ISAAC LEVI

JAAKKO HINTIKKA

I agree with Jaakko Hintikka that the so-called "conjunction fallacy" of Kahneman and Tversky is no fallacy. I prefer a different explanation of the mistake made these authors (Levi 1985).

Experimental subjects are invited to rank a set of propositions about Linda with respect to how probable, credible or likely they are on the basis of information given about a 31-year Linda. The task is to rank the set of hypotheses.

Hintikka suggests that the experimental subjects think of the propositions as testimony of witnesses and have a prior view (relative to the background information contained in the sketch of Linda's character) of the reliability of witnesses who testify to T and who testify to T&F. A witness who testified to T would be judged less reliable than a witness who testified to T&F. Hence, so Hintikka claims, the probability of T conditional on the biographical information about Linda and the testimony of the unreliable witness who testifies to T is less than the probability of T&F conditional on the biographical information about Linda and the testimony of the reliable witness who testifies to T&F. So T&F is ranked over T.

If I have understood Hintikka's suggestion correctly, he has not succeeded in saving the rationality of the experimental subject. In the first place, nothing in the scenario suggests that the hypotheses in question are or ever have been the testimony of any witnesses. But suppose we waive that point and accept Hintikka's elaboration according to which the experimental subject takes one witness to have testified that T and another that T&F. So the total relevant evidence is now the background biography for Linda (E) and the testimony of both witnesses. If, as Hintikka, along with Kahneman and Tversky, seem to think, the experimental subjects are ranking the propositions on the list with respect to probability and these probabilities are posteriors conditional on the the total relevant information available, the comparison being required is a comparison using the same probability evaluation. To assign probability to T higher than the probability for T&F where these two posterior judgments are constituents of the agent's probability judgment in a single context is simply incoherent. If it is claimed that the contexts are different, then the pair of probabil-

ity judgements do not appear responsive to the question the experimental subjects are invited to answer. Hintikka's experimental subjects seem to be as incoherent in their probability judgments as those of Kahneman and Tversky.

Even so, I agree with Hintikka that the experimental subjects in the experiments under consideration do not behave irrationally.

The experimental subjects are offered data in the guise of the story about Linda. The task is to rank a set of propositions with respect to how "probable" or likely" the propositions are in the light of the data. I submit that the message that would be retrieved by the experimental subject here is that the ranking should reflect the experimental subject's judgment with respect to how well the several propositions are supported by the data in the sense that the "best" proposition to "adopt" is the best supported. It does not matter whether they are asked to evaluate probabilities. Presystematically, "probable" is every bit as equivocal as "support", "confirmed". We would have to be sure that the experimental subjects are evaluating probabilities obeying the calculus of probabilities and not some other index of support on the evidence. They are asked to *rank* the propositions. The better propositions are more worthy of their assent than the inferior ones. In the context, it is easy to see that experimetal subjects might rank the propositions according to some value that they might maximize in seeking to choose among the various hypotheses. It is not strange to think of choosing the best-supported hypothesis. Unless Kahneman and Tversky offer some evidence that experimental subjects do not think this way, they are failing to establish the allegation of fallacy.

Of course, apologists for Kahneman and Tversky might insist that some account of alternative ways of ranking hypotheses with respect to support be identified. The demand is easily met. There are many ways – some of them very well known to Bayesians.

When considering the question of evidential support in a sense that calls for maximizing support among the alternatives available, even orthodox Bayesians do not think of maximizing probability. Maximizing probability, as Peirce, James and Popper all know promotes trivial answers. Carnap and Savage knew it as well.

Bayesians suggest, instead, that inquirers maximize or should maximize some index measuring increase of the posterior probability over the prior. This could be $P(H/E) – P(H)$, $P(H/E)/P(H)$ or $\log P(H/E) – \log(P(H)$. These measures are to be expected to rank T&F over T.

I wish to emphasize that *Bayesian* philosophers and statisticians commonly use these measures. To ignore the possibility that experimental subjects might be proto Bayesians in this respect is one of several flaws

in the work of Kahneman and Tversky that has enabled them to make the hit parade. It is fervently to be hoped, that their success will not fool the committee that selects the Nobel Prize in Economics.

