CORRESPONDENCE

To the Editor:

I found Etzel Cardeña's review of my coedited book *The Survival of Human Consciousness: Essays on the Possibility of Life after Death* (*JP*, **70**, pp. 179–185) to be a reasonable and fair evaluation of most of the chapters. However, some of his comments and views are unfounded or incomplete. It is clear that, in some cases, Mr. Cardeña is unaware of the circumstances pertaining to the preparation of the book, and on other issues he is rather casual in his criticisms, or misleading in his approach. I therefore find some of his statements considerably and problematically misrepresent the book. I would like to address these concerns.

First, it is true that "two [chapters] are reprints from years-old sources" (p. 179), but that is not to suggest to *JP* readers that the content of those two chapters is antiquated and anachronistic compared to current research, nor that the book is of any less value in terms of originality than it could otherwise have been. The fact that Cardeña later praises Haraldsson's chapter (previously a journal article) as featuring "a good case" (p. 182) lends partial support to my claims, but it should be noted that the chapter by Boyd and Zimbardo (also a reprinted article) focuses on a completely different aspect concerning survival belief not considered in the parapsychological literature. More importantly, it is likely that those readers of *Survival* (especially those with a cursory interest in afterlife research) who have read only one of these two chapters in its original form have probably not read the other. This is because Haraldsson's chapter comes from a parapsychological journal (*JSPR*) whereas Boyd and Zimbardo's article comes from a nonparapsychological journal (*Time and Society*—a journal[1] that "[discusses] the workings of *time and temporality* [italics added] across a range of disciplines. . . .").[2] However, that is not to say that serious researchers in the field and/or readers with considerable interest in afterlife research would not be familiar with this nonparapsychological source, and/or have not cited relevant literature from that source, but I very much doubt it in this case. More to the point, selection of the above two articles was intended to support two of our aims for the book, these being: (1) to collect a diverse range of relevant subject matter either original to the book or not easily accessed by the typical reader and thereby (2) not over-represent any given theoretical or methodological approach or school of thought.

Second, Mr. Cardeña faults the book for not featuring other key researchers (unambiguously worded as "crucial omissions"), and he offers

[1] Not a chapter "reprinted from a 1997 book" (p. 181), as Cardeña states.

[2] See http://www.sagepub.com/journalsProdDesc.nav?prodId=Journal200801

two example studies (one of which post-dates the publication date of *Survival*). It should be noted that the contributors to *Survival* are primarily highly experienced researchers of long standing in the field, and many other key figures were invited to contribute but declined, including Gary Schwartz and Stephen A. Braude,[3] absences of whom Cardeña mentions and bemoans. We maintain that these "omissions" do not detract from the quality of the book, and only to a limited extent do I agree with Cardeña that the presence of these recommended researchers might have been good for the book. In all fairness, Cardeña, having been an editor himself, knows full well that compiling the ideal book is nothing more than an ideal in itself and the final product is always likely to fall short of the original plan. Furthermore, most of his short-listed researchers who might have contributed (i.e., J. Beischel, T. J. Robertson, A. E. Roy, G. E. Schwartz) specialise in mediumship-type research that is sufficiently well-featured throughout *Survival* as it is (e.g., chapters 4, 11, and 12), and the inclusion of their work would only undermine point (2) above. It is also debatable whether the work of these researchers is above reproach as the methodology of *at least* one of them has been criticised by Ray Hyman (2003). Also, typical arguments from skeptics, or what may be viewed as skeptical approaches, have been subthemes throughout *Survival* (see chapters 1, 2, 3, 4, 5, and 15), and it is a moot point whether anything further, old or new, whether from a "well-informed and well-intentioned skeptic (such as [E. R.] Dodds . . ." (p. 180) or more contemporaneous skeptics, would have added much to the debate. More likely their contributions would merely labour the point. Besides, another key aim of the book was to give special attention to the debate between the two major afterlife positions: super-psi and survival per se, not to present skeptical arguments from, for example, psychopathology (though this has also been covered—see especially chapter 5).

Third, the criticism of starting the book on a "very wrong footing" is in no way justified. Giving the afterlife issue an historical context is perfectly acceptable and reasonable given the book's main theme, but I will not stress this point as it may be a matter of "horses for courses," and Cardeña is entitled to his opinion. More importantly, I regard it as unfair and below the belt to undermine Keith Chandler's scholarship just because a mere 3 of 22 references in Chandler's list of references are his own books, which just happen to be self-published. Put in context, Chandler's "vanity" texts, which are very well researched and referenced, were used primarily to describe issues to do with human ontology, interpretation of mystical experience, archaeology, and civilisation in general, as opposed to advocating and justifying a specific theoretical position on the survival issue. It is up to the reader—enthusiastic amateur and seasoned scholar alike—to discern whether Chandler's arguments have any soundness to them or not, and that can be a well-informed, well-considered decision based

[3] Note, however, that Braude's key arguments and ideas from his book *Immortal Remains* (Braude, 2003) are given ample coverage in chapter 15.

exclusively on Chandler's works, irrespective of where and how his sources were published. As it happens, Chandler undertook courses in theology, philosophy, and psychology at the University of Chicago in the 1950s, and it is possible that this information (unfortunately not completely stated in the Contributors section of *Survival*) might have ameliorated Cardeña's berating of Chandler's scholarship.

