

ESSAY

Recent Responses to Survival Research

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Abstract — I examine and reject some recent objections to the evidence for belief in personal post mortem survival, and especially a few objections to the evidence for belief in reincarnation. The first objection we may call the ET Hypothesis; it seeks to provide a plausible anti-survivalist explanation to the reincarnation data in the 'richer' cases. The second is the ~~Psi~~ Hypothesis also offered as an alternative explanation for the reincarnation evidence (and other survival data). Unlike the ET Hypothesis, however, a discussion of this second objection does appear in my *Death and Personal Survival*. But it needs refurbishing. The third objection also proposes an anti-survivalist interpretation of reincarnation data, appears in Wheatley's recent review of my book, and derives from some comments made by A. J. Ayer. I examine this third alternative hypothesis for the reincarnation data in the course of offering a general reply to a number of Wheatley's comments in his recent review. Finally, I discuss again the objection that nobody should take seriously any of the so-called data for survival because so much of it has been a matter of simple delusion, error, fraud or hoax.

The ET Hypothesis

Imagine that there is a group of extra-terrestrials considerably more intelligent and technologically advanced than *homo sapiens* and who, purely for entertainment, manage to provide selected humans with special memories and cognitive skills that they would have had if they had lived earlier as people who had those memories and skills. In short, they recreate, for example, Napoleon's memory, sense of humor, and cognitive skills (including his ability to speak Napoleonic French) and implant them into young Ann Davis who, in suddenly becoming aware of these memories, then for obvious reasons mistakenly believes that she was in a past life none other than Napoleon himself. After all, in her mind, and in the mind of others also, she has all the memories we would expect Napoleon to have and she has the memories that only Napoleon could have. She remembers having lived as Napoleon and describes, for example, the details of the battle of Waterloo in the way that only Napoleon's military mind could describe it. Besides that, she can now speak in proper dialect Napoleonic French, which she demonstrably did not learn to speak in her lifetime as Ann Davis, and she typically says such things as "I was Napoleon in my last life" or "I remember living as Napoleon in my past life." When questioned, she asserts (to the sheer delight of our alien manipulators)

that there could be no better explanation for her having all these verified memories than that she is indeed the reincarnation of Napoleon. In short, we can easily imagine a hypothesis in which all the evidence that counts, or would count, for justifiably believing in reincarnation equally counts for this ET hypothesis. In fact, under the ET hypothesis, the evidence that we would count as evidence for reincarnation (assuming that the criterion for personal identity is having certain systemically connected memories) is more fundamentally empirical evidence that supports the ET hypothesis [1]. Under the ET hypothesis, all the evidence that counts for reincarnation counts more properly for the ET hypothesis. Is there anything wrong with the ET hypothesis as an alternative explanation for the reincarnation data?

To begin with, if we assume, (as I think we should) that empirical testability is a necessary condition for any hypothesis that seeks to explain human behavior, we will not succeed if we criticize the ET hypothesis by claiming that the hypothesis is not empirically testable. It certainly seems testable in principle. As we have just described the ET hypothesis, all the empirical evidence that supports the reincarnation hypothesis will count equally for the ET hypothesis. The only difference by way of testability is that confirming the ET hypothesis requires testing the claim that the cause of all the evidence supporting the belief in reincarnation roots in the activities of the extra-terrestrials. It is easy enough to imagine what would convince us of the ET hypothesis; but we would certainly need to wait until we could talk with the ETs at length and until they could show us exactly how it could be done. For obvious reasons, the ET hypothesis may not be actually currently testable, but it is certainly testable in principle. As it presently stands, however, any explanation of the reincarnation data in terms of the activities of extra-terrestrials would need to assume the existence of extra-terrestrials who are the causal agents producing the data that would otherwise support belief in reincarnation.

Note, incidentally, that the ET hypothesis is not an instance of a Cartesian evil demon hypothesis, which we shall discuss later. The latter asserts that there may be a being no less powerful than malicious whose sole function is to make us believe what is humanly undetectably false. In such a world whatever evidence one has for a thesis is actually evidence for its denial; and in such a world it is logically impossible that we could prove anything or that we could in principle establish anything about the world. Apart from the fact that such a hypothesis leads to certain clear contradictions (*e.g.* knowing that one knows nothing about the world is knowing something about the world), an evil demon hypothesis could not in principle be empirically testable and confirmable or falsifiable, unlike the ET hypothesis.

Will we be tempted to respond that the ET hypothesis is arbitrary, or *ad hoc*, because belief in extra-terrestrials is willful belief in what is merely logically possible, and there is no good independent evidence for there being such creatures anyway? If so, we can expect the advocates of the ET hypothesis to respond properly that, apart from the fact that the reincarnation hypothesis fits

the data in the richer cases, which the **ET** hypothesis also does, there is no independent empirical evidence for belief in minds that could reincarnate.

Advocates of the **ET** hypothesis might, of course, argue even more strongly that there is in fact good empirical evidence that there are extra-terrestrials; and there is certainly a compelling argument that the probability is pretty high that there must be extra-terrestrials, even if nobody has yet publicly confirmed their existence in any clear way. But, they will add, there is no such independent evidence favoring belief in minds that could reincarnate. So, from an empirical view, belief in aliens manipulating people's minds to produce data confirming the false belief in reincarnation, is actually a much stronger empirical hypothesis than belief in reincarnation, even though it need only be *as plausible* in order to undermine belief in reincarnation.

Moreover, advocates of the **ET** hypothesis will urge that the ET hypothesis is equally empirically falsifiable: whatever evidence falsifies belief in reincarnation will by implication obviously falsify the ET hypothesis, by simple *modus tollens*. If the **ET** hypothesis is true then it implies whatever evidence we would accept as necessary and sufficient for belief in reincarnation.

In the end, however, the problem with the ET hypothesis is basically that it is simply not *as plausible* as the reincarnation hypothesis. After all, we all pretty much know that what we would take as a necessary and sufficient condition for somebody being the reincarnation of Julius Caesar. Such a person would need to not only claim to remember having lived as Julius Caesar, but also that person would need to have many of the memories we would expect of Julius Caesar, some confirmed memories that only Caesar could have, and a limited number of other mental states or dispositions having to do with one's sense of humor, temperament, or non-verbal skills possessed by the previous personality [2]. At least this is what Derek Parfit claims, and I agree with him [3]. Parfit claims that this is what it would have taken to prove the existence of Cartesian mental substance, distinct from material substance, as we know it, and that could survive death; but he hastens to add that there is no such empirical evidence, and that is why belief in reincarnation is not justified along with the hypothesis that there is some basic aspect of human personality that survives bodily corruption. In short, what makes the reincarnation hypothesis so plausible is that the data in the richer cases is what we would have *antecedently* accepted as sufficient evidence for reincarnation [4]. I have argued in *Death and Personal Survival* that, in fact, we only need a certain number of rich memory claims, memory claims we would expect of Caesar and memories that only Caesar could have had. For the present discussion, however, I add something not necessary, but which surely, in conjunction with the latter will render the list of traits certainly sufficient for justifying belief in reincarnation [4]. It fits neatly our basic and intuitive sense of what constitutes personal identity over time, assuming, for many good reasons, that personal identity cannot be simply a matter of bodily continuity over time. So, there is a *prima facie* plausibility to the reincarnation hypothesis as an explanation of the data

in the richer cases because the content of the richer cases is precisely what we would expect or predict if we thought there was any evidence at all that would confirm the hypothesis of reincarnation.

The ET hypothesis, however, is crucially different in that it is by no means the hypothesis we would offer initially to explain the data in the richer reincarnation cases. Of course, if one's criterion for personal identity is hard-wired in terms of bodily continuity of some sort, then, of course, there is nothing one would accept as empirical evidence for belief in reincarnation or any other form of personal post mortem survival. And if one has good reasons for adopting some criterion of personal identity in terms of bodily continuity of some sort, she cannot accept any evidence for *post mortem* personal survival.

But, interestingly enough, it is just these richer reincarnation cases that *challenge* or test that materialist intuition because one has to offer alternative explanations in terms of highly speculative potential causes for the data, such as extraterrestrials, whose existence we cannot now confirm in any public way. If one is not dogmatic about one's materialism, the data in the richer reincarnation cases overwhelmingly suggests as the first plausible hypothesis that the subjects in these cases are indeed reincarnated persons. If one is dogmatic about one's materialism, there is no good explanation of the data, except to say we have no good explanation for this data and will need to wait until it can be explained, presumably in terms of causal mechanisms and processes more amenable to the intuitions of natural science as we now know it. Of course, that is not to explain the data; rather it is a refusal to do so because one just 'knows' that materialism must be true, and that personal identity cannot be a matter that commits us to some form of Cartesian dualism.

If asked, moreover, what empirical evidence we should accept for the claim that ETs are manipulating people into thinking they are reincarnations of various people who had lived earlier, we would doubtless come up with a suitable response. But claiming that such evidence in fact obtains would appeal to the existence of entities and processes that we cannot now plausibly claim exist. In this sense the ET hypothesis is an *ad hoc* explanation because it assumes what is quite questionable for a number of reasons. But if asked what empirical evidence we would accept for the claim that people sometimes reincarnate, we would come up with evidence of the sort that we actually find in the reincarnation cases and which are not manufactured for the purpose of proving reincarnation; and that is not to appeal at all, by way of an explanation, to entities whose existence we cannot now establish. That is ultimately why the reincarnation hypothesis is the more plausible explanation fitting these cases. It only assumes that personal identity cannot be solely and simply a matter of bodily continuity over time.

Moreover, this latter assumption is less an instance of begging the question against materialism and positions hostile to reincarnation than it is a simple matter of displaying our deepest intuitions about personal identity. And if there is nothing one would take as evidence that somebody is the reincarnation of

Caesar, then it is hard to see how one can avoid dogmatic materialism, which is maximally counter-intuitive when we examine closely the implications in terms of an adequate criterion for personal identity over time.

The only other objection to the above line of argument against the ET hypothesis consists in urging (as Stephen Braude has done in correspondence) that the so-called obviousness of what we would take as evidence both necessary and sufficient for belief in reincarnation is problematic because such a requirement is satisfied by certain cases of multiple personality disorder (MPD) which should in fact be distinguished from alleged cases of reincarnation. In other words, the data supporting belief in reincarnation is equally supportive of certain MPD cases in which the manifest personality demonstrates all the traits that would be essential and sufficient to warrant belief that the manifest personality is the reincarnation of some other person. Sometimes this same objection is made by those who think the criteria for reincarnation are in fact satisfied by particular cases of "spirit possession" which is distinct from both reincarnation and MPD cases. In short, even if we could dispose of the ET hypothesis, nevertheless there are these two logically distinct hypotheses that are equally good explanations for the data in reincarnation cases. And if one adopts Occam's principle of parsimony, one should adopt the MPD hypothesis because it does not require of us anything more mysterious than delusional behavior, a good dose of ESP, and an ability to convincingly dramatize fictive personalities. Or so it may well be suggested.

