

## Survival or Super-psi?

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Abstract— Even the most sophisticated discussions of the evidence for survival underestimate the conceptual difficulties facing the survival hypothesis. Perhaps the major challenge is posed by the rival "super-psi" hypothesis, which most writers fail to confront in its most plausible and potent form. Once the super-psi hypothesis is taken seriously, two major weaknesses in discussions of survival stand out clearly. First, analyses of apparently anomalous knowledge *that* tend to be fatally superficial in their treatment of subject psychodynamics. And second, analyses of apparently anomalous abilities and skills trade on an impoverished and naive conception of the nature of human abilities.

### Introduction

Two questions have dominated parapsychology since the founding of the Society for Psychical Research. The first is: "Do human beings have psychic (psi) abilities (ESP or PK)?" The second is: "Does human consciousness survive bodily death?" The first question, in my opinion, has been answered satisfactorily in the affirmative, and although the road to that answer has been bumpy and somewhat circuitous, the issues are relatively straightforward—at least as compared to the second question. But the question about survival is almost transcendently recalcitrant; it may, in fact, be impossible to rule out alternative hypotheses. Unfortunately, however, most writers on survival have failed to appreciate the point. Indeed, even the most sophisticated authors tend to underestimate the enormity of the theoretical challenges facing them. The goal of this paper is to bring some of their more serious shortcomings into sharper focus.

Any satisfactory discussion of survival must proceed along two fronts, one of which is empirical and the other of which is philosophical. In this respect, the study of survival is no different from any scientific investigation, except that in most areas of science fundamental philosophical assumptions usually form part of the received conceptual background. But in the case of survival research, deep philosophical issues dominate the foreground. For example, many have wondered whether the very *concept* of survival is intelligible. And as the reader may realize, some have decided that it is not, and have therefore argued that we should reject the survival hypothesis a *priori* (see, e.g., Flew, 1976).

Now I am by no means opposed to a *priori* arguments against scientific claims. In many cases, ostensibly empirical claims rest on thoroughly indefensible philosophical presuppositions, and those presuppositions often blind

researchers to alternative ways of interpreting the data. That is perhaps most conspicuously the case in the so-called cognitive sciences, a great deal of which is no more than bad philosophy couched in (and obscured by) the imposing technical vocabulary of the electrical engineer. Nevertheless, some *a priori* arguments are more persuasive and profound than others. And I consider the usual arguments against the intelligibility of survival to be quite shallow. Flew's arguments, for example, trade on superficial features of language use and suspicious thought-experiments whose plausibility rests on our meager imaginative capacities. In my view, it is irrelevant how much difficulty we have imagining what survival might be like. Similarly, it is hardly surprising that we have trouble describing an after-life in terms designed for dealing with an ordinary embodied existence. What really matters is that it is relatively easy to construct hypothetically ideal cases so coercive that we would have no choice but to admit (or at least to entertain seriously) that survival of some sort is a fact, no matter how much of a challenge that poses for our familiar conceptual framework. Our ability to formulate such ideal cases shows that the evidence for survival cannot be rejected for the sorts of facile reasons provided by Flew. The more pressing question is to what extent actual cases approach the theoretical ideal.

But even the best real cases—and possibly also the best ideal cases—face certain purely conceptual obstacles. These have to do with the challenge from an alternative psi-hypothesis, usually called the "super-psi" hypothesis. As I see it, the most serious obstacle to taking even the best evidence for survival at face value is the possibility that the data can be explained in terms of highly-refined psi among the living. What I shall argue below is that no case so far investigated resists explanation along these lines, and that the usual arguments against super-psi explanations are seriously inadequate.

### **Super-psi and the Ideal Case for Survival**

Parapsychologists and others tend to approach the study of psi with a standard set of indefensible tacit assumptions. First, they often assume that if psi occurs, it occurs only to a very modest degree. Second, they usually assume that when observable (and not merely statistically demonstrable) psi effects occur, those effects will be sufficiently overt or unusual to be identified as psi events.

Both assumptions, however, are intolerably naive. Indeed, the former is methodologically egregious. Given our current degree of ignorance concerning psychic functioning, we simply have no grounds for placing antecedent limits of any kind on its scope or refinement. To accept this point, it does not matter whether we believe that psychic functioning occurs or whether we are simply open-minded skeptics entertaining the mere hypothesis that psi occurs. If psi can occur at all, then until we have evidence to the contrary, we must assume that it can occur at any level of magnitude or sophistication. In fact, not only do we have no evidence against the possibility of unlimited psi, certain bodies of data actually support it. I have argued elsewhere that probably the best evidence for psi of any kind is the evidence of physical mediumship (Braude, 1986). But the

best of those cases shows that psi occurs in forms far more subtle, complex and extensive than would be suggested by any of the results reported from laboratory experiments. A sober appraisal of the mediumistic evidence can only make us more open to the possibility of even more elaborate or sweeping effects.

The second unwarranted assumption usually appears in attempts to argue that there is simply no evidence for super-psi. Many protest that if super-psi occurred, it would make itself known to us; but (they would argue), we have no evidence that people are able to do such things as psychokinetically affect the weather or make planes crash, or carry out sophisticated and detailed psychic spying. The assumption lurking beneath this argument is that occurrences of super-psi in everyday life will generally be conspicuous or easily identifiable as such, and that they will not simply blend in with or be masked by the extensive network of surrounding normal events. But that assumption is clearly defective (for a detailed discussion of the issues, see Braude, 1989). As far as physical phenomena are concerned, there need be no observable difference between (say) a normal heart attack or car crash and one caused by PK. The only difference may be in their unobservable causal histories. Similarly, there is no reason to suppose that information gathered by ESP has to hit us over the head with its obviousness. It needn't carry some marker—a phenomenological analogue to a flourish of trumpets—which identifies it as paranormally derived rather than random or internally generated normally.

When we think about the possible operation of super-psi, we must be careful not to suppose that it functions in total isolation from the full range of human needs and organic capacities. Quite the contrary; it is more reasonable to suppose that psi plays some role in life, that it may be driven by our deepest needs and fears (rather than those of which we are immediately or consciously aware), and that it does not occur only when parapsychologists set out to look for it. Moreover, if psychic functioning may be a component of everyday life, we must be open to the possibility that, like manifestations of other organic capacities, occurrences of psi will range from the dramatic and conspicuous to the mundane and inconspicuous.

In fact, it may be in our psychological interest for most psi manifestations to be covert. In the case of PK, that would help to preserve the useful illusion that we are not responsible for events we might have brought about. And in fact, most of us are acutely uncomfortable about accepting the possibility that we might play an active (but covert) role in the misery and calamities that occur around us (Braude, 1986, 1989; Eisenbud, 1982). That concession would force us to accept a magical world view we somewhat condescendingly associate only with so-called "primitive" societies, a world view in which we need to take seriously the possibility of hexing (or the "evil eye") and other uses of psychic functioning for malevolent and lethal purposes. Equally terrifying for most people is the fear that super-ESP might confer something like near-omniscience, or at the very least a far greater range of knowledge than we would be able to handle emotionally.

Let us suppose, then (if only to see where it leads), that we are not justified in ruling out the possibility of large scale or extremely refined psychic functioning.

And let us consider how that complicates our assessment of the evidence for survival.