Fourth, and in regard to my two chapters (particularly the "contentious" chapter 15), I can hardly feel guilty for stirring the pot when it comes to challenging taken-for-granted beliefs and ideological positions that laypersons and researchers hold dear to their hearts about survival issues. It must be remembered that ideas about super-psi and survival per se are unproven *theories* at best. Like Cardeña, I am not "troubled" so much by the fact that there are two chapters from me rather than one over-sized chapter, and I wonder why it was necessary to even raise what boils down to a trifling but pragmatically determined decision about formatting, when it seems to me that *JP* readers might have preferred a critical theoretical review, or brief outline *at least*, of *radical survivalism*, which stands as the sum-convergence of the main ideas in all the relevant chapters of the book. In fact, Cardeña's *contentious* (to use his word) accusation of a purported "meandering and obtuse" style of "writing and reasoning" (p. 183) hardly enlightens the *JP* reader as to the content of my two chapters. And to take a quote[4] out of context (p. 183) merely detracts from its meaning by isolating it from the theoretical points that lead up to and follow that quote. But it certainly does not stand as a reasonable means of demonstrating a "meandering" or "obtuse" style. Furthermore, if there are any grammatical or theoretical flaws in my chapters, Cardeña will have to do better than say as much and throw in a quote out of context—a literary trick that can make any author's writing appear flawed, even a reviewer's. If one feels it necessary to go all out and make a scathing assessment of an author's ability, one is expected to back it up with a modicum of examples and accompanying explications if one wishes to maintain one's credibility as an objective critic and reviewer.

Cardeña wraps up his review with brief statements about two "recurring themes in some of the *best chapters* of the book" (p. 184, italics added):

> . . . what may survive death is not likely to be our ordinary personality but something that underlies that self, or perhaps a field of shared consciousness. . . . The other is that findings in research on various anomalous experiences (e.g., NDEs, OBEs) remain very pertinent to our understanding of what may or may not survive. . . . (p. 184)

[4] Actually taken from page 289, not "p. 299."

Though true to a degree—these themes *are* often repeated—this action misleads the reader. It not only undermines a number of other important issues covered in all the chapters, even those that Cardeña has effectively down-rated quality-wise, but also fails to address my attempt at synthesizing various contributors' key ideas. I agree with Cardeña that what may survive may not be "our ordinary personality" (p. 184), but he then refers to the idea of fields of consciousness—discussed by Stokes (chapter 3), Roll (chapter 8), and Krippner (chapter 9)—but my chapter 15 does anything but promote field theories, and Cardeña might have mentioned this fact to give the reader a truer (i.e., well-rounded) perspective of the book.

I also agree with Cardeña's other conclusion, that "various anomalous experiences . . . remain very pertinent to our understanding of what may or may not survive" (p. 184)—these ideas *are* maintained by some of the contributors. For some people, NDE and OBE experiences may indicate survival, but for others, such experiences may have nothing at all to do with survival insofar as they are merely various (undoubtedly exceptional) forms of *altered states of consciousness* (like ordinary dreams, waking dreams, lucid dreams, etc., all of which may or may not involve psi mediation) that can be explained *completely* within the parameters of *living* systems without needing to appeal to the "discarnate entity" concept. However, what does not come to the *JP* reader's attention from Cardeña's review is that these ideas provided fertile ground for *radical survivalism*, but they are not the only ideas, and it is important to mention that (1) a number of key theoretical notions from Jung, Thalbourne, and Braude are incorporated into *radical survivalism* (along with findings and ideas from the book's contributors as already mentioned), and (2) other scientific conventions are actually undermined by *radical survivalism*, including arguments from complexity, causality, and parsimony, and (albeit tentatively) the "paranormality" of super-psi, which may only be a superior form of cognition.

Cardeña's implication that the be-all and end-all of *Survival* can be reduced to well-known "recurring themes" that would, in all likelihood, already be familiar to most readers anyway, not only inappropriately casts my coedited book in a mundane light shared by a host of humdrum and repetitive texts but undermines the intention in the book's concluding chapters of offering alternatives to hackneyed and conventional viewpoints that may well be retarding the advancement of afterlife research.

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To the Editor:

As a response to criticism, the eminent science writer Freeman Dyson (2007) recently opined that he usually learns more from his critics than from his flatterers. I wish that Lance Storm had shared this humble and generous attitude. Although I endeavored to provide a comprehensive review, pointing out strengths and weaknesses, he aggressively (I am "casual" or "misleading" in my criticisms) attacks my review, shooting the messenger rather than dealing with the message. Because he does not raise substantial points against my critiques, my reply will be brief.