The quick response to this last objection is basically a matter of denying that the criteria for reincarnation are satisfied in cases of either MPD or Spirit Possession. In the first case, in no MPD do we find a manifest personality claiming to remember having lived as a certain person in the past, where the claims are attended with rich and verified memories of that past life, where the verified memory claims are memories that only the former person could have, memories the core subject of the MPD could not have access to in his/her current life; nor in such cases do we find a manifest personality speaking a language (or demonstrating a skill that can only be learned) proper to the historical figure the manifest personality claims to remember having lived as, and which the subject of the MPD has never learned in his or her current bodily existence. Indeed, if we did have a case in which all the evidence that is present in the richer reincarnation cases obtained when examining an emergent personality in a MPD case, then presumably we would conclude that the personality being examined is indeed a person in whom the specified earlier person has reincarnated. As it is, however, this sort of evidence does not occur, and so what we would require as evidence for reincarnation is quite different than what we would require for an instance of MPD.

Possession is quite different also. Although possession would be a form of reincarnation, nevertheless, as we noted briefly in [2], subjects in reincarnation-type cases, often, if not typically, identify their current life-experiences as continuous with a life they remember having lived in a different body at a prior

time. They say such things as "I remember that when I was so and so I was a woman, but now I am a man" or "I remember that when I was so and so, I was bitten by a snake on the left leg on July 4 in the early morning, and the foot turned blue before I died of that bite." In these cases, while involved in describing activities, personalities and events of the remembered past life, the subject is simultaneously aware of the events and persons the personality in the current life would be expected to know. He or she retains, rather than loses, a dispositional awareness and memory of the current personality as affected by the historical events experienced in the current body. In short, in typical reincarnation-type cases we do not see the total personality replacement phenomenon that occurs in typical possession-type cases. In the latter, the subject asserts an identity of self that seems to exclude consciousness of the memories and personality associated with his or her physical body.

Let us turn, and return, to a second alternative explanation of the data, namely, the explanation that appeals to Psi or Super Psi.

The PSI and Super PSI Hypothesis

In *Death and Personal Survival*, and elsewhere, I have discussed the psi hypothesis, or explanation, for data that would otherwise strongly support some form of personal post *mortem* survival [5]. But this psi hypothesis seems tenacious, and so I would like to revisit it and then advance the effort to show that reincarnation and survival data are not well explained by appeal to psi or super-psi. In fact, the psi hypothesis as a possible alternative hypothesis to explain the data in survival cases is an instance of Descartes' evil demon hypothesis, and is an appeal to a totally ad *hoc* and untestable (hence unverifiable and unfalsifiable) hypothesis. In contrast, however, the reincarnation hypothesis, as well as the personal survival hypothesis is considerably more plausible because quite empirically testable and falsifiable.

Steven Braude, along with Jule Eisenbud and others, has argued, in this journal and elsewhere, that an equally plausible explanation for data in reincarnation cases, for example, may well be that the subjects in these cases are not reincarnations rather are people who have these very special paranormal abilities allowing them to replicate the propositional and non-propositional skills that otherwise seem to support belief in reincarnation. On this view, for example, Bishen Chand's memories of his alleged earlier life as Laxmi Narain, including the memories that only Laxmi Narain could have had, could well be attributable to ESP, or at least it is not implausible to think that need-based psi is the cause of the data here and in similar cases. So, on this view, it is not implausible to think that Bishen, through some form of ESP, was able to acquire the memories of Laxmi Narain and then mistakenly believed, for purely psychological reasons based on deep need of some sort, that he was in fact Laxmi Narain. And Bishen's ability to impersonate successfully Laxmi Narain, even though he had never seen him, is also plausibly construed as a paranormal ability activated by strong need-based desire or stress. Similarly, in those rare

cases of active xenoglossy wherein the subject has not only the memories of a certain past person but also the non-propositional skills (such as speaking in a foreign language the subject has not learned and which the person who has allegedly reincarnated spoke fluently), the possible explanation offered by Braude for the subject having this propositional and non-propositional knowledge is that it is equally plausibly a function of ESP or psi. He sometimes calls it "super psi" (as opposed to "puny psi" or "dandy psi") and refers to it as ESP (or PK) on a grand scale. Thus, for Braude, we can as plausibly justifiably explain all the data in these richer cases as simply a product of a form of paranormal knowledge which, however interesting, is ultimately only an emergent and irregular property of brains under unusual circumstances [6].

He also believes that a careful psychological examination of such subjects is as likely to show an important explanatory and causal connection between the subject's ability to generate such data and deep psychological needs and motives not always obvious to either the subject or the investigators.

Apart from his claim that we know super-psi exists even though we cannot produce it at will in laboratory settings, one of Braude's reasons for taking super-psi seriously as a possible, or equally plausible, alternative explanation for data in rich reincarnation cases (as well as in out-of-body cases, certain mediumship cases, and richer apparitional cases), is that any denial of such a claim is in fact a matter of placing arbitrary limits on the extent and magnitude of psi when in fact we do not know enough about psi to justify such an imposition of limits on when where and how it works. He says:

Given our present state of ignorance concerning the nature of psi, we must (at the very least) entertain the possibility of extensive psi... In fact, ...the only way we could ever be entitled to insist that psi effects have inherent limits would be on the basis of a thoroughly developed and well-supported full-scale psi theory, one that embraces the totality of available evidence for psi (not just laboratory evidence), and explains why or how psi functions both in and out of the lab. But at present no decent theory forbids large scale or super-psi (most simply ignore it), and certainly no scientific study renders any form of psi improbable [7].

Braude goes on to assert, moreover, that arguments dismissive of super-psi as a possible alternative explanation of so-called survival evidence are severely defective. On this issue, he engages the following three standing objections to the existence of super-psi: (1) there is no evidence for the existence of super-psi; (2) there is evidence against super-psi, and (3) the super psi hypothesis is not falsifiable. In response to the first of these objections, Braude claims that it assumes we would know *super psi* if we saw it, and that this assumption is clearly indefensible because there need be no observable difference between a heart attack or a plane crash caused normally and one caused by PK (or super psi). The only difference may be in their unobservable causal histories (p. 29) [7]. He claims further that those who assert the first objection are also guilty of the more general methodological mistake of offering only theory-dependent

arguments, that is, of using arguments and data that presuppose the denial of super-psi.

In response to the second objection, Braude argues that, contrary to what some have asserted, we know of no established limits of super-psi and that large scale super-psi might occur surreptitiously in less contrived or ritualized human circumstances outside the lab (p.33) [7]. Finally, in response to the third objection, Braude admits that the existence of super-psi is not falsifiable, but he is quick to add that non-falsifiability is theoretically uninteresting and certainly does not undermine the super-psi hypothesis. On this last point he says:

Even if hypothesis H is non-falsifiable, there may still be other grounds for deciding between H and rival theses — for example, higher level pragmatic considerations concerning theoretic systematicity, explanatory fecundity, and conceptual cost. Besides, the non-falsifiability of an hypothesis may simply reflect the intractable nature of the phenomenon in question, rather than a theoretical deficiency, or the fact that the phenomenon does not exist. Widespread, large scale and inconspicuous *psi* would be the sort of phenomenon whose existence might never be conclusively demonstrated or disproved. But in that case we would have no choice to accept the cards dealt us by nature. It would be indefensibly presumptuous to insist that nature operate only in ways amenable to the preferred methods of science (p. 35-36) [7].

In fact if we rely rigidly on Popperian falsifiability and other analytical or theoretical techniques drawn from the 'hard' sciences, we will have to reject perfectly acceptable everyday hypotheses concerning the mental lives of ourselves and others. But not only do we constantly evaluate such hypotheses against competing hypotheses, our psychological survival depends on it. It is by means of such a process that we *reliably* determine whom to confide in, how to speak to other people (*e.g.* which issues to avoid, what tone to take), whom we can rely upon in times of stress, etc.. And, clearly, the ability to do this consistently requires a mastery of a certain kind of theoretical activity. Indeed, some of us are much better able than others to hypothesize about people's intentions, desires, needs, interests, capacities, etc. And although no such hypothesis is strictly falsifiable, we are highly justified on pragmatic grounds. That is demonstrated in the way they successfully guide our dealings with other people (p. 36) [8].

In response to the super-psi hypothesis, I agreed in *Death and Personal Survival* (pp. 52ff) that there is empirical evidence of the existence of super-psi, or *psi* on a grand scale, and that it may function in ways distinct from our current understanding of what the limits of ESP or PK may be. That of course is the striking point of Braude's *The Limits of Influence*. Moreover, doubtless, nobody should place any a priori limits on what forms *psi* may take in the future. That said, however, there is something very dissatisfying about offering *psi*, or some form of *psi*, as a plausible possible alternative explanation for the data in the richer survival cases, including reincarnation cases.

The main problem seems to be that, as Braude and others describe it, the existence of super-psi, or sneaky *psi*, as a causal agent in these cases is neither confirmable (because, as he says, we cannot empirically distinguish it from normal physical causes) nor falsifiable. Insisting that we should empirically

confirm the existence of *psi*, or sneaky *psi*, as a particular causal agent in some way before appealing to its existence to explain anything in particular is less a matter of erroneously assuming *a priori* limits on *psi* or *pk* than it is simply a request for some evidence of the causes cited in the proffered explanation. If a possible alternative explanation is not in any way empirically testable, as indeed it would not be if it could not be falsified or confirmed, there is no way the explanation could have any empirical validity. There would be no way to discriminate empirically between an explanation offered in terms of *psi* and one offered in terms of usual causal agencies in the world. We cannot know that Jones is the robber of the Rabun Gap Bank if we cannot distinguish between Jones and Brown who might equally well have robbed the Bank. Moreover, if no empirical evidence (that is, public sensory evidence implied by the truth or falsity of hypothesis) could ever count either for or against Jones' robbing the bank, and if the supposition that Jones is the robber has no other testable implications, we might just as well claim that an angel robbed the bank on a whim, or that God did it. Its total lack of testability in terms of some distinctly empirical data that would allow us to adjudicate between it and any competing alternative hypothesis is what renders the *psi* hypothesis a pseudo-hypothesis [9].