### The Survival Hypothesis: Some Preliminaries

Before we can get to the heart of the issues before us, we must make some rather elementary, but very important, observations. First, we must resolve an important ambiguity in the claim that consciousness may survive bodily death. This concerns the distinction between *survival* of bodily death and mere *life* after death. To be a case of survival, properly speaking, there would have to be a relation of *identity* between a post-mortem individual and an ante-mortem individual. In some sense, the two deserve to be considered the *same*. No doubt it will be difficult to state precise or generalizable criteria in virtue of which that sort of identity holds. Nevertheless, one might think that this sort of identity is not too different from the relatively familiar respects in which other sorts of things remain the same through change. My body now, for example, is quite different from the body I had as an infant. Millions of cells have been lost and only some of them have been replaced; others have appeared for the first time as part of the normal growth process, and many of them have likewise been lost and occasionally replaced. Moreover, my thoughts and other mental states have evolved and changed deeply over time. In short, a person may remain the same in some important sense, despite undergoing a vast number of physical and mental changes. Similarly, there is a sense in which an acorn is the same as the oak into which it evolves. But even if we grant that two things can be identical without being *strictly* identical—that is, without satisfying Leibniz's Law that every property of one is a property of the other, we must be open to the possibility that after bodily death there is life without survival. Suppose, for instance, that after death one's consciousness gets absorbed in a great universal soup of consciousness (or something of the sort). I'm not sure I understand that hypothesis; but I encounter claims like it all the time. In that case, we would say that although no ante-mortem individual is even loosely identical to (or survives as) a post-mortem individual, nevertheless there is some sort of post-mortem existence.

Another vital preliminary distinction concerns two different sorts of knowledge. Generally speaking, a case suggestive of survival is one in which one or more living persons display knowledge closely (if not uniquely) associated with a deceased person, and which we have good reason to believe could not have been obtained by ordinary means. But that knowledge tends to fall into two broad categories: knowledge *that* (information or propositional knowledge) and knowledge *how* (abilities or skills). For example, some cases hinge on pieces of information displayed by a living person, which could not have been obtained normally, but which would have been known to a deceased person now ostensibly communicating that information. In other cases, however, a living person (say, a professional medium or a child) displays an ability or skill she never displayed before (say, the ability to speak a certain language, or write music), or perhaps an ability or skill quite idiosyncratic to a deceased person (say, a certain

distinctive style of humor or musical composition). And of course, in an impressive case of this type, we would have good reason to believe either that these abilities could not have been acquired by normal means at all (say, if they are likely to be organic endowments which only a few enjoy), or that they could only be acquired normally after a period of practice which we are quite certain never occurred.

Many would say that the cases most strongly suggestive of survival are those of the latter sort, displaying the persistence of knowledge how. I tend to agree. But many mistakenly think that cases of both sorts easily resist alternative explanations in terms of super-psi. Let us, therefore, consider the issues surrounding each type of knowledge. This should help us to appreciate, first, why the apparent persistence of abilities or skills is more impressive than the mere display of propositional knowledge, and second, why not even the manifestation of abilities is as coercive as it might first appear.

### **Super-psi and Propositional Knowledge**

To simplify matters, let us assume that the cases under discussion are always well-authenticated. In other words, let us assume first, that the hypothesis of fraud is highly improbable, and second, that we have good reason to believe that events occurred as reported. By assuming that testimony is honest and that observation and testimony are reliable, we may therefore concentrate on *explaining* the phenomena observed and reported. And let us assume, further, that there is good reason to believe that no explanation in terms of currently understood processes is wholly satisfactory. In that case, we may jump immediately to the question of which alternative account, in terms of presumably paranormal processes, we should embrace.

Now in a number of cases of ostensible mediumship and reincarnation, persons exhibit unusual and surprising pieces of propositional knowledge. Some of the best examples of this sort are cases featuring what Stevenson has dubbed "drop-in" communicators. As the name suggests, "drop-in" communicators appear without invitation, and are usually unknown to both medium and sitters. In the best of these cases, the "drop-in" communicators make various statements about themselves which are later verified and which nobody present at the sitting knew to be true through any normal means.

As Gauld (1982, pp. 58ff) has properly observed, such cases would seem to discourage super-psi explanations for two main reasons. The first concerns the identity and apparent purpose of the communicator. One would have to explain why the medium (or someone else present at the sitting) used their ESP to obtain information about an individual unknown to those present and at best only tangentially connected to one of them. One would also have to explain why the communicator supplies information of no apparent interest to the sitters but of understandably serious concern to the communicator. (A good example is the case of Runki's leg, reported in Haraldsson and Stevenson, 1975.) By contrast, the survival hypothesis seems rather straightforward. To put it simply, a

deceased individual needs to deal with a matter of importance to him (e.g., to console a grieving relative or take care of some unfinished business), and so he seizes the opportunity of the sitting to get his message across.

The second obstacle for super-psi explanations concerns the obscurity and diffuseness of the information which the medium (or someone else) would have to acquire paranormally. To begin with, in many cases that information is quite arcane and apparently irrelevant to sitters' concerns—for example, the fact that the ostensible communicator was buried without one of his legs, which he lost in the accident that killed him. And although in most of those cases the information would need to come from only a single source (such as a written record, or one living person's memory), in others the information would have to be assembled from separate and equally obscure sources (e.g., different written records and memories). By contrast, on the survival hypothesis, the necessary information may all be reasonably attributed to the communicator.

Both of these alleged problems strike me as overrated. In fact, the second may be dispensed with rather quickly. Since we presently have no grounds for imposing any antecedent limits on the scope or refinement of psychic functioning, we are simply in no position to assert that accessing multiple sources of obscure information is any more imposing than accessing one. In evaluating the super-psi hypothesis, we must be careful not to treat the processes involved as if they were simply a collection of really good psi, of the kind we apparently see in limited forms in some lab experiments. When we do that, it is all too easy to think that psi functioning involves an *effort* of some kind, and that if one psi performance is difficult, several ought to be out of the question. But in fact, in all its intimidating richness, the super-psi hypothesis should perhaps be called the *magic wand* hypothesis. It asserts that (as far as we know) anything at all can happen, given the relevant need for it to happen. For example, we needn't suppose that refined PK must be accompanied by constant ESP vigilance of the results of one's activities, in the way that driving a car requires sensory feedback. It may be enough simply to wish for something to happen, and then it does. Task complexity is simply not an issue. (Ironically, the irrelevance of task complexity has been emphasized even in laboratory experiments on random number generators; see Braude, 1979; Schmidt, 1975, 1976.)

The first problem, that of explaining the identity of the communicator, raises a rather different set of issues. Gauld notes correctly that the survival hypothesis

has obvious advantages when it comes to explaining why the medium selects one unknown deceased person rather than another unknown deceased person as the subject for her extrasensory researches. The deceased person selects himself. (1982, p. 61)

Indeed, as Stevenson once remarked, "Some 'drop-in' communicators have explained their presence very well" (1970, p. 63). But according to Gauld, on the super-psi hypothesis "we seem reduced . . . to supposing that selection of communicator depends upon the random operation of wholly unknown factors."

(1982, p. 59). Stevenson concurs, and his way of stating the point brings its weakness squarely into the open. He writes,

since the [super-psi] theory assumes that discarnate personalities do *not* exist, it has to attribute motive for a particular mediumistic communication or apparitional experience to the subject. But evidence of such a motive is not always available, and we should not assume that one exists in the absence of such evidence. (1984, p. 159)

The proper reply to this has two parts: first, that we should not assume such evidence is absent unless we look for it, and second, that hardly anyone looks for it, except in the naive way we expect questionnaires or casual conversations to reveal the deepest secrets of one's soul, or the half-hearted or superficial way searches are usually conducted by the lazy or frightened. If the motives in question exist, they are unlikely to reveal themselves to the sorts of surface investigations Stevenson and others conduct. Without an extensive and penetrating examination into the lives of clearly relevant (and perhaps even seemingly peripheral) personnel, we are simply in no position to reject explanations in terms of motivated super-psi.

Some might feel that this criticism is unfair. After all, our goals and interests are often unconscious and difficult to discern, and in actual case investigations we may have no real prospect of ferreting out potentially relevant deep needs and concerns. That is especially true for the older cases, where we are no longer able to interrogate medium and sitters. Now I grant that in many (if not most) cases, we may never get a clear feel for the pertinent underlying psychodynamics, no matter how hard we try. But that is no reason for not trying, and often we do not have to probe very far to glimpse some of the significant psychological activity simmering beneath the surface. Indeed, some case studies reveal clearly how much we stand to learn from depth-psychological detective work. Probably the best example is Eisenbud's brilliant analysis of Mrs. Chenoweth's Cagliostro persona (1983). The case is complex and deserves to be read in its entirety. But in a nutshell, here is what it is about.

