X) = P(\sim X)$$

$$= \frac{P(A,X)}{P(A,X) + P(A,\sim X)} \qquad \text{by factoring and canceling}$$

$$= P(A,X) \qquad \text{when } P(A,\sim X) = P(\sim A,X). \text{ for then the previous denominator equals 1}$$

Huemer explicitly makes assumption (i), and assumption (ii) follows from his Assumption 5—"the chances of Alice or Bert reporting incorrectly are independent of what the true value of x is"—together with his unstated assumption that the witnesses must give some answer or other. Assumption (ii) also follows from the three Sobel assumptions identified in Appendix B.

2. The "negative thesis" of Huemer, Olsson, and Shogenji, as proved by Olsson (p. 218)

Assumptions:

(1) P(A,X) = P(A) and P(B,X) = P(B).
This is the "no initial credibility" assumption as formulated in Olsson's book. In Huemer and in the O&S article, it is formulated as P(X,A) = P(X) and P(X,B) = P(X). The two formulations are equivalent given the symmetry of probabilistic independence.
(2) P(A&B,X) = P(A,X)P(B,X)
(3) P(A&B,~X) = P(A,~X)P(B,~X)
These are the two independence assumptions.

To prove: $P(X,A\&B) = P(X)$

$$P(X,A\&B) = \frac{P(A\&B,X)P(X)}{P(A\&B,X)P(X) + P(A\&B,\sim X)P(\sim X)} \qquad \text{by Bayes's theorem}$$

$$P(X,A\&B) = \frac{P(A,X)P(B,X)P(X)}{P(A,X)P(B,X)P(X) + P(A,\sim X)P(B,\sim X)P(\sim X)} \quad \text{substituting in previous line with (2) and (3}$$

$$P(X,A\&B) = \frac{P(A)P(B)P(X)}{P(A)P(B)P(X) + P(A)P(B)P(\sim X)} \quad \text{substituting in previous line with (1)}$$

$\qquad = P(X) \qquad$ by factoring out $P(A)P(B)$ in the denominator, canceling, and noting that $P(X) + P(\sim X)$ sum to one.

3. How Keynes derives his formula for the probability that X is true given that two witnesses independently attest to it (with thanks to Tim Chambers for fleshing out the proofs in this section and the next)

$P(X, A\ \&\ B) = \dfrac{P(\ X\ \&\ (A\ \&\ B))}{P(A\ \&\ B)}$ by Bayes's Theorem in the form given in note 00

$= \dfrac{P(X\ \&\ B,\ A)\ \text{x}\ P(A)}{P(B,A)\ \text{x}\ P(A)}$ regrouping in the numerator and applying Special Multiplication to both numerator and denominator

$= \dfrac{P(X\ \&\ B,A)}{P(B,A)}$ dividing through by $P(A)$

$= \dfrac{P(B,A\ \&\ X)\ \text{x}\ P(X,A)}{P(B,A)}$ applying Special Multiplication to the previous numerator

$= \dfrac{P(B,A\ \&\ X)\ \text{x}\ P(X,A)}{P(B\ \&\ X,A) + P(B\ \&\ \sim X,A)}$ rewriting B as (B & X) v (B & ~X) in the previous numerator and applying Special Addition

$= \dfrac{P(B,A\ \&\ X)\ \text{x}\ P(X,A)}{P(B,A\ \&\ X)\ \text{x}\ P(X,A) + P(B,A\ \&\ \sim X)\ \text{x}\ P(\sim X,A)}$ applying Special Multiplication to the two summands in the previous denominator

$= \dfrac{P(B,A\ \&\ X)\ \text{x}\ P(X,A)}{P(B,A\ \&\ X)\ \text{x}\ P(X,A) + P(B,A\ \&\ \sim X)\ \text{x}\ (1 - P(X,A))}$ applying the Negation Rule to the rightmost expression in the denominator