Similarly, if nothing empirical could count for the existence or non-existence of super-*psi*, the hypothesis appealing to it as an explanation for the data in the richer survival cases seems empirically meaningless. Some form of empirical testability (and by implication falsifiability or confirmability) is simply a necessary condition for any hypothesis being an empirically significant hypothesis. Here again, if we do not know what to accept for the falsity of a hypothesis (such as sneaky *psi* being at work), then anything and everything could count as positive confirming instance of the hypothesis; and this makes the hypothesis meaningless because vacuously confirmed by any data. Appealing to so-called higher order criteria (such as explanatory fecundity, simplicity and systematicity) for theory selection seems quite pointless if the meaning of the hypothesis cannot be clearly specified in terms of the evidence that would need to be present for justifiably rejecting or accepting the hypothesis. But there is no way, then, in principle, to determine whether *psi* is present as the cause of the data that otherwise supports the survival hypothesis. In short, to the question "What would you take as evidence that sneaky *psi*, or super *psi*, is (or is not) producing the data in these cases" the answer is "Nothing because we cannot distinguish in principle between natural causes and sneaky *psi* (or any *psi*) as the cause of any particular event." Empirical testability, and hence falsifiability, in terms of what the explanation implies at the sensory level, is a necessary condition for any plausible explanation of fact. But that is just what explanations in terms of *psi*, or sneaky *psi*, cannot achieve as long as phenomena caused by or sneaky *psi*, are observationally indistinguishable from phenomena caused by natural causes and when we have an alternative explanation before us in terms of natural causes.

As for the claim that falsifiability is not a necessary condition for empirical causal knowledge (which the above extended quotes assert), one might justifiably simply deny the claim. Knowing "with whom to talk successfully" is an hypothesis that has test implications and is readily falsifiable. Such claims could not be highly justifiable unless what is implied by them at the sensory level (unless, that is, their test implications) are clear and the claim falsifiable in principle under some reasonably adequate concept of empirical testability.

Braude is dismissive of falsifiability as a necessary condition for empirical significance because, as he says,... "It is foolish and arrogant to think that the only phenomena or hypotheses worth discussing are those that conform to our preferred forms of empirical investigation." (see note [8]) Such an attitude is the adoption of an "old-fashioned rigidly Popperian stance" because we can justifiably reject an explanatory hypothesis (for pragmatic reasons) even when it is not conclusively falsified. Indeed, he thinks that in the end we accept or reject theories or hypotheses on 'higher level' pragmatic considerations (such as systematicity and conceptual cost) because any body of evidence is compatible with so many different hypotheses.(see again note [8])

In response to these reasons for disparaging falsifiability as a necessary condition for the plausibility of any empirical explanation, one might note that it is neither arrogant nor foolish to insist on falsifiability as a necessary condition for empirical significance because without it we have no way of determining whether the hypothesis is true or false. If the hypothesis makes no specific predictions at the sensory level that tend to support the hypothesis, or tend to undermine it, then nothing at the sensory level could count either for or against the hypothesis; and the *psi* hypothesis makes no specific predictions at the sensory level, least of all that it should ever appear again in this world. That's why the *psi* hypothesis is not falsifiable as an empirical hypothesis about data in survival cases.

Along with Carnap, some of us continue to believe that a primary end of cognitive inquiry is the production of some specific predictions (which only falsifiable explanations can provide) of our sensory experience. Hypotheses that serve that purpose provide us with adaptive power generally and nature will select out whatever methods provide for such predictions as a reliable belief-making method for understanding the physical world.

Nor is the adoption of "falsifiability" as a necessary condition for a legitimate explanation of physical phenomena the adoption of an "old-fashioned rigidly Popperian stance." It is simply the insistence that if an hypothesis is to count as potential explanation for physical phenomena it must have some test implications by way of providing deductively specific predictions of sensory experience expected if the hypothesis is true or if it is false. How one goes about testing an hypothesis is indeed an interesting question, and when exactly the hypothesis has merited robust acceptance in terms of the various kinds of tests conducted is also an interesting question [10].

But what is not an interesting question is whether empirical hypotheses need

to be tested and confirmed or falsified in terms of their deductive implications at the sensory level. Otherwise, acceptance or rejection of theories or hypotheses is made on a purely arbitrary basis and provides no reasonable grounds for expecting anything at the sensory level as a result of such an acceptance or rejection. This is standard empirical practice, and if the hypothesis is not testable, and hence not falsifiable in terms of what the hypothesis predicts in terms of sensory phenomena, then nobody in the scientific community would regard it seriously because if it is not falsifiable then it is vacuously confirmed even if it provides no specific predictions. We do not rationally reject an empirical hypothesis when it is not falsified in terms of the test conditions of the hypothesis in question.

Moreover, it is certainly not the case that the acceptance or rejection of empirical hypotheses depends, as a rule, on 'higher level' pragmatic considerations that have nothing to do with whether hypotheses are empirically testable (and hence either falsifiable or confirmable in terms of the test implications of the hypotheses at the sensory level). If two hypotheses are equally well confirmed by the data, then we may be at liberty to choose one over the other until such time as one provides a better or more precise set of predictions of sensory phenomena. But just because various testable and falsifiable hypotheses equally fit the data or the evidence as possible hypotheses, does not imply that we decide on one over the other on purely pragmatic grounds. Again, empirical testability, and hence falsifiability and confirmability, is a necessary condition for any explanation being a scientific explanation.

If the appeal to psi is meant to offer a plausible alternative explanation of the data, it is not offering a scientific explanation. And if there is some other way of explaining empirical phenomena we need to know what counts as a successful explanation in that area. How would one distinguish between a good and a bad explanation when falsifiability and confirmability in terms of sensory implications are not at issue? Appealing to so-called "pragmatic considerations of a higher order" is consistent with willful belief regardless of what the facts may be. Otherwise, as an hypothesis in science, appeal to psi as a possible plausible alternative would not seem to be any better than the famous hypothesis of neovitalism in biology; it predicted (and retrodicted) nothing specific that would count for its confirmation or falsification, told us nothing about how it affected biological organisms, when and how it begins to work, and led to no fruitful expectations.

This is not to say that one may not have private knowledge of the existence of psi or super psi. Private knowledge, incidentally, is the knowledge one would have if it is based on evidence that is quite transitory, nonrepeatable and hence accessible only to the subject for a limited amount of time. But private knowledge is, by definition, not the public knowledge we seek in natural science, and there is no reason for anybody to accept an item of private knowledge as an item of public knowledge. Moreover, even if we could show empirically (as Braude has, I believe) that psi, super-psi or sneaky psi has existed in

the past, it still would not follow that appeal to any form of super-psi is a plausible explanation of the data in the better survival cases because we do not know what would count for falsifying the hypothesis (see p. 35 of "Evaluating the Super-psi Hypothesis"). Claiming, as Braude does, that the fact that the super-psi hypothesis is non-falsifiable is of no theoretical consequence, or is of little theoretical interest, seems false and is very revealing because if the thesis is not falsifiable, then, once again, it is difficult to see how any appeal to psi as a causal explanation can be anything other than vacuously true (because in principle nonfalsifiable), a priori true and evidence of a dogma.

Of course, if super-psi can be so sneaky, its presence undetectable and unpredictable, one can only wonder how Braude succeeded in establishing that it ever existed in any past circumstances. The perplexing point here is that he did succeed, in my view, because the hypothesis was the best then, and now, available to explain the empirical data in question [11].

But this conclusion of itself affords no independent plausibility at all to the view that such forces are at work accounting for the strongest cases in the survival data. And some will suggest that if the psi hypothesis was not in any sense falsifiable, then so much the worse for the claim that it ever existed.

For all the above reasons, we need to have some independent empirical evidence (which is not to say, necessarily, laboratory evidence) for the existence of sneaky-psi in other contexts before we can appeal to it in order to explain those features of survival cases that do not fit into established (or confirmed) views about the limits of psi. Until that occurs it seems gratuitous and maximally implausible to assert that we might appeal successfully to psi or super-psi to explain, for example, the acquisition of unlearned skills (such as playing an instrument or speaking in an unlearned foreign language).

In the end, given Braude's admission that the existence of psi, or super-psi, is, as a cause at work in the survival cases, neither confirmable nor falsifiable by appeal to any factual evidence at all, it is difficult to see any explanation couched in terms of it as anything more than a merely logically possible explanation, no different in kind than offering explanations in terms of angels, godlings, the gods of Homer, or Descartes' evil demon hypothesis, all of which are equally incapable of being empirically confirmed or falsified. Explanations in terms of such appeals are plausibly not explanations at all because in not having any *statable* test implications, they provide for no predictions either.

Consequently, it is difficult to see just what justification there could be for conducting in depth-psychological investigations of subjects in the richer survival cases in an effort to determine the causes of their behaviors when an explanation for such goes beyond what we can plausibly ascribe to ordinary psi. After all, apart from determining any motivation for fraud or simple deception (which is usually done anyway), if we cannot prove, or even say what would count for detecting the existence of psi at work (rather than natural causes),

what would be the point of it? Nor could the appeal to such psi, for the same reasons, ever undermine any strong case of survival evidence.

Curiously enough, it is interesting that Braude, more than anybody else has argued (convincingly I believe), that in cases such as Joseph of Copertino, D. D. Home, and Eusapia Palladino, we sample evidence for the existence of PK on a grand scale [12]. It is difficult to see how one can accept as much and also argue that there is no way of confirming or falsifying that psi exists. The natural inclination here is to grant that we do have strong evidence for the past existence of PK on a grand scale, but it is a long way from there to justifying the claim that it is super-psi (or even dandy psi) at work in the survival cases when we want to explain behaviors that go beyond explanation in terms of ordinary psi... especially when we cannot, according to Braude, confirm or falsify such claims. Until we can do as much, appealing to psi in order to explain the unusual data in these cases is very much a *ad hoc* and unjustified, even if super-psi exists.

Response to Wheatley

James Wheatley's review of *Death and Personal Survival* (JSE, Vol. 9, No. 2, p. 294, 1995) asserts that the book makes audacious claims, some of which he believes are extravagant. He thinks, for example, that my claim that it is irrational not to believe in some form of personal survival is "surely an overstatement". He also asserts that my proof that personal survival is neither logically impossible or factually impossible is not altogether successful. Further, he believes that it is incautious to claim that we can best explain the very rich reincarnation cases by "assuming that 'human personality, whatever it is, admits of reincarnation.'" And, finally, in commenting on the last chapter of the book which in part argues that personal survival is neither logically nor factually impossible, he says:

I do not find that anything in this book serves to counter Penelhum's argument that the idea of a bodiless person is logically incoherent (see Penelhum, especially pp. 54-78), but Almeder does not insist that what survives is bodiless (though he seems to believe that personal survival without a body is a possibility).

Let me respond to these criticisms. Later I will respond to others.