In 1914 James Hyslop held a series of sittings with one of his favorite mediums, Mrs. Chenoweth. Also present was Doris Fischer, whom W.F. Prince described exhaustively in his monumental study of her multiple personalities (W.F. Prince, 1915:116) and in whom Hyslop was interested because of his suspicion that multiple personality might be a disguised form of mediumship (Hyslop, 1917). One of the most interesting of Mrs. Chenoweth's trance personalities was a "drop-in" who emerged on several occasions over the series of sittings. He claimed to be Count Alessandro Cagliostro, the notorious eighteenth-century mystic, healer, and (as some have alleged) con-artist. His behavior at the sittings was vivid and flamboyantly salacious, but nevertheless rather one-dimensional. Cagliostro came through as a vigorous defender of sexual freedom, including that of women, and as a severe critic of Christianity—indeed, "as a reckless blasphemer who wouldn't have lasted forty-eight hours in the Church-dominated Europe of the time" (p. 230). More importantly, however, the behavior of this

trance personality corresponded to nothing in any of the accounts of his life published up to the time of the sittings.

The real Cagliostro was arrested in Rome in 1789 and brought to trial by the Holy Inquisition. Charged with freemasonry, heresy, and promulgating magic and superstition, he was condemned to death. That sentence was later reduced to life imprisonment. But it was not until 1972 that an account of Cagliostro appeared which presented the Vatican's version of the trial (the account was translated into English in 1974). That characterization was based on a digest of charges obtained in 1855 by the National Victor Emmanuel Library of Rome. The original was still held in secret by the Vatican. For various reasons surveyed by Eisenbud, it is highly unlikely that any of the sitters had normal access to the material in the National Library, and Eisenbud's sleuthing turned up no publications citing the Vatican's version prior to 1972.

But again, perhaps the most striking feature of the trance persona was that it did not correspond to the picture of Cagliostro painted by all the reliable sources available not only at the time of the sittings but until the present time as well. Not even critical accounts of Cagliostro accused him of being lascivious or religiously cynical. Indeed, there is reason to think that Cagliostro's trial was rigged, and that it was simply expedient for the Vatican to charge him with blasphemy and rampaging licentiousness.

Hence, there is no good reason to regard this case as presenting evidence for the survival of Cagliostro. But in that case, what was the function, psychodynamically speaking, of the Cagliostro persona? Why should a colorful but historically inaccurate trance personality emerge who was so flagrantly sexual and religiously cynical? Eisenbud offers numerous intriguing reasons for thinking that the Cagliostro persona had a great deal to do with, among other things, the sitters' sexual repressions and religious upbringing.

For example, Hyslop, who "apparently devoted much of his life to spiritual and moral development" (pp. 233-4), predictably found the Count to be a deplorable figure. Hence, it seems both interesting and significant that, by his own admission, Hyslop repeatedly encountered non-spiritual "sensuous" characters in sittings he conducted. Moreover, Mrs. Chenoweth displayed a surprisingly intense attachment to the Count when it looked as if other communicators might banish him from the scene. Crying to the other ostensible communicators who tried to exorcise him, she said, "You give him back [Pause] You give him back . . . Give the Count back to me." Hyslop asked who wanted the Count, and Mrs. Chenoweth replied, "We all do. We are lost. We are lost, we are lost, we are lost [Pause] Oh, Devils, to take him away from us. [Distress and crying] . . . I won't stand it [Pause] I don't want your old God. I want the Count." Furthermore, Doris (like Hyslop) was a model of moral propriety. In fact, she seemed almost to be a caricature of naive virtue. According to W.F. Prince, "A purer, more guileless soul it was never the writer's good fortune to know." Prince also notes that Doris had a "notable lack of sex-instinct."

Now it apparently never occurred to Prince (or, apparently, Hyslop) that Doris' lack of sexuality may have indicated an inhibition of powerful sexual



desires. And in 1914 nobody considered seriously the possibility of experimenter-influence, or more generally the possibility that persons other than the medium might play an active role in shaping the material presented by a medium. Eisenbud proposes, for these and many other reasons, that the Cagliostro persona might have been a composite "dream figure omnibus for the repressed unconscious hankerings of all the principals at the sittings" (pp. 237–8). And considering some of the startlingly close correspondences between the trance personality and the behavior attributed to Cagliostro by the Church, it appears as if one or more of Mrs. Chenoweth, Hyslop, and Doris Fischer psychically raided an extremely obscure portrayal of Count Cagliostro to provide some material for the sitting.

For present purposes, it does not matter particularly whether Eisenbud's psychoanalytic conjectures are correct. What matters is the level at which he attempts to evaluate the data. Still, Eisenbud's proposals do make very good sense of the evidence, including peculiar and otherwise unexplained bits of behavior on the part of the sitters. Hence, his analysis demonstrates clearly the potential benefits of depth-psychological probings. And, of course, since the Cagliostro persona corresponds only to a false characterization of the Count apparently cooked up by Vatican officials, the case is not even a remotely plausible candidate for the survival hypothesis. On the contrary, it strongly suggests the operation of a high level of dramatic creativity and some pretty dandy psychic functioning. But with this sort of evidence staring us in the face, it is both presumptuous and naive to rule out super-psi conjectures in cases where no comparable depth-psychological study has been conducted. Regrettably, however, by comparison to Eisenbud's standard of analysis, most other case investigations are unacceptably superficial.

Consider, for example, Stevenson's treatment of the Sharada case of ostensible reincarnation (Stevenson, 1984). Since the most striking feature of this case is the evidence it offers for responsive xenoglossy—hence, the persistence of an ability or form of knowledge *how*, it does not exemplify the sort of case we are currently considering. On the other hand, since we are presently focusing on the need for depth-psychological investigation in connection with the evidence for survival, discussion of the Sharada case is quite apt. The subject, a woman named Uttara, began to have apparent memories of an earlier life when she was in her thirties. These memories occurred during dramatic changes in Uttara's personality, at which time she spoke a language (Bengali) which she apparently neither spoke nor understood in her normal state, and which she apparently never had an opportunity to learn.

Stevenson and his associates did a great deal of valuable and careful investigative work to rule out explanations in terms of normal processes. Most of that investigation was devoted to uncovering the extent to which Uttara might have learned normally about Bengali history and customs, and whether she was ever exposed to the Bengali language in a way that would explain her apparent facility in speaking it. But despite all the detail Stevenson provides, the reader gets no feel whatever for Uttara and other relevant individuals as persons. We have no

idea what moved them or what their needs and desires were. We get no sense of the profound personal issues that shaped their lives and actions. In fact, it is quite remarkable how little effort Stevenson apparently made to dig beneath the surface of their concerns, either in the actual course of investigation or in his subsequent evaluation of the case material.

A couple of examples should make this clear. One of the most glaring concerns Uttara's relationship with a homeopathic physician which began prior to Sharada's first appearance. The physician, Dr. Joshi (a pseudonym), had been treating Uttara for a variety of physical ailments. For several years he treated her as an outpatient, and then (since her condition was not improving) he admitted Uttara to his private hospital. Uttara clearly felt strongly attracted to Dr. Joshi, and at times she behaved toward him like a jealous spouse rather than a patient. Eventually, Uttara's behavior became so annoying that Dr. Joshi had her removed from the hospital. Moreover, when Sharada finally appeared she claimed that the doctor was her husband from her "previous" life in Bengal.