This is the final term/fraction/expression reached by Keynes in line 8 on p. 182. There is one typographical error: his '$1 - x_2$' should be '$1 - x_1$', which translates into my notation as '$1 - P(X,A)$'. Telescoping the series of equalities, we now have

$$P(X,A\&B) \ = \ \frac{P(B,A\&X) \times P(X,A)}{[P(B,A\&X) \times P(X,A)] + [P(B,A\&\sim X) \times (1 - P(X,A))]}$$

Using the two independence assumptions I discuss in section 6, P(B,A&X) = P(B,X) and P(B,A&~X) = P(B,~X), we may now make three substitutions in the previous formula to obtain

$$P(X,A\&B) \ = \ \frac{P(B,X) \times P(X,A)}{[P(B,X) \times P(X,A)] + [P(B,\sim X) \times (1 - P(X,A))]}$$

4. How Keynes reveals the hidden commitments of Boole's formula

In Boole's notation, w = P(X,A&B), p = P(X,A) and q = P(X,B), so Boole's formula may be rewritten as

$$P(X,A\&B) = \frac{P(X,A) \times P(X,B)}{[P(X,A) \times P(X,B)] + [(1 - P(X,A)) \times (1 - P(X,B))]}$$

If this formula is to be correct, its right-hand side must be equal to the right-hand side of Keynes's own formula (which version?) above, in which case we must have

$$\frac{P(X,B)}{1 - P(X,B)} \ = \ \frac{P(B,A\ \&\ X)}{P(B,A\ \&\ \sim X)}$$

By the Keynesian independence assumptions P(B,A&X) = P(B,X) and P(B,A&~X) = P(B,~X), we may rewrite the right-hand side of the above equation as

$$\frac{P(B,X)}{P(B,\sim X)}$$

By Special Multiplication, P(~X & B) = P(B,~X)P(~X) and P(X & B) = P(B,X)P(X). Dividing through by P(~X) in the first of these equalities and by P(X) in the second, we obtain P(B,~X) = P(~X & B)/P(~X) and P(B,X) = P(X & B)/P(X). Substituting in accord with these last equalities in the preceding fraction, we obtain

$$\frac{P(X\ \&\ B)/P(X)}{P(\sim X\ \&\ B)/P(\sim X)}$$

Simplifying the preceding compound fraction, we obtain

$$\frac{P(X\ \&\ B)}{P(\sim X\ \&\ B)} \ \times \ \frac{P(\sim X)}{P(X)}$$

By Special Multiplication, P(X & B) = P(X,B)P(B) and P(~X & B) = P(~X,B)P(B). Substituting in accordance with those equalities for numerator and denominator in the left-hand fraction above and dividing through by P(B), we thereby obtain

$$\frac{P(X,B)}{P(\sim X,B)} \quad x \quad \frac{P(\sim X)}{P(X)}$$

Or, equivalently by the Negation Rule,

$$\frac{P(X,B)}{1 - P(X,B)} \quad x \quad \frac{P(\sim X)}{P(X)}$$

By the preceding chain of equalities, we have now shown

$$\frac{P(X,B)}{1 - P(X,B)} \quad = \quad \frac{P(X,B)}{1 - P(X,B)} \quad x \quad \frac{P(\sim X)}{P(X)}$$

which of course can happen only if P(~X) and P(X) are equal to each other and therefore to ½. Keynes may therefore conclude as he does:

> This then is the assumption which has tacitly slipped into the conventional formula—that a/h = ~a/h = ½. [Translating into our notation and suppressing the background evidence h, this becomes : P(X) = P(~X) = ½.] It is assumed, that is to say, that any proposition taken at random is as likely as not to be true, so that *any* answer to a given question is, *a priori*, as likely as not to be correct. Thus the conventional formula ought to be employed only in those cases where the answer which the "independent" witnesses agree in giving is, *a priori* and apart from their agreement, as likely as not. (182)

Acknowledgments

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