To begin with, there is nothing at all 'extravagant' about the claim that it is irrational not to believe in some form of post mortem personal survival. Nor is it 'surely an overstatement' to assert as much. But perhaps we have here a misunderstanding of what "irrational" means. When one says that Smith's belief is irrational or that Smith is irrational for believing something or other, one need not mean that Smith is "insane" in some clinical sense of the term, as if Smith were dysfunctional for being deluded in some belief and, for that reason, in need of psychiatric therapy of some sort. 'Insanity', as the term is often used in non-clinical contexts, may well be a form of irrationality rooting in deep

need or biochemical irregularity, producing beliefs that are readily and publicly discernable as contrary to fact; but not all forms of irrationality are forms of insanity. Even granting the difficulty of answering Bertrand Russell's famous question asking us to distinguish between a madman and an ardent supporter of an unpopular cause, insanity is a very special form of irrationality; but irrationality, *understood in an epistemic sense*, means merely that someone's belief is demonstrably epistemically defective, and that one has no epistemic right to accept it. Similarly, when one says that it is *irrational*, after viewing all the arguments available, not to believe something or other, one can only mean that deliberate refusal to accept the belief in question is, given the force of the arguments involved, epistemically irresponsible in a very basic way.

Without caring to define here more fully the concept of rationality, nevertheless we can offer a criterion for determining whether one is acting irrationally in accepting or rejecting a belief:

Minimally, one acts irrationally when one fails, after viewing all the relevant evidence available, to accept a demonstrably sound argument (or a proof) showing that something is so.

Admittedly, there is abundant research showing that people hold on to their beliefs very often when the evidence for those beliefs is no longer available to them or is clearly refuted [13]. In such cases we can, and do, say that their beliefs are irrational; by this we merely mean to say that there is a serious epistemic defect running quite deep in their thinking. Their beliefs are at variance with very strong evidence contrary to their beliefs. It does not mean that they are delusional rather than that they are strongly epistemically defective in believing what they do, given the evidence available against their belief. By implication, after looking at all the evidence, it is, for example, irrational to believe that there never were any dinosaurs on the earth, or that people do not generally descend when they jump from tall buildings, or that the sun rotates around the earth, or that there are no muons.

Perhaps I should not have used the word "irrational" when characterizing the belief of those who would not accept some form of *post mortem* personal survival. Given the clinical sense of the term, I would certainly agree with Wheatley's view that the attribution of such irrationality to those who would not accept some form of personal *post mortem* survival is extravagant and surely an overstatement. But, given an *epistemic* sense of the word, that is, the sense that applies when people simply do not, and will not, accept a sound argument when it is presented to them, then the characterization is by no means extravagant or an overstatement. The only question is whether the argument for some form of personal *post mortem* survival is so strong that rejecting it is a clear sign of epistemic irresponsibility unworthy of anyone seeking to satisfy minimally acceptable standards of rational belief. I argued at length that the

answer to this question is 'yes'. Here again, not to accept the only plausible explanation available and fitting the best cases examined, is irrational in the epistemic sense just characterized. Refuting such a claim depends on whether anybody has as plausible an explanation that does not require belief in some form of personal survival. If not, then it is not a matter of "assuming" that reincarnation is the best available explanation; it's a matter of having shown as much. So, there is nothing extravagant or incautious about the claim that it is irrational to disbelieve the evidence for *post mortem* survival. That claim follows simply from the fact that, for the best cases examined, there is no available anti-survival explanation as plausible as the argument that explains the data in terms of some form of *post mortem* personal survival.

But Wheatley goes on to assert that I failed to show altogether successfully that there is nothing logically impossible or factually impossible about *post mortem* personal survival. In fact he claims that there is nothing in the book to counter Penelhum's 'argument' that the idea of a bodiless person is logically incoherent. In response, however, I examined closely and rejected Penelhum's argument on pages 83-85. There I urged, *inter alia*, that the concept of a bodiless person is incoherent only if one assumes or proves that it is a necessary truth that human personality is in some basic way identical with one's continued bodily existence, or that having a body is a necessary condition for being a person. Neither Penelhum nor anybody else ever proved as much. So Penelhum's position is a clear case of question-begging. The most people say is that they cannot imagine what a bodiless person would, or could, be like; but that is not to prove, rather than to plead, what is at issue here.

There is certainly nothing logically impossible about human personality being in some important measure distinct from, and hence not completely reducible to, biochemical states of the body or brain, as we now know them. It may be difficult to imagine what it would be like to be a bodiless person, but if we have learned anything at all from the followers of Aristotle, Plato and Descartes, believing in mind-body dualism (and minds as irreducible to properties of bodies) is by no means akin to believing in square circles. The idea of a square circle is incoherent (logically contradictory); but the idea of human personality largely consisting in properties of a substance which while real (and probably sharing properties in common with matter as we know it) is not identical with the physical body as we now know it, is not at all contradictory. As was argued in the book, the explanation offered by C. D. Broad for what human personality must be like, and the nature of mind and its interactions with the body, shows very clearly that mind-body dualism is certainly not logically incoherent. The only way the idea of a bodiless person could be incoherent would be under the assumption that only physical objects (as we now understand them) exist and, as I have been arguing, under that question-begging assumption we have no way to explain plausibly the data in the best cases for *post mortem* survival.

But perhaps Penelhum, Wheatley, and others believe that the incoherence

here is not a matter of logical incoherence, that the idea of a bodiless person is *factually incoherent* rather than logically incoherent. Well, if this sort of incoherence is what is meant, it can only amount to claiming that mind-body dualism and what is required of it must somehow deny what is factually necessary, and thus be logically inconsistent with well established laws or theories or basic principles of some sort. Along this line, some have suggested that if a bodiless personality could be a causal agent causally interacting with the body (as we know it) and vice versa, then there would need to be a violation of the law of conservation of energy. In answering this objection, Broad was quick to note that the objection assumes that the only kind of causal interaction is that which involves the transfer of kinetic energy from one physical object to another. That in itself assumes what needs to be proven, namely that only physical objects as we customarily understand them, exist. Even so, it is to Broad's everlasting credit that he simply grants that a bodiless person would not be nothing; rather it would need to have, or in all likelihood would have, some properties that it shares in common with matter as we ordinarily understand it. In that way, causal interaction between bodies and the "bodiless person" can go forward without there needing to be a causal interaction between two bodies of the same type. In this way, no law of nature would be violated in defending the causalistic interactionism of mind-body dualism.

Besides, Broad also claims that there is no logical nor factual incoherence involved with claiming that there is a basic kind of causality between two different kinds of items that cannot be totally understood by natural science alone, although one can verify that it exists simply by reflecting on one's own mental operations. So, it is difficult to see, for these reasons as well as the ones laid out in the book, how one could defend the view that the idea of a bodiless person is incoherent. It is certainly not logically impossible nor, for the reasons offered by Broad, is it factually impossible in any clear sense of what it would mean for a claim to be factually impossible. It may well be that a bodiless person is something whose causal activities, at least as a source of explaining human behavior, we would not understand in natural science; but that is no reason for thinking it factually impossible that such a causal interaction between bodies and bodiless persons cannot occur. But appealing to that interaction to explain human behavior may well be unavailable to us. Whatever it is, a bodiless person is not nothing, and probably has some properties that we also find in physical bodies.

Wheatley also claims that it is incautious to believe that the best way to explain the very rich reincarnation cases is by "assuming that 'human personality (whatever it is) admits of reincarnation'" (p. 26). He defends this claim by observing a little later:

As Almeder allows, we do not know what reincarnates, "how it reincarnates, how long it reincarnates, whether it disappears after a series of reincarnations, or even why it reincarnates." (p. 267) Do we nonetheless have a clear enough understanding of the concept of reincarnation to accord it the role of explaining cases of the sort Almeder

discusses? I tend to doubt it. To say, with Almeder, that the survival hypothesis is now "strongly confirmed" (p. 269) or that not to believe in survival is "irrational" (p. xi) is at odds, it seems to me, with the vagueness, controversy, obscurity, and confusion that marks the whole idea of "life after death". Such haziness, further, is one reason I disagree with Almeder's claim that belief in reincarnation is at least as well established as belief in the past existence of dinosaurs. Another reason is that the dinosaur belief, at home as it is in the framework of paleontology, contrasts tellingly in this respect with the belief in reincarnation, which is housed in no scientific theory at all. (p. 296)

In response, yes, I think we do know enough about reincarnation to allow the belief to explain the richer cases. We may not know how reincarnation occurs, or why, or for how long, but that is perfectly consistent with knowing *that* it occurs. We only need to know that a sufficient criterion for personal identity is continuity of memory, or that what we should and would accept as sufficient evidence for somebody being the reincarnation of Julius Caesar, for example, is that, apart from claiming that he remembers having lived as Julius Caesar, he have most of the memories we would expect Julius Caesar to have and confirmed memories that only Julius Caesar could have. My argument was, in part, that if somebody did show up with all those memories and claimed to be Julius Caesar reincarnated, there is available to us no plausible way to explain his having of those memories if we do not explain it by appealing to reincarnation. And to say that people reincarnate is not to appeal to a vague, obscure or confused concept. It is to say very simply that human personality is not totally identifiable with properties of the human body as we know it and that it survives biological death as the repository of certain mental states and dispositions having to do with memory, intelligence, sense of humor, and acquired cognitive skills; and that it enters another bodily form some time later in order to continue its existence as a human being. There is nothing vague, obscure or confused about the claim that "Whatever I am, I am not identifiable with my body; I may therefore survive my biological death with more or less the memories I now have and with the mental dispositions I now have, and continue my existence after a while in another body in which I will acquire additional beliefs and dispositions."

What makes such a claim controversial, of course, is that it is difficult to imagine surviving as a causal agent in the world without having a physical body. Bear in mind also that in the history of science we often get solid evidence for a fact, but do not know why the fact occurs or even how it occurs. It is fair to say, for example, that a certain percentage of the cases of primary schizophrenia were known to be purely genetic or metabolic as a result of identical twin studies long before we could identify the gene causing the disease and the mechanism by which the gene produced the disease. In those days, it was not uncommon to hear geneticists say "We do not know what gene or complex of genes causes it, how such gene(s) work, or why such genes are there at all, but we do know that this is sometimes a purely genetic disease because there is no other equally plausible way to explain the results of the identical

twin studies." As it happens, for the above reasons, we knew long before the discovery of the gene causing primary schizophrenia, and long before we knew how the gene works in producing the disease, *that* a certain percentage of the cases of primary schizophrenia were in fact of a purely genetic origin. Failure to know why or how the disease occurred was by no means evidence for the "haziness" of the belief that primary schizophrenia is a purely genetic disease. And the claim itself was not, for that reason, vague, controversial, obscure or confused. I am arguing for much the same reasons that we know that some persons have reincarnated. There is no more plausible explanation for the data in the richest cases. In the sense specified above, it is, for that reason, irrational not to accept belief in reincarnation because it is the best available explanation of the data (assuming, as we should, that there is nothing logically or non-logically contradictory about it).