Although Stevenson notes that Uttara's relationship with Dr. Joshi may have contributed to the appearance of Sharada, he has strikingly little to say about the nature of Uttara's interest in the doctor, or in the doctor's reaction to her affection. This is especially regrettable, since there are good reasons to think that there is more to their relationship than meets the eye. For example, Stevenson claims that Uttara had been "strangely moved (1984, p. 105) when she first met Dr. Joshi. Now to put it bluntly, Stevenson offers no reason to think that there was anything strange about it, and in the absence of serious probing into Sharada's feelings toward the doctor in particular and toward men in general, that is a surprising choice of words. Indeed, it is out of character in a prose style that otherwise aims at being quite neutral (it may effectively reveal Stevenson's antecedent inclination to treat the case as indicative of reincarnation). Perhaps Uttara's feelings could justifiably be termed strange if they were really those of a deceased individual who had not yet clearly manifested in Uttara's behavior. But Stevenson has given us no reason to think they were anything more than the common sort of attraction one feels suddenly for another, which we often later learn has quite pedestrian origins in our various hidden needs and agendas.

Perhaps most notably, Stevenson tells us that despite Uttara's annoying displays of affection, Dr. Joshi visited her at home "a few times" (p. 105) after she had been discharged from his hospital. Stevenson attributes these visits to Dr. Joshi's "interest and perhaps compassion," but he says the doctor "indicated no deeper attachment to either Uttara or Sharada" (p. 105). But Stevenson also notes that Dr. Joshi was evasive and unrevealing during attempts to examine the nature of his relationship with Uttara. The only explanation Stevenson offers for that evasiveness was that the doctor found Sharada's attentions embarrassing.

But that explanation is hardly compelling, and Stevenson offers nothing to support it in the face of rather obvious sorts of doubts. To begin with, why should Uttara's affection and attention toward the doctor be embarrassing? Patients often fall in love with their doctors. Why would Dr. Joshi not simply

take Uttara's interest in stride? And if Uttara's behavior was so embarrassing and her attention "discomfiting and potentially compromising" (p. 105), why did the doctor visit her several times at home? That could not have helped to quell the affections of his former patient, and it could only have offered further opportunity for embarrassing confrontations. If Stevenson is correct that the doctor's motive was interest or compassion, then one would think that either the embarrassment was not all *that* acute or else the doctor's interest and compassion were strong enough to overcome it. But if the latter, why would he have been evasive in an interview? If he was feeling ordinary human compassion and a strong (but merely professional) interest in the case, might he not have been more cooperative and forthcoming in his interview? Moreover, it is not particularly helpful to learn simply *that* Dr. Joshi denied feeling attracted toward Uttara (or Sharada). *How* did he deny those feelings? What were his tone and his manner? Might they have indicated that he had something to hide? Stevenson tells us only that the doctor "practiced masterly evasion" (p. 106) during their interview.

Stevenson sheds equally little light on the subject of Uttara's feelings for men. He notes that there may be some significance in the fact that Uttara never married and that Sharada claimed to be married. He concedes that "frustrated aspirations for an independent domestic life may have found fantasied satisfaction in the role of Sharada" (1984, p. 144). But since Sharada "hardly satisfies all the criteria of the idealized, fulfilled, married woman," (p. 144), Stevenson dismisses that possibility with the rhetorical question, "why did she not complete the fantasy with a happy ending?"

The proper reply to that question should be, "You tell me!" Certainly, Stevenson reveals nothing about Uttara's fantasy life to help us make those conjectures for ourselves. But in any case there is no reason to think that we generally express our fantasies in the straightforward and flagrant way Stevenson apparently expects. If a person were living out a fantasy in too obvious a manner, it loses much of its psychological utility. Moreover, our fantasies may simultaneously represent our feelings on a number of different issues. How, exactly, did Uttara feel about men, children, or marriage, or her parents' marriage in particular? And how might that *constellation* of feelings have expressed itself in fantasy?

Stevenson is equally unhelpful in his examination of Uttara's long-standing interest in Bengal and her preference for Bengali culture over her own Marathi culture. But we needn't consider those issues here. I must also emphasize that the doubts I have been raising may come to nothing, and that Stevenson has actually interpreted the facts correctly. But on the basis of what little he has given us or has attempted to uncover, how is one to know? One would think that the individuals interrogated in case investigations are, like the rest of us, teeming cauldrons of issues, hopes, and fears under the surface. But since there are many reasons for *thinking* that psi functioning is deeply need-determined, Stevenson's perfunctory peek into the heads of his subjects simply will not do. And unfortunately, that sort of *skimming* of the psychological surface is not limited to Stevenson's work. It is characteristic of virtually every treatment of the evidence suggesting survival.

### Super-psi and Knowledge *How*

Some writers on survival (including Stevenson) grant that we cannot rule out, *a priori*, explanations in terms of super-ESP. Even so (they would say), such explanations can only handle cases of apparent knowledge that. Hence, they would still fail to accommodate certain forms of knowledge *how*—in particular, the apparent persistence of a deceased person's abilities or skills. The general line of reasoning behind this position is as follows. Mere information or propositional knowledge is the sort of thing which can be acquired simply through a process of communication (normal or paranormal). But skills, such as playing a musical instrument or speaking a language, cannot be accounted for so easily. Granted, obtaining information is often a necessary part of skill development; but it is hardly sufficient. That is because skills are the sorts of things which persons develop only after a period of practice. But since the subjects in survival cases who display anomalous skills have had no opportunity to practice them first, it is reasonable to reject explanations in terms of super-ESP and resort to survivalist explanations instead.

Although this familiar argument is superficially rather appealing, it is defective nevertheless. To see why, consider first how the argument has been applied to the evidence for responsive xenoglossy. Many have felt that if a person can carry on a conversation in a language never learned through normal means, and if that is the language of an ostensible communicator expressing himself through that person, then this would constitute good *prima facie* evidence for survival (see Gauld, 1982; Stevenson, 1974, 1984). Now as Stevenson has observed, this bit of reasoning rests on a crucial and usually tacit principle—namely, that "if skills are incommunicable normally, it follows that they are also incommunicable paranormally" (1984, p. 160). According to Stevenson, it was Ducasse (1962) who first applied this principle to the evidence for survival, and Stevenson apparently considers it to be self-evident, or at least not worthy of a defense. But in fact, Ducasse's principle is not nearly as obvious as Stevenson suggests.

Consider: if Ducasse's principle is true, that is not because it is an instance of the more general principle, "if any bit of knowledge *x* is incommunicable normally, then *x* is incommunicable paranormally." That general principle, in fact, seems quite clearly to be false. Indeed, if we accepted it, we could conclude *a priori* that ESP is impossible. It is reasonable to assume, then, that Stevenson (and others) do not accept this more general principle.

Hence, if Ducasse's principle is true, it would presumably be true only of skills. But why? Every time we learn a new skill we must do a considerable bit of unlearning, if only of acquired motor and cognitive habits which would interfere with manifesting that skill. Moreover, learning of any kind (whether of skills or information) is often highly resistance-laden; it can be hampered by an endless number of interfering beliefs, insecurities, and other fears. But these sorts of physical, cognitive, and emotional obstacles are often overcome relatively easily in hypnotic or other profoundly altered states. But in that case,

learning a skill might even be facilitated if the process bypasses the normal states in which our resistances to learning are strongest.

Actually, there are two crucial sets of issues here. The first concerns the possibility of expressing and acquiring skills by sidestepping our customary resistance-laden modes of cognition. And the second concerns the difficulty in generalizing about skills or abilities, including the ability to speak a language. These two sets of issues overlap somewhat, but I will try to keep them distinct.