I myself do not want to recommend any of the measures of the increase in posterior over prior mentioned above as an index to be maximized in the choosing among hypotheses on data. The difficulty is this. Let H and H′ be equivalent given the total evidence B&E but not equivalent on the prior information B. Then P(H/B&E) – P(H/B) will not equal P(H′/B&E) – P(H′/B). But the inductive support accorded H and H′ relative to the total evidence should be equal because they are equivalent given the total evidence. There is no difference in the value of the information H and H′ add to B&E!

Many, many years ago, Hintikka and Pietarinen (1966) wrote a paper arguing that in choosing among hypotheses, one should choose the one maximizing expected epistemic utility. Their favored suggestion for a measure of expected epistemic utility was the difference between the posterior and the prior – a proposal I myself had floated in a discussion of Popper (Levi 1963). Subsequently I had raised the objection just levelled against the difference between posterior and prior (or ratio or difference of logarithms) as a criticism of my own earlier view as well as the view of Hintikka and Pietarinen (Levi 1967). Instead, I proposed a different measure of expected epistemic utility. It is represented by a function P(H/B&E) – qM(H/B&E) where the M-function is, like the P-function a probability measure formally. Its intended application is different. 1 – M(H/B&E) measures the value of the information added to B&E by H. q is an index of boldness and is restricted to values between 0 and 1.

This measure does satisfy the condition on equivalence of hypotheses that the difference between posterior and prior does not. And it is readily seen to be a generalization of the defective measure in terms of the difference between posteriors and priors.

I have no idea whether experimental subjects use my proposed measure of epistemic utility or not. I think they should use it or some improvement on it. But that is not my current concern. The measure I propose, like the measure favored by Hintikka and Pietarinen a long time ago, evaluates T&F over T. In general, there is an abundance of measures that do the trick.

On this construal of the responses of the experimental subjects, there is no fallacy in the use of expectation determining probabilities. And there is no equivocation among priors.

Is there anything to be said in favor of the discussion of Kahneman and Tversky? They have clearly not established the presence of a conjunction fallacy. But something else interesting may be going on.

Kahneman and Tversky report that failures of the multiplication theorem are also found in estimating relative frequencies in populations. And such failures are, indeed, fallacious.

These results lend no credibility whatsoever to their claims about Linda problems exhibiting "conjunction fallacies". But they may help support another somewhat more abstruse claim that Kahneman and Tversky are fond of supporting.

It is at least entertainable that both the estimation of frequencies and Linda problems are evaluated by crude rules of thumb of the sort that Kahneman and Tversky call "heuristics". They themselves insist that such heuristics would not normally be operative unless they gave good approximate answers to questions in some contexts according to acceptable normative standards of rationality. My suggestion is that the Linda example and medical diagnosis examples used to exemplify the conjunction fallacy are cases where the heuristics work well. The examples of estimation of frequency are cases where they work poorly.

My proposal does depend on an interpretation of the responses of experimental subjects just as do the proposals of Hintikka and of Kahneman and Tversky. Perhaps, there is some way to settle the controversy empirically that is implementable. I do not know. The advantage of my proposal is that it conforms nicely with modes of evaluation that Bayesians are traditionally prone to endorse. I am not urging anyone to be a Bayesian. However, Kahneman and Tversky seem sympathetic to Bayesian standards as norms of rationality. Within that framework, my suggestion seems the most charitable of the alternatives canvassed here. Finally, if it is true that experimental subjects use crude rules of thumb to answer problems such as the Linda problem, the heuristic that Kahneman and Tversky identify works excellently well with the Linda problem as I interpret it and poorly with the estimation of frequencies. In this respect, the account I give fits well with the vision of how heuristics work that these authors themselves have sketched.

What should matter to philosophers, however, are the standards of good reasoning that are being promoted. Kahneman and Tversky seem to have gone around lecturing at medical schools as to the dangers of the conjunction fallacy. In my judgment, if they were persuasive, they would be hazardous to our health.

Whatever differences there may be between Jaakko Hintikka and me concerning the diagnosis of the mistakes made by Kahneman and Tver-

sky, we do agree that they have made mistakes – mistakes that, given the influence they have had, need reciting over and over again.

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Department of Philosophy
Columbia University
708 Philosophy Hall
New York, NY 10027
U.S.A.
E-mail: Levi@columbia.edu