There is nothing at all 'hazy' about the belief in life after death, or in reincarnation, for that matter. What is unclear is just what the precise properties of human personality (or consciousness) are that allow such a phenomena. But, then again, the history of genetics will testify to the ever-changing and expanding definition of the gene *after* we knew that it was an effective causal agent in the world, although we often did not then know how it did its job.

Wheatley claims in the section quoted above, moreover, that it is because of the 'haziness' surrounding the whole idea of "life after death" that he disagrees that reincarnation is at least as well established as belief in past existence of dinosaurs. Another reason he offers is that the dinosaur belief, at home as it is in paleontology, contrasts tellingly with the belief in reincarnation which is housed in no scientific theory at all.

In response to the first, I hope the points raised above show that there is nothing 'hazy', obscure, or confused about the belief in life after death. We only need to understand the thesis that human personality (or consciousness), however it will ultimately come to be understood in the fullness of time and science, is in some important measure distinct from, and irreducible to, any biochemical property of human bodies as we now understand them and, for this reason, not biologically corruptible as are physical biological bodies. There is nothing mysterious about this claim; the only question is whether it is true, and its truth does not depend on our being able to define more concisely the nature of consciousness beyond claiming that it is not reducible to a biochemical set of properties either identical with, or produced by, the brain.

Secondly, I am not sure what to make of Wheatley's claim that the dinosaur belief is at home in paleontology but the reincarnation belief is housed in no scientific theory at all. Belief in past existence of dinosaurs is well confirmed as the best available hypothesis fitting all the data, and leads to predictions which are confirmed, but not under conditions that are controllable in a laboratory setting. Similarly, the belief in reincarnation is as scientific a claim as the belief in dinosaurs (both are empirically confirmable or falsifiable under empirical test conditions), and falls fairly squarely, at this point, into the behav-

ioral sciences such as psychiatry or psychology. As I understand it, Ian Stevenson, for example, is simply trying to explain the ostensibly unusual behavior of the people in these richer cases, in much the same way Freud was trying to explain certain forms of hysterical behavior by appealing to the hypothesis of the subconscious. And for Stevenson, the behaviors in question have no better, or as plausible, an explanation as the belief in reincarnation. We might, if we like, house the belief in reincarnation in empirical psychology generally conceived; but that might not help because it is just such work that is determinative of the nature of psychology. In fact, if, as we have argued, the hypothesis of reincarnation along with the general belief in "life after death" is true, not only would belief in reincarnation fall into the house of psychology, it would fairly well define the subject area of psychology, by setting it in stark contrast to a purely materialistic understanding of human behavior. Otherwise, belief in reincarnation need not be housed in any particular area of science, or fall under some wider theory in natural science in order to be a well-defined and well-confirmed hypothesis. As long as the hypothesis is empirically confirmable and falsifiable, in the way indicated in the book, it makes little difference where we end up placing it.

Finally, Wheatley says I oversimplify A. J. Ayer's views on reincarnation. He cites the following from Ayer's book *The Concept of a Person and Other Essays* (St. Martin's Press) which appeared in 1963 (the same year the hard-back volume of *The Problem of Knowledge* appeared):

...even if someone could convince us that he ostensibly remembered the experiences of a person long since dead, and... this were backed by an apparent continuity of character, I think that we should prefer to say that he had somehow picked up the dead man's memories and dispositions rather than that he was the same person in another body; the idea of a person's leading a discontinuous existence in time as well as in space is just that much more fantastic (Ayer, 1963, p. 127)

Well, in retrospect, A. J. Ayer was certainly tempted to argue in *The Problem of Knowledge* [14] that evidence sufficient for claiming that somebody was the reincarnation of Julius Caesar would be that the person in question have all the memories we would expect of Caesar, and memories that only Caesar could have. The person must also claim to be the reincarnation of Julius Caesar; he utters such sentences as "I remember being Julius Caesar in my last life". But it is also fair to say that Ayer does not nonequivocally endorse the view that if someone were to present such evidence, then we would have clear and non-controversial evidence for reincarnation. He says it would call for a decision on our part. But he does make it clear that personal identity would need to be a matter of having certain memories rather than bodily traits or bodily continuity. It may be fair to say that Ayer waffles a bit on what would count as evidence for reincarnation, but he allows that there would be nothing unjustified in believing in reincarnation if somebody with all of Julius Caesar's memories (and a few other mental traits) showed up on the scene. Like Parfit, Ayer

thinks it is not much of an issue because he thinks that nobody in fact has ever turned up with such memories. But they both seem to agree that if somebody were to show up with such traits then belief in reincarnation would be appropriate.

Even so, there seems to be a contradiction in the passage just quoted from Ayer simply because one cannot conceivably pick up a dead man's memories and dispositions without thereby becoming that person's reincarnation, if the criterion for personal identity which Ayer defends in *The Problem of Knowledge* is to be accepted; and I have argued that that criterion should be accepted. The "same person in another body" just means having all the memories we would expect of that person (and some memories that only that person could have) and mental dispositions of the dead man. The point here is that if I had most of the memories of Julius Caesar and some memories that only Julius Caesar could have had, and if I claim to remember, in virtue of these memories, having lived as Julius Caesar, then I am Julius Caesar reincarnated... although to be sure, I may be more than that also. If I have all those properties that would be sufficient for saying that I am, in part, Julius Caesar reincarnated. It cannot be correct to describe me as somehow picking up the dead man's memories and dispositions but not being a person in which Julius Caesar has reincarnated: Otherwise, nothing could count for anybody ever being reincarnated, and that, by implication, is to declare *a priori* that belief in reincarnation is false.

So, it will not do, by way of offering an alternative explanation for some of the richer reincarnation cases, to simply say "He somehow picked up the memories and mental dispositions of Julius Caesar (as well as a few other traits that only Caesar could have), but Julius Caesar is not really reincarnated in this man." That alternative interpretation of the data, whoever offers it, is consistent with adopting a criterion of personal identity only in terms of bodily continuity; and while Ayer has argued against the latter, it is not at all clear that the position he finally adopts is as squarely consistent with the criterion for personal identity he in fact adopts. Presumably, at any rate, the historical point is not as crucial as the logical point that personal identity cannot be explicated solely in terms of bodily continuity.

A Methodological Objection

It may be helpful to repeat, and elaborate briefly upon, a fairly pervasive methodological objection. Perhaps the most pervasive criticism of the case studies offered and discussed in *Death and Personal Survival* is that there is altogether too much inductive evidence of fraud, delusion, hoax, or simple deception in the history of the paranormal to justify generalizing from what seem to be persuasive case studies to conclusions supportive of some form of personal survival. In *Death and Personal Survival* this objection emerges when examining Susan Blackmore's analysis of the OBE material and also in the last chapter under the heading of "The Long Shadow of Hoax and Fraud." The

short answer to this objection is that the richer cases are quite different in that they defy an analysis in terms of fraud, hoax, delusion, or simple deception; and hence are as a class logically distinct from those that would be unacceptable as evidence. And it is not simply that in these richer cases we have not yet found the fraud, deceit, or delusion that must be there. The analysis of the cases offered as evidence (especially in the area of reincarnation and out-of-body experiences) shows that the richer cases are not at all like those cases which have in the past turned out to be fraudulent or defective. Let the richer cases selected as the ones to carry the burden of evidence speak for themselves, and let those who think they fall inductively into the class of fraudulent cases show that these cases warrant being placed into that category. Failure to do as much amounts to nothing more than an *a priori* claim that nothing can count as empirical evidence for some form of personal *post mortem* survival, when in fact what is being argued is that these cases should count.

Even if we admit that historically most claims about the paranormal, when examined closely, have been shown to be unworthy of rational acceptance, it by no means follows that we have examined the large number of distinctive ones that do not fall into that class. If most people die who jump off the Brooklyn Bridge, it by no means follows that everybody who jumps off the Brooklyn Bridge will die; and if some people survive the jump, we need to find an explanation of that unusual fact, rather than deny that it occurred. Similarly, if most reported cases of the paranormal have turned out to be unacceptable as evidence for reasons of fraud, delusion, deception, or sloppy methodology, it has not been shown that all cases (or even the majority of cases) are unacceptable for those reasons. By analogy, some of us have been trying to show that the richer cases are more numerous than generally supposed or reported and, in fact, like those people who jump off the Brooklyn Bridge and survive; and that would be to put these cases beyond the pale of an inductive generalization from defective cases. These cases need to be explained rather than summarily dismissed because they bear superficial similarities to those past cases that have been instances of fraud, deceit, deception, hoax or sloppy methodology. And, we contend, the only plausible explanation that fits with the facts is some form of *post mortem* personal survival. Before we reject these richer cases as solid evidence for some form of *post mortem* survival, surely somebody needs to show that they are defective in the way others have been defective in the past.

Conclusion

It is plain that if all the evidence is examined closely, the only plausible explanation for the richer cases is some form of personal *post mortem* survival. Consciousness can exist, and in some cases has existed, without a brain. If the above arguments offered against these alternative interpretations are sound, and if (as I submit they are) the rest of the major arguments in *Death and Personal Survival* are sound, then it truly is irrational to reject the view that some

people survive, in some measure, bodily death as we know it. It makes no sense to deny it.