To begin with, in order to decide whether skills can be communicated or acquired paranormally, one must first evaluate the rich and suggestive literature on dissociation.' For example, cases of multiple personality suggest that dissociation facilitates the development or acquisition of personality traits and skills which might never be developed or displayed under normal conditions. Alternate personalities exhibit wide varieties of behavioral and cognitive styles which are not explainable simply in terms of propositional knowledge (Braude, 1991). Those cognitive styles encompass various sorts of abilities and skills, such as artistic and literary ability, and the skills of drawing, sculpting, and writing poetry. Differences also manifest commonly as changes in handedness and handwriting. (And of course these abilities and skills, like those of a normal person, might occur in quite distinctive or idiosyncratic forms.) But since alternate personalities appear quite suddenly and sometimes evolve rather quickly, their distinctive traits might emerge without any practice. (I realize we are very close here to the second set of issues — namely, whether Ducasse's principle applies to every skill, or just certain kinds of skills, and whether there are, accordingly, relevant differences between kinds of skills. I shall return to these issues shortly.)

Moreover, until one decides what to make of the case of Patience Worth (Cory, 1919; Litvag, 1972; W.F. Prince, 1927/1964; Braude, 1980), it is premature to dismiss super-psi—or simply non-survivalist—explanations of responsive xenoglossy. The medium in this case, Pearl Curran, with only an eighth-grade education, no apparent literary ability, and no apparent interest either in literature or in arcane areas of scholarship, suddenly began producing a steady stream of poetry, novels, and remarkably pithy and witty conversation through a ouija board. The material purportedly came from a personality named Patience Worth, who claimed to be a seventeenth-century Englishwoman. But there is little reason to think that the evidence supports the hypothesis of survival. Although Patience offered various clues regarding her origin and identity, subsequent investigation revealed nothing to indicate that a Patience Worth ever existed.

A more reasonable interpretation of the case is that it demonstrates, even more dramatically than the usual good cases of hypnosis, the power of dissociation to liberate otherwise hidden or latent abilities. Although all the Patience Worth communications exhibit a distinctive and consistent personality as well as common verbal traits, Patience expressed herself in several different linguistic styles. In fact, one of her works was a Victorian novel, despite the fact that (as the book's dust-jacket wryly noted) Patience was a pre-Victorian author. Most of the time, however, Patience communicated in a quite unprecedented style rooted

in archaic Anglo-Saxon idioms. Much of her vocabulary was appropriate to the seventeenth through nineteenth centuries, but some seemed to belong to a period several centuries earlier. Moreover, certain of the words she used on those occasions were tracked down by scholars only *after* they appeared in the Patience Worth scripts.

Many view Patience's literary works as being of exceptional quality, quite probably the best literature ever produced in a case of mediumship. But quite apart from issues of literary criticism, what matters here is that Patience Worth's poems and novels—and, indeed, her entire vivid personality—betray an intelligence and psychological style profoundly different from that displayed by Mrs. Curran. Furthermore, Patience's abilities and skills go well beyond anything Mrs. Curran (and, arguably, anyone else) ever exhibited. Patience was able to compose (often exquisite) poems on the spot, in response to requests to write poems on particular topics. She could compose several works (sometimes in distinct literary styles) on the same occasion, alternating passages of one with those of another. She could write part of a novel for a while, leave off in mid sentence to converse or work on something else, and then return to the novel the next day exactly where she had left off. More impressively still, with the exception of a beautiful child's prayer written haltingly and with a few revisions, Patience produced her entire corpus of thousands of poems and several novels without ever making a correction. She also performed astonishing compositional stunts. On one occasion she was asked to compose a poem, each line of which would begin with a different letter of the alphabet, from A to Z (omitting X). After a pause of a few seconds, the poem came through the ouija board as fast as the scribe could take it down.

The literature on dissociation, then, indicates that a person may apparently acquire, develop, or manifest novel abilities and skills under various kinds of abnormal circumstances. We are hardly in a position, then, to assert that the sudden appearance of new abilities and skills is impossible under even more extraordinary (i.e., paranormal) conditions. In fact, it seems we must plead ignorance here for reasons similar to those mentioned in connection with ostensibly paranormal propositional knowledge. We simply do not know what human beings are capable of under conditions we can scarcely comprehend.

Another (possibly deeper) set of problems concerns the way even sophisticated writers on survival (such as Stevenson and Gaud) generalize about skills. For example, Stevenson asserts, "Practice does not just make perfect; it is indispensable for the acquisition of any skill" (1984, p. 160). There are at least two related problems with that claim. The first is that skills can differ dramatically from one another in many respects, one of which is the importance of practice in skill development. I shall return to this point shortly. The second problem is that the *acquisition* of skills is not clearly the issue. All one is entitled to discuss, strictly speaking, is the *manifestation* of skills. We have no idea whether or to what extent new skills have been acquired by mediums or by the subjects of reincarnation investigations. This is not a trivial distinction, because practice is clearly not always needed to manifest skills for the first time.

To see this, one needs only to consider child prodigies and cases of savantism. In fact, typical musical prodigies such as Mozart, Mendelssohn, and Schubert, and mathematical prodigies such as Gauss, manifest exceptional skills prior to their being perfected or developed through practice. Moreover, it is of no use to protest that those prodigious skills were quite rudimentary when they first appeared, and that they simply evolved with amazing rapidity. For one thing, that seems simply to be false. For example, Mozart was able to write down a complex piece of music while composing another one in his head; but to my knowledge there is no evidence that he first had to practice that skill. But more importantly, we have no reason to think that the subjects in survival cases demonstrate levels of expertise more impressive than (say) Mendelssohn's initial displays of musicianship. Quite the contrary; the suddenly emerging skills of child prodigies often far exceed anything displayed by the subjects investigated in xenoglossy cases or other cases suggesting survival. But then, how do we know to what extent certain conditions (e.g., dissociation) may unleash prodigious capacities latent in many (or all) of us?

Of course, we do not need to consider prodigies and savants to appreciate this point. Ordinary folk demonstrate it all the time. Consider, for example, the skill of playing tennis. Many people are naturally athletic, even though they may not be prodigiously gifted. And to the occasional consternation of those who are less precocious athletically, natural athletes can, on their first try, play a game of tennis reasonably well—at least without looking hopelessly foolish. In fact, on their first try they might even play as well or better than others who have played for years, taken lessons, etc. More importantly, however, the initial tennis-playing skills of natural athletes would—at the very least—match the rather unimpressive linguistic skills displayed in the best cases of responsive xenoglossy. (There is even an interesting parallel between conversing in a language and playing tennis. Responsive xenoglossy involves more than the ability to form sentences in a new language; it also involves understanding and responding appropriately to sentences in that language. Similarly, the skill of playing tennis goes beyond being able to get the ball over the net and in bounds. It also requires being able to return shots.)

To complicate matters further, when Stevenson argues that skills cannot be communicated or manifested without practice, he mentions riding a bicycle, dancing, and speaking a foreign language as examples. Similarly, Gauld writes,

The ability to play bridge well is not simply a matter of learning (whether normally or by ESP) the rules (considered as a set of facts together with the precepts given in some manual). It can only be acquired by practising intelligently until things fall into place. And it is the same with learning a language. (1982, p. 102)

It appears, however, that there may be serious disanalogies between linguistic competence and these other skills. In fact, it is unclear whether one can even generalize about how difficult it is to learn a new language.

Let us take second things first and consider some aspects of language learning. To begin with, learning a second language may be a significantly different process from learning a language for the first time. And if the new language is not radically different from one's own, the sort of minimal linguistic competence displayed in cases of xenoglossy may require little more than some knowledge (that) of vocabulary and grammar, possibly paranormally acquired. After all, once one already speaks a language, a major part of learning a new language is exposure to it, whether that is through listening to actual conversations, or by watching movies, or listening to audio tapes in one's sleep (or while falling asleep). And of course, since we are entertaining paranormal hypotheses, we cannot rule out the possibility that subjects might have had the requisite exposure unconsciously and psychically. Sharada's mastery of Bengali, T.E.'s command of Swedish (or Norwegian), and certainly Gretchen's German (Stevenson, 1974, 1984) do not seem outlandish for an adult who might have been exposed to those languages extensively, but unconsciously (and even psychically), especially if we leave open the possibility that one's linguistic skills may be enhanced under dissociative or other unusual conditions. Moreover, the linguistic competence of Sharada, Jensen, and Gretchen is not as much of a feat as demonstrating a similar competence in a language *radically* different from one's own.