References

- [1] This hypothesis comes essentially from correspondence with Steven Hales who thinks the ET hypothesis is as plausible as the reincarnation hypothesis and that both are equally credible and supported by the evidence.
- [2] As stated, incidentally, these conditions do not distinguish clearly between possession and reincarnation. Elsewhere I have discussed this matter at length and pointed out that, inter alia, the further conditions that distinguish between the two include the condition that in the case of reincarnation, but not in the case of possession, the subject claims to remember having lived as the former person, whereas in the case of possession, the subject usually simply claims to be the previous personality. In the typical reincarnation case, the subject is simultaneously aware of the events and persons in the current life that she would be expected to know. He or she retains, rather than loses, the dispositional awareness and memory of the current personality as affected by the historical events experienced in the current body. In short, in reincarnation-type cases we do not typically see the total personality-replacement phenomena. For the present discussion, however, we need not explicitly point to these conditions that also allow us to distinguish between evidence that supports reincarnation rather than possession. But in the richer cases discussed these differing clinical features are very much in evidence. For a fuller detailed discussion on the differences, see *Death and Personal Survival* (Littlefield Adams, Lanham, Md.:1992) pp. 155-158.
- [3] Derek Parfit, *Reasons and Persons*, Oxford, Oxford University Press: 1980. p. 287
- [4] I have argued in *Death and Personal Survival* that, in fact, we only need a certain number of rich memory claims, memory claims we would expect of Caesar and memories that only Caesar could have had. For the present discussion, however, I add something not necessary, but which surely, in conjunction with the latter will render the list of traits certainly sufficient for justifying belief in reincarnation.
- [5] *Death and Personal Survival* Lanham, Md., Rowman and Littlefield, 1992. 42-47, 51-53, 117-120, 151-154, 195, 264ff, 268, See also, "Beloff's Reservations" in *Parapsychology and Thanatology*. eds. L. Coly and J. D. S. McMahon (eds), Annual Proceedings of the Parapsychology Foundation, N.Y.: 1995.
- [6] See Stephen Braude, *Survival or super-psi?* *Journal of Scientific Exploration*, (Vol. 6, No. 2, 1992) 127-144; Reply to Stevenson (Vol.6, 1992) 151-155. Evaluating the super psi hypothesis in George Zoltschan, John Schumaker, and Greg Walsh, eds. *Exploring the Paranormal: Perspectives on Belief and Experience* (Dorset, England; Prism Press, 1989) 125ff; and *Disassociation and survival: A reappraisal of the evidence*. L. Coly and J. D. S. McMahon (eds.) *Parapsychology and Thanatology* (N.Y.: Parapsychology Foundation, 1995) 208-228. See also Jule Eisenbud, *Parapsychology and the Unconscious* (Berkeley, California: North Atlantic Books, 1983) Ch.14.
- Sometimes, incidentally, Braude claims that his position is not properly understood by those who take issue with it. Sometimes he says that he is not trying to offer a competing explanation for the data in the reincarnation cases, or other survival cases. Rather he is only arguing that "the plausibility of the super-psi hypothesis is such that there are fewer reasons to reject it in these cases than commentators usually suppose" and that his point is basically to show that "super-psi explanations have been underrated and misunderstood, not that they are either clearly or even marginally preferable". (see "Reply to Stevenson" p.151). This seems to suggest that under the best of circumstances the super-psi hypothesis must be at least as plausible an explanation for the data in the best cases as is the hypothesis of survival. Any weaker claim about the value of super-psi as an alternative plausible explanation for such data would not seem to warrant serious consideration.
- [7] Stephen Braude, "Evaluating the super-psi hypothesis" in George K. Zololschan, John Schumaker and Greg Walsh, eds. *Exploring the Paranormal: Perspectives on Belief and Experience* (Dorset, England, Prism Press, 1989) pp. 27-28.
- [8] Elsewhere, on the same issue he says:

"Now I grant that there are problems here regarding falsifiability, and that it is often unclear what would count as evidence for super-psi, especially if the only differences between psi and non-psi effects are in their unobservable causal histories. But one must be wary of attaching too much importance to the straightforward falsifiability of empirical hypotheses. For one thing, since there are good reasons for thinking that psi can be both sneaky and naughty in its manifestations — that is, that psi effects (especially, but not exclusively, outside the lab) might be surreptitious and generally unpredictable (Braude, 1986, 1989), we might have no choice but to accept that state of affairs. It might simply be the case that some natural phenomena are not as neat and easy to theorize about as we would like, and if so we have no choice but to accept the hand Nature deals us. It would be both foolish and arrogant to think that the only phenomena or hypotheses worth discussing are those that conform to our preferred modes of empirical investigation.

"Furthermore, although it is often apparently quite convenient to reject certain psi hypotheses on the grounds that they are unfalsifiable, it is also somewhat disingenuous, because the same critics would never adopt such an old-fashion rigidly Popperian stance with regard to numerous other (less controversial or more immediately useful) empirical hypotheses. The fact is, there may be good reasons for thinking that an hypothesis is true even if it is strictly unconfirmable. Similarly, there may be good reasons for rejecting an hypothesis, even if it cannot be conclusively falsified. Any collection of evidence is compatible with an indefinite number of hypotheses; and ultimately we select one candidate from the lot on the basis of higher level pragmatic considerations (*i.e.*, systematicity and conceptual cost) . Hence, claiming that an hypothesis is unfalsifiable is not the same as claiming that nothing can count against it." ("Reply to Stevenson" p.152)

- [9] As Hempel has noted: "But if a statement or set of statements is not testable at least in principle, in other words, if it has no test implications at all, then it cannot be significantly proposed or entertained as a scientific hypothesis or theory, for no conceivable empirical finding can then accord or conflict with it. In this case, it has no bearing whatever on empirical phenomena, or as we will also say, it lacks empirical import." (Philosophy of Natural Science. Prentice Hall, 197., p. 30)
- [10] See Karl Hempel's discussion "Provisos: A Problem Concerning the Inferential Function of Scientific Theories" *Erkenntnis* Vol.28, 1988, 147-164. Reprinted in James Fetzer ed. *Foundations in Philosophy of Science*, Paragon Press, 199., pp. 127-138.
- [11] Admittedly this sounds contradictory. But it is not. Belief in the existence of psi in the D. D.Home case, the Paladino case and the Copertino case, for example, seems quite reasonable because no other hypothesis fits as well with the facts in those cases even though we may not be able to predict when, where, and how psi will emerge in any other case, and even though the explanation in terms of psi did not retrodict the evidence in these cases. It provided then, and even now, the best coherent fit on the data, there being no plausible alternative explanation that was logically coherent. But belief in the existence of psi as a plausible alternative explanation for the data in survival cases is unacceptable because it has no testable implications as an alleged cause in those cases, whereas the alternative hypothesis (survival) does have testable implications and is therefore falsifiable or confirmable.
- [12] See Limits of Influence, Chapters Two and Three.
- [13] See Kahneman, D. and Tversky, A. "On the Psychology of Prediction" *Psychological Review* (1973) Vol. 80. 237-251 and Ross, L. and Anderson, C. "Shortcomings in The Attribution Process: On the Origins and Maintenance of Erroneous Social Assessments" in D. Kahneman P. Slovic and A. Tversky (eds.) *Judgment Under Uncertainty: Heuristics and Biases*, Cambridge, Cambridge University Press: 1982 for evidence to the effect that people continue to hold some beliefs even after justification for those beliefs have been explicitly removed.
- [14] See the paperback edition of *The Problem of Knowledge*, Penguin Classics, 1964.

Braude's Reply to Almeder's "Recent Responses to Survival Research"

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Almeder's very interesting paper raises a number of points worthy of further discussion, but I will restrict my comments to his remarks on falsifiability and the super-psi hypothesis. Evidently I have not expressed my position on this topic as clearly as I had hoped, because when Almeder challenges it, he seems to set up a straw man by attacking a position I do not hold. In addition, it seems that Almeder conflates two distinct senses of the term "falsifiability", and I suspect that some of our apparent disagreements result from his failure to keep those two meanings distinct. In fact, my suspicion is that our positions are actually closer than Almeder makes them appear. So rather than dwell on textual exegesis and further wrangling over the ways I have expressed my views in the past, let me try once more to clarify my position.

In one (strong) sense of "unfalsifiable", if hypothesis H is unfalsifiable, nothing whatsoever can count against it. That is, not only is H compatible with any evidence one can muster, but in addition no evidence can even detract from the plausibility of H and give us good reason for preferring not-H to H. But there is a weaker sense in which H can be unfalsifiable. Even if all prima facie evidence against H can (in principle) be seen as compatible with H (*i.e.*, even if H and not-H are both compatible with the data), some evidence can reasonably be taken as rendering H less plausible than not-H.

I had hoped to make it clear that I consider motivated super-psi explanations of the evidence for survival to be unfalsifiable in the weaker sense. In fact, that is the sense in which most interesting hypotheses about human motives are unfalsifiable. Moreover, I have noted repeatedly that we formulate these sorts of weakly unfalsifiable hypotheses all the time, and they play a crucial role in the way we conduct our lives. For example, consider the hypotheses "S is angry with me" and "S is not angry with me." In many real life situations there may be no way to decide conclusively between them. For example, even if S says he is not angry, one can always interpret that remark as (say) a sign of S's reluctance to admit his anger, or a sign of self-deception or lack of self-awareness. Similarly, ostensible evidence for a lack of anger can always be interpreted as evidence of veiled anger. Nevertheless, some people are much better than others at selecting among such rival hypotheses, and accordingly they make less of a shamble of their lives than those who are more "explanatorily challenged". The situation is much the same in the case of

super-psi explanations of the evidence for survival. I have said repeatedly that although one cannot conclusively prove either the survival or super-psi hypothesis — that is, that both are in principle compatible with the data — that does not mean that nothing can count against the super-psi hypothesis. So it is surprising, to say the least, to find Almeder claiming that on my view no empirical data is relevant to whether or not we accept a super-psi hypothesis.

Indeed, one would have thought that my analysis of the Sharada case (Braude, 1992, 1995) made this clear. The whole point of that analysis was to show how apparent evidence for survival can be handled by a psi hypothesis with real empirical credentials, even though both the super-psi and survival hypotheses were compatible with the totality of available (and foreseeable) evidence. I argued that the super-psi hypothesis gained in plausibility relative to various discoveries about the needs, interests, and behavior of the principal figures in the case. If no such needs, etc., had turned up after careful depth-psychological examination, that would count against the super-psi hypothesis and in favor of survival. That is why a motivated-psi hypothesis differs from classic cases of statements that seem to be unfalsifiable in the strong sense (*i.e.*, no evidence of any kind would be seen as counting against the hypothesis), an example of which would be, "God loves us as a father loves his children." Nevertheless, it remains true that the operations of a reincarnated mind might be indistinguishable from those of motivated psi among one or more living persons. That is why the evidence for survival will not be conclusive in the way many would prefer. As far as I can see, any ostensible evidence for survival will be compatible with an alternative super-psi explanation. But mere compatibility with the data or logical consistency is not what is at issue.

While we are on the subject of logical consistency, I should note that Almeder oversimplifies the process of generating and confirming scientific hypotheses. Sounding uncharacteristically like an old-fashioned logical positivist, he says, "if an hypothesis is to count as a potential explanation for physical phenomena it must have some test implications by way of providing deductively specific predictions of sensory experience expected if the hypothesis is true or if it is false" (p. 504, emphasis added). And shortly thereafter he says:

what is not an interesting question is whether empirical hypotheses need to be tested and confirmed or falsified in terms of their *deductive* implications at the sensory level. Otherwise, acceptance or rejection of theories or hypotheses is made on a purely arbitrary basis and provides no reasonable grounds for expecting anything at the sensory level (p. 505., emphasis added).