(I should note that there are cases in which mediums speak in languages (e.g., Hungarian, Chinese) which are quite different from their own and to which they presumably had no normal exposure. But quite apart from serious questions concerning the reliability of the data in those cases, in every case I'm familiar with some sitter present knew the language, and either they or someone else benefited psychologically in rather obvious ways from receiving communications in those languages. At best, then, the possibility of sitter-influence (including sitter-PK) and unconscious sitter-collaboration in these cases would be so strong that we are not justified in making much of the medium's apparently surprising linguistic competence.)

At any rate, if my earlier suggestions about language use are on the right track, then linguistic competence may differ significantly from other sorts of skills—for example, riding a bicycle, dancing, or playing the piano, expertise in which may be rather more independent of one's other abilities and skills. Of course, if one who can already dance performs a kind of dance he never learned before, that is considerably less impressive than a dance performed by someone previously lacking in muscular coordination and rhythmic finesse. That is why the musical compositions of Rosemary Brown are less than compelling. They are clearly continuous with musical abilities she had already displayed, just as Uttara's command of Bengali is clearly continuous with her already well-developed linguistic skills in another Indian dialect.

Apparently, then, what would be impressive *prima facie* evidence for survival is not merely the manifestation of a novel ability or skill, but rather an ability or skill substantially different from and discontinuous with those one has already displayed. But in that case, it is irrelevant to point out how difficult it might be



to acquire (or manifest) such skills as playing the piano or dancing without practice (ignoring, for the moment, the problem posed by child prodigies). The evidence for the persistence of skills suggesting survival contains nothing better than the evidence for responsive xenoglossy, and the best of those cases do not demonstrate the manifestation of skills radically discontinuous from the subject's other abilities. Hence, until someone does something comparable to playing piano, never before having played a musical instrument or exhibited any musical ability, I think we must conclude that this portion of the evidence for survival is considerably less impressive than its proponents have claimed.

### Notes

1. Both Stevenson (1974) and Gauld (1982) do this to some extent. But both authors are saddled with an impoverished picture of multiple personality disorder (see Braude, 1991). Moreover, they both fail to discuss some of the more impressive cases of dissociation, such as Patience Worth.

### Acknowledgments.

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## Survival or Super-psi: A Reply

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Although readers will soon see that I do not agree with Braude's assumptions, I warmly appreciate the trouble he has taken to consider alternative interpretations of evidence bearing on the question of the survival of human personality after death. Critics of this evidence have hitherto focused almost exclusively on its authenticity and have assailed its credibility at real or imagined points of weakness. It is encouraging to find oneself engaged in controversy over interpretation with an agreed assumption that the cases brought into the discussion actually occurred with satisfactory closeness to the reports we have of them.

In what follows I will first address Braude's argument in general terms and then comment on some of the illustrative cases that he discusses.

Braude argues that paranormal powers of living persons can better explain the evidence of the survival of human personality after death than the idea of such survival. He believes that the extraordinary paranormal powers required (by the types of phenomena adduced in favor of survival) may and probably do occur among living persons. Because we do not know the limits of such powers, he argues, there may well be no such limits. We are free to imagine a "super-psi" that can obtain information from anywhere and also (in its physical manifestations) move mountains (if need be). Braude disarmingly acknowledges that we have almost no evidence for the existence of paranormal powers of the magnitude required, and the reference he gives to his own book on psychokinesis hardly suffices to fill that gap. That we cannot directly observe super-psi (except in the cases to which he thinks it applies) does not seem to Braude a reason for denying its existence; but neither, one can surely argue, is it a reason for assuming that it does exist.

Super-psi will manifest, Braude believes, when a need arises on the part of a person capable of manifesting it. He states that "there are many reasons for thinking that psi-functioning is deeply need-determined." I agree that *some* psi-functioning is need-determined, but much is "flaw-determined" to use Ehrenwald's (1978) term. Ehrenwald considered that the (usually meager) results of laboratory experiments that demonstrate paranormal cognition show, when positive results do occur, expressions of flaw-determined psi. I also assign the cases of children who remember previous lives to the category of flaw-determined psi; the memories of these children appear to occur through a break in the normal process of amnesia. Even these two categories—of need-determined and flaw-determined psi—do not cover all cases of paranormal cognition. A small

number of persons have great powers of paranormal cognition which they have manifested without either need or flaw. They are talented in this way, just as other persons are talented in athletic, musical, or linguistic abilities.

Let us, however, accept Braude's first assumption and agree that at least some and perhaps much psi *is* need-determined. How can we demonstrate the imputed need? Braude believes that we can do this, provided we are given enough information about the subject's personality. As an example, he discusses the case of the Sharada personality manifested by Uttara Huddar (Stevenson, 1984). He appears to believe that a further probing of Uttara Huddar's personality would show motives for her manifestations of paranormal phenomena that are not revealed by the information that I elicited during my interviews. He invokes the word *psychodynamic* as if that term confers a license to assume motives that would be fully exposed, if only we were told more about the deeper layers of the personality in question. It is several decades, however, since psychoanalysts could overawe other psychiatrists and philosophers by asserting a claim to special insights concerning motivation. Psychoanalysis has become discredited because of its irrefutability. It claims always to hold the high cards. Braude states that the superficial interviews with which he reproaches me will not suffice to lay bare the subject's hidden motives. I suspect that if the interviews had been longer and deeper and the motives were still unrevealed, advocates of psychodynamic interpretations would call for yet more interviews to go deeper. The motives are assumed to be there if only we search long and well enough for them. I cannot agree with this assumption. In this connection Hempel and Oppenheim (1948) wisely remarked:

A potential danger of explanation of motives lies in the fact that the method lends itself to the facile construction of ex-post-facto accounts without predictive value. It is a widespread tendency to 'explain' an action by ascribing to it motives conjectured only after the action has taken place. (p. 143)

Turning from these general remarks I will comment on some of the examples that Braude introduces to buttress his argument.

### Cagliostro

About Cagliostro I will first point out that he should not be classed as a *drop-in communicator*. As the deviser of that term I am entitled to insist that we should apply it only to communicators whose existence is completely unknown to everyone present at a mediumistic sitting. Hyslop was certainly familiar with Cagliostro; and although Mrs. Chenoweth denied that she had ever heard of him, it seems almost inconceivable that she had not read popular accounts of his life in magazines or historical novels. Flournoy, writing at the turn of the century, described Cagliostro as "famous." (Flournoy, 1900, p. 115). Braude appears to accept Eisenbud's (1983) analysis of Mrs. Chenoweth's Cagliostro communicator without having made further inquiries himself. I think it improbable that the

allegations of sexual misconduct and blasphemy made by the Inquisition against Cagliostro were kept as secret as Eisenbud believes. Leaks from "classified" documents did not begin in this century. One can find hints of Mrs. Chenoweth's Cagliostro in sources that might have been available to her. For example, the 11th edition of the *Encyclopedia Britannica* (1910–1911, Vol 4, pp. 946–47) gives more than half a column to Cagliostro and states that in 1771 in London and Paris he was "selling love-philtres, elixirs of youth, mixtures for making ugly women beautiful, alchemical powders, etc." Spence (1920/1960) refers to a *Life of Cagliostro* published with the authority of the Inquisition after Cagliostro's trial. Spence quotes passages from this work which include descriptions of "the use of a young boy or girl, in the state of virginal innocence" during masonic rites that the author of this work characterizes as "execrable" and that we would today consider evidence of child abuse. I do not intend to suggest that the Chenoweth Cagliostro had behind it the real, supposedly surviving Cagliostro. I only question that the construction of the Chenoweth Cagliostro entailed any paranormal cognition, let alone super-psi. The case seems to me better explained as an instance of cryptomnesia with the details, to be sure, shaped by elements of Mrs. Chenoweth's personality. Her Cagliostro communicator differed notably from the earlier Cagliostro communicator of H el ene Smith (Flournoy, 1900).

### **Uttara Huddar and the Sharada Personality**

I have already implied that I do not believe further knowledge of Uttara Huddar's personality would contribute to our understanding the principal feature of her case: her ability to speak Bengali responsively. I can, however, clarify one matter that may be difficult for persons who are unfamiliar with India to understand. Braude suggests that Dr. Joshi's evasiveness in his interview with me and his visits to Uttara after she left his hospital may be clues to a significant relationship between them. In one of her first manifestations as Sharada, Uttara burst into a room where Dr. Joshi was dining with one of his female assistants (Stevenson, 1984, p. 74). That she had done so and what he had been doing when she came into his room then became more or less public knowledge in the community. Concerning this, there would be much more disapproval of Dr. Joshi in India than would occur regarding similar conduct in the United States. I believe Dr. Joshi did not wish me or anyone else to learn anything more of his private life than was already known around his hospital and to some extent elsewhere in Nagpur. Later, however, he would understandably have felt a compassionate interest in what had happened to Uttara. She had, after all, developed her changes of personality while under his care, and he may have believed that he had some responsibility for this. House calls by doctors have ceased almost entirely in the West, but in India Dr. Joshi's visits to Uttara at her home would be regarded as professionally appropriate. He may also have wished to know whether experts had confirmed his conjecture that the Sharada personality spoke Bengali.

### Patience Worth

Braude is incorrect in stating that I have failed to discuss the case of Patience Worth; I did so in my first work on xenoglossy (Stevenson, 1974, pp. 13–14). I think it is a mistake to subsume this case under multiple personality without qualification. One important distinction is that Pearl Curran, the automatist of the case, was never fully dissociated during the manifestations of Patience Worth. In her autobiographical statement she stated: “. . . while I am writing there seems to be no definite place where my consciousness ceases, and that of Patience comes in. Very early I began to notice that even while I was carefully spelling a poem, I was keenly conscious . . . of everything about me . . . ” (Prince, 1929, p. 398). In a passage before the one just quoted Pearl Curran had described how pictorial images came to her during her compositions and that she would sometimes see herself as within the scene she mentally viewed. This aspect of her experiences suggests that she may have had memories of previous lives on which she drew, both for the claimed life of Patience Worth and the poetic, fictional, and witty productions of Patience Worth. It is incidentally not surprising that no real Patience Worth was verified as having lived in seventeenth-century England, because Pearl Curran's Patience Worth gave only the scantiest of biographical information about such a life. This does not mean that no such person existed, but it is not necessary to suppose that one did. The conjecture that Patience Worth was a discarnate personality communicating through the mediumship of Pearl Curran does not fit the facts so well as the idea that she may have been a *persona* (to use Hart's [1958] helpful term), comprised partly of elements of Pearl Curran's personality and partly of elements of personalities from previous lives. The persona hypothesis also seems more adequate than attributing super-psi to Pearl Curran.

### Responsive Xenoglossy and Knowledge How

Although Braude refers to my citation of Ducasse (1962), he does not include Ducasse in his list of references, which suggests that he may not have read Ducasse's paper. He does not refer to the prime example of "knowledge *how*" that Ducasse uses. It is that of the "Lethe" case (Lodge, 1911). This remarkable case illustrates the possession and appropriate use of mental contents known to have been possessed by Frederic Myers but beyond the knowledge and capacity of the mediums concerned in the case. Ducasse admits that the mediums might have obtained by telepathy all the information communicated; but he denies (as I do) that the medium could also acquire by telepathy the skill needed in order to deploy the knowledge in the appropriate manner demonstrated by the Myers communicator in the Lethe case. Myers when alive, however, did have this skill.

So far as I know, Ducasse did not elaborate in other works his conviction concerning the importance for the question of survival of the distinction between knowledge *that* and knowledge *how*. The philosopher who did elaborate the distinction and might be said almost to have made it the basis for an entire philoso-

phy was Michael Polanyi (1958, 1962, 1966), whose works should be read by anyone who concerns himself with the interpretation of responsive xenoglossy. (Polanyi, I should add, had a constructive attitude toward paranormal phenomena; but he did not apply his conviction that we cannot learn a skill without practice to cases of responsive xenoglossy.) Although I have cited Ducasse and Polanyi in my writings I have never denied that the incommunicability of skills in a philosophical assertion, an axiom, if you like. It has so far received little support from empirical studies. A contribution by Cohen and Squire (1980) provides some such support.

I do not regard the examples of such child prodigies as Mozart, Mendelssohn, and Gauss as counter-examples supporting the concept of super-psi. Their cases enable us when we talk about skills to keep clear the important distinction between an *aptitude* and a *capacity*. It is certain that these three persons and other child prodigies had extraordinary aptitudes to learn the skills they acquired; I know no evidence, however, that they manifested the skills without practicing them. If they did, perhaps they brought the skill from a previous life. Indeed, they are often cited as instances of reincarnation; but not by me, because apart from their unusual talents they never gave the slightest indication of remembering a previous life.

The length of these comments testifies to my gratitude to Stephen Braude for his trenchant discussion of an important issue. No one should think that I am certain Braude is wrong; he may be right. I hope to have shown that even where disagreements persist understanding may be advanced. And it, in turn, may lead to more and better investigations that will bring us evidence permitting stronger conclusions than are now warranted.

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## Reply to Stevenson

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I'm glad to have the opportunity to reply to Stevenson's thoughtful comments, and in particular to clear up some confusions which I no doubt helped to create. Although Stevenson's remarks have not persuaded me to revise my views, this is precisely the sort of dialogue I had hoped my paper would stimulate. If nothing else, perhaps this exchange will manage to sharpen the issues further. And incidentally, I quite agree with Stevenson that it is a refreshing change to be able to debate the interpretation of the cases without the customary wrangling over questions of authenticity. Stevenson raises too many points to be dealt with in the space of a short reply. So I shall focus on those that seem to be most important, and I pledge to address the rest on a later occasion.

To begin with, Stevenson incorrectly characterizes my view as if it is a defense of the super-psi hypothesis *over that* of the survivalist hypothesis. He says, "Braude argues that paranormal powers of living persons *better explain* the evidence of survival of human personality after death than the idea of such survival." (italics added) But in fact, I only argued 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. My point was to show that super-psi explanations have been underrated and misunderstood, not that they are either clearly or even marginally preferable.

Stevenson and I disagree, apparently, over the degree of empirical support for the super-psi hypothesis. I would say that there are coercive reasons for accepting the reality of both large-scale and rather meticulous or fine-grained psi, quite apart from the evidence suggesting survival. So for me, the most interesting question in this debate is whether (or to what extent) we are justified in explaining the evidence for survival in terms of psi among the living.

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 may 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-fashioned 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 (e.g., systematicity and conceptual cost). Hence, claiming that an hypothesis is unfalsifiable is not the same as claiming that nothing can count against it. (I'll return to the topic of falsifiability shortly, in a different context.)

In fact, it is because of such higher-level considerations that I would argue, against Stevenson, that the totality of parapsychological evidence lends considerable plausibility to the super-psi hypothesis. To begin with, for those opposed to retrocausation, super-psi is perhaps the only way to make sense of Schmidt's experiments with pre-recorded targets (Schmidt, 1976), and also much of the evidence for ostensible precognition (Braude, 1986; Eisenbud, 1982). Moreover, the well-documented cases of large-scale psychokinesis discussed in my book, *The Limits of Influence* (Braude, 1986) reveal a degree of refinement in psychic functioning that may not be too far removed from what would explain away the apparent evidence for survival. Stevenson thinks that these cases show relatively little. I strongly disagree; and I invite the reader to study closely the best cases of ostensible materialization, and also the evidence for D.D. Home's accordion phenomena (in particular, the playing of tunes, on request, with the accordion either untouched or else held at the end away from the keys). Although these are not forms of psi cognition, if the phenomena are indeed genuine, they demonstrate that psychic functioning can be dramatically more subtle and controlled than one would think on the basis of ordinary laboratory experiments.