These claims are surprising for several reasons. First, there are serious questions about the extent to which any scientific law describes the world, and one could argue plausibly that hypothesis testing is seldom as straightforward as Almeder suggests, even in physics (Cartwright, 1983). For example, if sci-

entific laws are idealizations that are only approximately true of real-life situations, then they may have no deductively specific predictions for real cases with much more complex sets of initial conditions. Second, there are well-known classic philosophical examples of rival hypotheses which, although equally compatible with the evidence, do permit non-arbitrary choices on the basis of the sorts of higher-level pragmatic criteria to which I have referred. Consider, for example, (1) "All emeralds are green," and (2) "All emeralds are green and examined or not-green and unexamined." Clearly, all the evidence supporting the first hypothesis is also evidence for the second. But the hypotheses are not equivalent, because they have different consequences regarding unexamined emeralds. Philosophers have had much to say about this sort of case. But for now, we need only to note that the reason we reject the second hypothesis is not because of their differing "deductive implications at the sensory level." Indeed, the two hypotheses have exactly the same implications regarding our observations of emeralds: every observed emerald will be green. Our rejection of the second hypothesis has to do with various background assumptions we make about (among other things) the causes of physical change, the nature of color, and the stability of properties (see Aune, 1970, for a relatively nontechnical discussion of these issues).

Moreover, Almeder's remarks are surprising because no consequences about human behavior or feelings follow deductively from either the survival or super-psi hypotheses. One of the reasons mechanistic analyses of the mental fail is that it is impossible to specify necessary and sufficient conditions for being in any psychological or behavioral state, or write programs that simulate (in any interestingly robust sense) human behavior (see, *e.g.*, Braude, 1997, Goldberg, 1982). But that doesn't mean that acceptance or rejection of claims about needs, motives, etc. are "purely arbitrary." Indeed, the fact that some people are much better than others at understanding and predicting human behavior shows that there is an inferential skill involved. It is simply not a deductive skill. Rather, it has to do with the sorts of higher-level pragmatic criteria I mentioned. It has to do with a person's instincts for such things as explanatory simplicity, systematicity, and conceptual cost.

So there is no particular way subjects in survival cases must behave if the survival hypothesis is true or, alternatively, if the motivated-psi hypothesis is true. We can do no better than to conjecture what sorts of behaviors one can reasonably expect to see in each case. And even then, what is reasonable to expect depends critically on subtle details involving the subjects' histories and psychological background, and also the cultural and more local social milieu in which the behaviors take place. That is precisely the sort of examination I tried to initiate in the Sharada case, and it is what I have argued is essential for evaluating any case suggesting survival.

I would recommend a similar strategy for evaluating any proposed super-psi explanation. For example, even if a car crash caused by sneaky psi is indistinguishable from one caused normally, we could still have reason — although

never a conclusive reason — for choosing one explanation over the other. We might have to string together lots of apparently isolated facts to opt for the sneaky-psi alternative, but it could be done. We would have to find plausible links to the needs and interests of the presumed aggressor and tell a reasonable story about (say) conflicts of interest between that person and the driver of the car, and maybe also look for patterns in the data (*e.g.*, accidents befalling people the agent doesn't like). In many cases, we may simply have too little information to know whether the super-psi explanation is a live option rather than a mere possibility in logical space. But in those cases where we can make educated guesses of the aforementioned sort, when we make a decision, we do so on the basis of the kinds of pragmatic criteria to which I have repeatedly referred. We are looking for the story which makes the most sense systematically and which appeals to our instincts about explanatory simplicity. And this is essentially the procedure we follow any time we explain human behavior. Moreover, in both cases, the information needed to choose one hypothesis over another requires a certain amount of digging. In the super-psi case, of course, the process is more daunting, and in many cases we will simply have to conclude that we do not know what to say. But that is not unprecedented, nor a sign that we are entertaining hypotheses that are empirically defective. Many times in the case of acceptable everyday attempts to explain human behavior, we likewise do not know what to say.

Almeder writes as if we do not yet have "independent empirical evidence... for the existence of sneaky-psi" (p. 506), which he says we would need before we can appeal to it in cases suggesting survival. Several points need to be made about this. First, sneaky psi need be nothing more than psi triggered by one's unconscious or subconscious needs, and it is no more incredible to suppose that this occurs than to suppose that unconscious motivations undergird ordinary behavior. But of course, the evidence for everyday, non-psi effects of unconscious processes is not something one obtains by the sorts of deductive empirical procedures outlined by Almeder. Moreover, quite apart from all the evidence for (generally unintended) experimenter effects in parapsychology and also evidence for psi in the context of psychotherapy, one could argue that we already have ample and apparently straightforward laboratory evidence for sneaky psi: that is, psi that betrays either unconscious knowledge or unconscious motivations on the part of the subject (see, *e.g.*, Schmidt, 1970; Stanford, et al., 1975). Also, Almeder seemed to suggest, a few paragraphs earlier, that I had already shown in *The Limits of Influence* "that psi, super-psi or sneaky psi has existed in the past" (p. 506). So it would appear as if Almeder's demand for independent empirical evidence has been met after all.

I doubt, therefore, that Almeder and I differ as profoundly as he suggests. I would be willing to accept a survivalist explanation over a super-psi explanation if a reasonable depth-psychological examination failed to render the latter plausible. My repeated complaint about investigations of survival cases is that they are psychologically superficial, not that there can never be grounds for

accepting or rejecting the survival hypothesis. Where Almeder and I might differ more substantively is over the quality of the evidence in favor of survival. For example, Almeder is more persuaded than I am about the evidence regarding the acquisition of unlearned skills in survival cases. I would say (and have said) that before we take this evidence as evidence of survival, we need to learn a great deal more about savants and prodigies, and we need to examine carefully various underlying assumptions about skills (e.g., the extent to which we can generalize over skills, and the extent to which learning a second language is a different process than learning a first language). But all that is another story.

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Wheatley's Reply to Almeder's "Recent Responses to Survival Research"

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I am grateful to Robert Almeder for his thoughtful, patient, and interesting comments on my (1995) review of his *Death and Personal Survival*, "a well-organized and clearly written book" (p. 294), as I said in my review, to which among other writings his "Responses" is addressed. And I also said, while disagreeing with his assessment of the strength of the survival evidence, that I found that assessment "invigorating and thought-provoking" (p. 300). Although regarding matters touched on in my review, I do not believe that "Responses" adds to the argument of the book; nevertheless I appreciate the obvious care with which Almeder has read the review.

Between Almeder and me there appears to be a substantial area of agreement. We both prize falsifiability in scientific hypotheses; more significant, perhaps, we each subscribe to psychophysical dualism in one form or another and so reject reductive materialism; and like him I disregard the thesis that it is false *a priori* to say that survival occurs. On the contrary, Almeder and I both view survival as, at least, an empirical possibility. For my part, I agree with Price (1960|1995) when he writes:

There is a chance that we may find ourselves surviving after death; a chance which is not small enough to be negligible... ...[I]t is a risk which a reasonable man must take into account. (p. 218)

Further, Almeder and I share the view that rich cases of the reincarnation type require the most serious attention and examination and challenge us to consider the possibility of reincarnation.

However, I still believe it oversells the amassed evidence to maintain that not to believe in survival is *irrational*. (And no, it did not occur to me that Almeder was using *irrational* in any but an epistemic sense.) Almeder himself, it is worth noting, even though arguing "that we *know* that some persons have reincarnated" (p. 512, my emphasis), continues to speak of "the reincarnation *hypothesis*" (p. 5006, again, my emphasis). For him, I wonder, what is hypothetical about the occurrence of reincarnation if he knows that reincarnation occurs?

At any rate, Almeder does speak of the reincarnation hypothesis and moreover, in contrast with some writers (see, *e.g.*, Chari, 1978), he regards it as fal-

sifiable. And perhaps it is, though never having seen a falsification procedure clearly spelled out for it, I am not altogether persuaded. In "Responses" Almeder seems to hold that the hypothesis can be seen as embodying three claims about human personality:

1. Human personality "is not totally identifiable with properties of the human body as we know it." (p. 511)
2. "It survives biological death as the repository of certain mental states and dispositions having to do with memory, intelligence, sense of humor, and acquired cognitive skills." (p. 511)
3. "It enters another bodily form some time [sic] later in order to continue its existence as a human being." (p. 511)

(Reincarnation has sometimes been thought to involve rebirth in nonhuman form, an allowance that would require a recasting of at least claim 3. And with regard to claim 2, what if, for instance, sense of humor were not among the surviving dispositions: Would that be crucial?)

Claim 2 is essentially the so-called survival hypothesis, which seems to me to be unfalsifiable. But on the assumption that (2), and hence (1), is true, I suppose that (3) may be falsifiable in principle, though exactly how an empirical proof of its falsehood would look remains hard to say. Would a falsification procedure involve predicting, searching for, and failing to find many cases of the reincarnation type perhaps richer than any already on record, as well as showing that the latter can indeed be explained in nonsurvivalist terms? How rich, and how many, must the predicted ones be? How long must we seek them? "In terms of what the [reincarnation hypothesis] implies at the sensory level" (p. 503), to adapt Almeder's words, what are we to accept for its falsity? In *Death and Personal Survival*, he gives an inkling of how to proceed when he writes that

if we were to regress a large number of people and never get the sorts of memories or unlearned skills that only reincarnation could plausibly explain, or if... we were never to come across any more spontaneous cases like the ideal [*i.e.*, very rich] cases, we would need to reject the hypothesis. (p. 269)

Although this remark is relevant and not unhelpful, it is undeniably vague and in itself does not much bolster confidence that the hypothesis is falsifiable. Yet it seems promising, and I think it serves as a sketch that might usefully be filled in.

Finally, because Almeder thinks he knows, on the basis of evidence available to all, that reincarnation occurs, or that other forms of personal survival do, and given his comments on the irrationality of nonbelief in survival, I infer that he regards all those familiar with the evidence who remain unconvinced as simply guilty of "epistemic irresponsibility unworthy of anyone seeking to satisfy minimally acceptable standards of rational belief" (p. 508). But in an

excellent synopsis of reincarnation studies, Matlock (1990) refers to "the difficulties of reconciling reincarnation with the accepted body of scientific knowledge" (p. 236) and writes that

it would be rash to declare that reincarnation has been shown to occur. Until the data and concepts discussed in this chapter can be assimilated to the rest of scientific knowledge, the data, at their best, will remain no more than suggestive of reincarnation. (p. 255)

This is a position with which I am quite sympathetic. But while I do not agree with Almeder that withholding belief in survival has now become epistemically wrong, I can well understand his evident frustration. With regard to many complex claims, however, I suppose that rational persons may sometimes disagree (even rationally) concerning whether they have, in Ayer's useful phrase, the right to be sure that the claims are true.

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Almeder's Reply to Wheatley & Braude

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I would like to thank Professors Wheatley and Braude for their thoughtful comments and insights on my paper. It is nice to see that we have so much to agree upon. But let me say a few things briefly about what we do not yet agree upon.

Professor Wheatley still thinks it is a matter of overselling the amassed material to claim, as I have, that it is irrational not to believe in some form of personal survival, and that my calling it a 'hypothesis' indicates as much. He also seems to see the survival hypothesis as essentially unfalsifiable, thinking that the conditions I offered for the falsifiability of belief in reincarnation are helpful but undeniably vague. Finally, he agrees with Matlock's claim that it would be rash to say that reincarnation has been shown to occur until the data and concepts can be assimilated to the rest of scientific knowledge; and until that is so, the data at best is only suggestive of reincarnation.

In response to his first point, when I claimed that it is irrational not to believe in reincarnation, I meant merely to indicate that the argument offered in *Death and Personal Survival* for believing in some form of personal survival is a demonstrably sound argument because it is the best available scientific explanation for the relevant body of data. Many, and all, attempts to falsify the hypothesis in the cases presented have failed, and predictions implied by the hypothesis have been and continue to be born out. Assuming that one understands the argument, not to accept such an argument after examining it is epistemically irrational. Calling it a hypothesis does not imply that it is not a robustly-confirmed and strongly acceptable hypothesis or claim. At the very least, those who refuse to accept the argument as a sound argument, or those who think that the argument is only *suggestive* of reincarnation, need to offer a good alternative explanation fitting the data equally well. But that is precisely what has not yet happened. The fact that the scientific community has not yet achieved anything like a consensus in support of the hypothesis is largely reflective of the fact that the community tends to move slowly. I have not claimed that belief in reincarnation is an accepted fact in science, but only that it is irrational not to accept the argument for reincarnation as a sound argument. Mendel's laws of heredity were quite sound long before the scientific community came to accept them. Doubtless, the scientific community may have a deep-seated bias favoring crude materialism and the belief that if anything exists as a causal agent in the world such an object must be like the usual

physical objects in the visible neighborhood. Also, in explaining the reluctance of natural scientists to adopt a sound argument for the belief in reincarnation, one cannot help but note profound questions about how we might even examine the causal activities and effects of such causal agents in the world. That there could be objects in the world that might be causal agents that do not suffer corruption in the way typical physical objects do, raises the disturbing spectrum of believing in the existence of objects in some ways very unlike physical objects and whose causal activities may not be understandable and predictable within science even though the existence of such objects could be established by science. After all, if one of the central goals of natural science, not to say of all inquiry, is to provide precise predictions of our sensory experience, it is not surprising that one might have difficulty in accepting the existence of objects whose causal activities may seem inaccessible in principle to natural science.

Secondly, it is difficult to see anything particularly or *undeniably vague* about the view that it will be sufficient for rejecting belief in reincarnation that all past memory cases be shown to be instances of fraud (or methodological sloppiness), and that we obtain no more non-fraudulent cases. There are other things one might also accept as evidence sufficient for falsifying the reincarnation hypothesis. For example, extra-terrestrials might in fact someday appear and show us in detail how they succeeded in implanting the memories of past dead persons into those who for various reasons were made to believe fraudulently that they were the reincarnations of those past persons. They might even show us how they made such people speak in foreign languages not previously learned. A number of particular cases of alleged reincarnation have in fact been falsified because the memory claims were falsified: there was no person that the subject 'remembers' having been in a previous life. And some have been conclusively falsified because the events which the subject allegedly witnessed in a previous life demonstrably never took place in a previous time. These are not undeniably vague as refutations of claims of subjects who claimed to have lived a previous life.

Finally, Wheatley approvingly cites Matlock's claim that until the scientific community assimilates and accepts the evidence for reincarnation, it is rash to say anything more than that the strongest cases are no more than suggestive of reincarnation. In response to this claim, one might urge that there is a distinction between a criterion for acceptance of an argument as sound and a criterion for the soundness of an argument. We have seen that the argument for reincarnation is sound and that therefore the thesis has been as well demonstrated as a number of theses already accepted in natural science. The fact that the scientific community has not yet accepted the thesis as demonstrated is a sociological fact which should not be confused with the evidence for the soundness of the thesis. Obviously, belief in reincarnation is not now broadly accepted in science as the best available explanation for the cases we have advanced. For various reasons it is very difficult to get the scientific community to even look

at the data. Remember that it took the scientific community over fifty years to take a close look at Mendel's principles of heredity. Still for all that, it would be irrational for anybody to look carefully at the argument for reincarnation and not accept it. There is more to be said on this, but not now.

In response to the interesting comments offered by Professor Braude, the essential point Braude asserts is that the psi-hypothesis is only weakly unfalsifiable. I think that means that it really is falsifiable in some important sense. In characterizing the concept of weak unfalsifiability, he says that "Even if all prima facie evidence against H can (in principle) be seen as compatible with H (i. e. even if H and not-H are compatible with the data), some evidence can reasonably be taken as rendering H less plausible than not-H." Accordingly, motivated super-psi explanations of the evidence for survival are unfalsifiable in only this weak sense. Braude claims that he has repeatedly said, for example, that although one cannot conclusively prove either the survival or the super-psi hypothesis, that does not mean that nothing can count against the super-psi hypothesis. If in the Sharada case, after an in-depth psychological study of the interests, behavior, and needs of the subject, one found no appropriate needs, that would count against the super-psi hypothesis and in favor of the survival. So, I take it that what Braude is really saying is that explanations in terms of motivated psi are in fact in some important way falsifiable, at least in terms of plausibility considerations. Braude then adds,

"Nevertheless, it remains true that the operations of a reincarnated mind might be indistinguishable from those of motivated psi among one or more living persons. That is why the evidence for survival will not be conclusive in the way many would prefer. As far as I can see, *any* ostensible evidence for survival will be compatible with an alternative super-psi explanation."

Before going on to other comments Braude makes, let me say something about this.

To begin with, to say that "it remains true that the operations of a reincarnated mind might be indistinguishable from those of motivated psi among one or more living persons" and that "any ostensible evidence for survival will be compatible with an alternative super-psi explanation" is to assert something that needs to be justified, and I doubt that it can be. After all, suppose, for example, that somebody claims to be the reincarnation of Napoleon Bonaparte and, in addition to having most of the memories we would expect of Napoleon, and memories that only Napoleon could have, he also speaks Napoleonic French, a language he has demonstrably not learned in this life. Demonstrably, in such a case the evidence is not consistent with any motivated super-psi explanation because there is no evidence outside these cases of anybody ever having such abilities simply because they might have a need to have such abilities. In the history of psychology there has never been a case of somebody speaking a foreign language they did not learn, no matter how much they might

desire or need to speak that language. Thus the evidence that counts for reincarnation and which one would expect if reincarnation were true, cannot in any given case be equally well explained by appeal to motivated-psi. Indeed, what is interesting is that even if Sharada, for example, had a deep-seated need to speak a language she never learned, her speaking the language she never learned could not be explained by appeal to the need unless in the history of the world we had some clear non-controversial cases of people speaking languages based on such need. But we do not have that, and that is why appeal to such an explanation is *ad hoc*. Thus, claiming that "any ostensible evidence for survival will be compatible with an alternative super-psi explanation" is by no means established.

Moreover, earlier Braude said that under certain circumstances he would accept that the motivated-psi hypothesis had been falsified, and yet goes on to claim that any ostensible evidence for survival will be compatible with an alternative super-psi explanation. If logical compatibility is not the issue here, how can these two claims be consistent? It still looks as if what is being claimed here is that the motivated-psi hypothesis, given any data for survival, cannot be falsified as an alternative hypothesis fitting the data.

As for the claim that I oversimplify the process of generating and confirming scientific hypotheses, what I said in the quoted passage is that it is a necessary condition for any scientific claim that it be tested in terms of the deductive implications of the hypothesis. That is not to say, of course, that such testing is sufficient. Indeed, Bayesian initial probabilities could certainly be necessary, and non-deductive support is also a feature of a robustly confirmed hypothesis. There are admittedly a number of issues involved in offering necessary and sufficient conditions for an adequate testing and confirmation of a scientific hypothesis; but no theory in the history of science was ever accepted that did not have positive instances deductively implied by the hypothesis in question. I will stand by the claim that in the absence of positive instances of the hypothesis in question, it is not acceptably confirmed, and that claiming that it is arbitrary. Doubtless the history of science has a number of cases in which two mutually exclusive hypotheses are supported to an equal degree in terms of positive instances of the hypothesis. This is a good argument for the decisive refutation of the traditional H-D model and packs a Bayesian message. Nothing I ever said denied as much.

Braude also asserts that no consequences about human behavior or feelings follow deductively from either the survival or the super-psi hypothesis. However, this a questionable claim. Derek Parfit and others have noted that the belief in reincarnation has specific deductive consequences at the sensory level. If one assumes, for example, that systematic memory is at least a necessary condition for personal identity, then one who legitimately claims to have lived a past life will have certain memories of the past life (memories that only the person in the past life could have), memories which admit of empirical confirmation. The absence of such confirmed memory claims will be sufficient to

refute or undermine the reincarnation hypothesis. In short, if the belief in reincarnation is true, then in certain circumstances we would expect certain behaviors and memory claims, the absence of which would be sufficient to reject the hypothesis (see Derek Parfit, *Reasons and Persons*, Oxford University Press, 1981. p.287). Certainly, however, one can agree with Braude that there are no deductive implications that would allow for falsification of the motivated-psi hypothesis. That's why the survival hypothesis is an empirically confirmable and falsifiable hypothesis and why the motivated-psi hypothesis is not. Even so, Braude also says that if certain psychological needs are determined to be absent in motivated-psi explanations, then the psi hypothesis is to be rejected. This certainly seems to be saying that there are deductive implications of the motivated-psi hypothesis. So, it is not clear whether Braude means to assert that the motivated-psi hypothesis does have deductive implications at the sensory level. The problem is that Braude seems to want it both ways. In any event, my inclination is to think that in spite of what we do agree upon, explanation of survival data in terms of motivated psi are still pseudo-explanations because, in Braude's own words, they do not have any deductive implications that would allow for empirical falsification.

Finally, I did not deny that there is sneaky-psi in the world. Rather what I denied primarily is that one should appeal to it as a causal explanation to account for the data in survival cases, and this because, in Braude's own words, it is not a falsifiable hypothesis. What we should all be happy about, however, is that for Braude, there needs to be some sense in which the motivated-psi hypothesis is falsifiable if it is to be a legitimate empirical hypothesis. The problem is that asserting as much is difficult as long as one says that the *psi*-hypothesis and the survival hypothesis have no deductive implications that would allow for testing at the sensory level.