Moreover, they suggest the futility of ranking psi phenomena in terms of impressiveness after a certain point. I would say that a lifelike materialization or musical performance on an untouched instrument is already extremely impressive, as are the cases of ostensible precognition most plausibly explained in terms of telepathic influence and detailed psi cognition of contemporaneous states of affairs. And it is far from clear how to determine at what point thereafter psi phenomena become "super," or whether there is *any* clear measure or scale of impressiveness according to which super-psi explanations of the evidence for survival require manifestations of psi that are substantially more virtuosic. (In fact, for reasons I have discussed elsewhere (Braude, 1989, 1991b), and to which I allude below, it is unclear whether or to what extent virtuosity or task complexity is even an issue.)

Interestingly, Stevenson touches on these matters when he claims that some "persons have great powers of paranormal cognition which they have manifested without need or flaw." He claims that such persons are analogous to musical or athletic virtuosi. I have two comments about this. First, I wonder how Stevenson knows that the psychic functioning of such persons is need-independent. If it is on the basis of the sorts of superficial psychological profiles characteristic of the case studies of survival, I would suggest his claim is groundless. But perhaps more importantly, Stevenson raises the issue of what psi "talent" is. Is psychic functioning unevenly distributed (like musical and athletic abilities), or is it something we all do some or all of the time, but which only a few can control consciously or manifest in clearly detectable ways (analogous to a yogi's control of blood flow)? Stevenson seems to assume the former is obvious. I'm rather inclined toward the latter, and I have argued for its plausibility on the basis of very broad considerations, concerning both the available evidence for psi and also the nature of abilities and skills generally (see Braude, 1991b).

Because he is a psychiatrist, perhaps the most interesting part of Stevenson's reply is his objection to psychodynamic explanations. But before examining Stevenson's own position, let me once again correct a rather overstated characterization of my own view. According to Stevenson, I claim that we *can* show that psi is need-determined if only we delve deeply enough, and in particular that "a further probing of Uttara Huddar's personality *would* show motives for her manifestations of paranormal phenomena that are not revealed by the information that I elicited during my interview" (italics added). However (and this is not merely a quibble), I argued only that further investigation might show this. I did not argue, as Stevenson maintains, that "motives are . . . there if only we search long and well enough for them." My general point was simply that (a) unless we look, we'll never know, (b) almost no one looks for hidden motives, and (c) often we don't have to look very hard to find them. Hence, in the Uttara case, I urged simply that there are very good reasons for penetrating more deeply than Stevenson and others have done.

Interestingly, Stevenson transforms his objection to my position into an attack on psychoanalysis. But my paper is not an endorsement of psychoanalytic theory, or any other specific approach to depth psychology. One hardly needs to advocate psychoanalysis in order to assert plausibly that people operate from motives discoverable only through deep psychological investigation of some kind or another. Hence, Stevenson's attack on psychoanalysis is irrelevant. (I should add, however, that Stevenson's negative assessment of psychoanalysis is also not quite the received view he makes it out to be. But that issue should perhaps be reserved for another occasion.)

In any case, Stevenson's appeal to the alleged unfalsifiability of psychoanalytic (or any depth psychological) claims is clearly unsatisfactory. Virtually every hypothesis about another person's mental states (deep or otherwise) is equally unfalsifiable. Yet attributions of mental states are both meaningful and useful. Indeed, some people are clearly better at it than others, and that is why being a

good judge of character is a pragmatically valuable form of real empirical insight or perceptivity.

Oddly, Stevenson's remarks here read like an argument in defense of ignorance. In light of the philosophically quaint passage from Hempel and Oppenheim, Stevenson seems to eschew *any* explanation of behavior in terms of motives, superficial or deep. But Stevenson surely doesn't want to forego all explanation of human behavior (or even just ex-post-facto accounts) in terms of hidden needs or motives. Not only would that be antecedently preposterous and totally impractical in day-to-day life; it would also be a tacit admission that *all* of psychiatry is methodologically unsound. Besides, (as one would expect) Stevenson quite properly considers the role of hidden needs and motives throughout his writings—for example, in connection with the possibility of observer (or reporter) bias, misinformation or fraud. In fact, Stevenson actually considers the sorts of hypotheses I recommend taking seriously, without challenging them on the grounds that one should not speculate about hidden needs or motives. For example (as I noted in my original paper), Stevenson entertains the hypothesis that the Sharada personality reflects Uttara's frustrations over never having married.

To repeat, the point of my remarks is that until the survival cases are filled out more, the individuals involved are no more than psychological stick figures, and that neither the investigator nor the reader is in a position to rule out super-psi explanations based on hidden needs or motives. I am not claiming that super-psi hypotheses *will turn* out to be confirmed on the basis of depth psychological investigations. And I make no recommendations of psychoanalytic accounts over numerous alternatives. I claim only that Stevenson's psychological profiles of relevant individuals have been unacceptably superficial, not that they fail to conform to psychoanalytic theory.

To avoid making this counter-reply interminable, let me offer a few brief comments or corrections regarding the specific cases Stevenson discusses. First, in connection with the Cagliostro case, since the criticisms of Cagliostro noted by Stevenson are not allegations of religious blasphemy or raging concupiscence, they do not undercut Eisenbud's super-psi explanation of the Cagliostro-persona. Eisenbud was not claiming that only favorable descriptions of Cagliostro were accessible to Mrs. Chenoweth. His point was that only the secret Vatican account attributed to Cagliostro some of the most vivid traits displayed by the Cagliostro-persona.

Regarding the Patience Worth case, I was aware that Stevenson mentions it briefly in his first work on xenoglossy. But I do not regard those three paragraphs as a "discussion" or adequate treatment of the case, especially considering how it can be used to undermine arguments for postmortem survival. Moreover, I did not claim that the Patience Worth case was one of multiple personality. I merely considered it to be a particularly vivid example of dissociation (see Braude, 1991a for a detailed examination of multiple personality and the concept of dissociation), and my point was to suggest that dissociative states might unleash both normal and paranormal abilities to a greater degree than we

usually suppose. And I certainly cannot agree with Stevenson that it is more plausible that Pearl Curran received some of her information and skills from previous lives than from super-psi. I might be willing to make that leap if there were independently coercive evidence for reincarnation. But (a) we already have first-rate evidence for pretty dandy psi, along with a great deal of evidence that dissociative states are psi-conductive; and (b) in light of that evidence, the significance of the Patience Worth case is precisely that it forces us to reconsider the evidence for reincarnation in the ways noted in my paper.

Finally, Stevenson apparently misunderstood the point of my discussion of child prodigies. I did not argue that such cases provide evidence (or support the concept) of super-psi. Rather, my point was a fairly modest one concerning the nature of skills. It was that our normal skills and capacities might far outstrip the relatively meager limits we customarily assign to them, and that the true extent of our non-psi capacities might be hinted at in cases of dissociation, savantism, etc. Hence, I argued that we must look to such cases to get a clearer picture of what humans might be capable of normally (say, through cryptomnesia).

I appreciate Stevenson's gracious comment that he is not certain I am wrong. I should add that I feel the same about Stevenson's position. And in case it wasn't clear enough already, let me say that I have no axe to grind either against the survival hypothesis or in favor of the super-psi hypothesis. In fact, I am actually somewhat biased in favor of the former, since I can imagine few discoveries more momentous and exciting than the reality of postmortem survival. I simply think that it is exceptionally difficult to make a good case for survival, and that one major obstacle is the super-psi hypothesis, which most researchers have either underestimated or misunderstood.

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