First, however good they may be, I see a problem in reprinting earlier publications in an ostensibly "new" book, as the buyer is asked to dole out money for items that s/he may already have or could obtain freely. What is typically done in anthologies is to have an author write a new chapter that will at least update what s/he had previously written. Mr. Storm and his coeditor chose not to do this.

Second, of course no anthology is perfect, but the editors are ultimately responsible for what their book does or does not include, no matter the circumstances. The fact is that authors carrying out programmatic research on mediumship (the two groups I mentioned plus Emily Williams Kelly and her group) are absent from the anthology, as is arguably the foremost current philosopher of survival issues. It is fair game for a reviewer to point out what s/he thinks may be lacking in a book. As for not including a well-informed and well-intentioned critic of survival, I maintain that parapsychology should not be different from any discipline in which constructive dialogues can break impasses (think, for instance, of the Honorton-Hyman debate on ganzfeld).

Third, Mr. Storm's take on Mr. Chandler's chapter actually strengthens my point. My main criticism was about the quality of the chapter, not that it was a historical overview, and the fact that Mr. Storm cannot offer any better defense for Mr. Chandler's chapter than that he undertook some related courses in the 1950s reinforces my position. Mr. Storm does not offer any evidence that Mr. Chandler had had any previous relevant peer-reviewed publications.

Fourth, and final, the issue with Mr. Storm's two chapters is not that he was proposing a challenging theory, but that he did so in a very unclear fashion. This is not a matter of context but of writing. In the passage I quoted in my original review (I did give an example, contrary to what Mr. Storm writes), there was a single sentence, out of various similar ones, containing 108 words and about 20 different clauses. If the reader finds this and similar passages in Mr. Storm's chapters clear and not meandering, s/he is definitely a smarter person than I am. To finish this response, and to come back to Dyson, I can only wonder what Mr. Storm learned from the review he wrote of his own book for amazon.com, after giving it five stars.

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To the Editor:

Caroline Watt's (2005) thoughtful and stimulating presidential address to the Parapsychology Association raised several points that could help parapsychology. For example, she pointed out that the proposals in a recent article by me on "A Proposal and Challenge for Proponents and Skeptics of Psi" (Kennedy, 2004a) would enhance the evidential value of meta-analysis. That point is true, but it may be useful to extend the discussion beyond enhancements to meta-analysis.

In particular, effective implementation of the proposals I suggested could virtually eliminate the need for meta-analysis. If appropriate power analyses were incorporated into the design of studies as recommended in my paper, 80% of clearly identified confirmatory or pivotal studies would be expected to be statistically significant, assuming that psi experiments conform to the assumptions for standard statistical research. A few such studies would provide strong evidence for psi without the need for meta-analysis and the associated controversies over alternative methods, criteria, and outcomes.

In the areas of medical research I currently work in, large well-designed studies are given greater weight than meta-analyses. The situation was well summarized in a recent book on statistical methods in cancer research:

> Our inclusion of [meta-analysis] in a chapter on exploratory analyses is an indication of our belief that the importance of meta-analysis lies mainly in exploration, not confirmation. In settling therapeutic issues, a meta-analysis is a poor substitute for one large well-conducted trial. In particular, the expectation that a meta-analysis will be done does not justify designing studies that are too small to detect realistic differences with adequate power. (Green, Benedetti, & Crowley, 2003, p. 231)

Among the medical researchers I have worked with in recent years, the conclusions of a meta-analysis are typically accepted only to the extent that they are supported by statistically significant results from large well-designed studies. In making the transition from parapsychology to other areas of research, the greatest adjustment for me was to start recognizing the fundamental importance of power analysis and the value of large well-designed studies.

To put the matter in concrete terms, I know of no well-designed ganzfeld studies. As noted in the previous article (Kennedy, 2004a), a reasonable power analysis for ganzfeld studies indicates a sample size of at least 192. I know of no ganzfeld studies with a preplanned sample size of at least 192. There are some cases in which a series of studies were combined to reach such a sample size, but these appear to have been done in a post hoc manner, and often combined exploratory studies that had variations in methodology or design. For example, the widely cited Bem and Honorton (1994) article is a meta-analysis of a series of studies from one laboratory. Based on a power analysis from previous research, each of those studies was severely underpowered and therefore would be considered poorly designed by the standards I work with now. I do not believe that post hoc meta-analysis can compensate for poor designs. In addition, the variability among the studies combined with the negative correlation between effect size and sample size raise doubts about the meta-analysis. A negative correlation between effect size and sample size is normally diagnostic of bias in a meta-analysis (Egger, Smith, Schneider, & Minder, 1997).

Of course, these arguments and proposals assume that psi conforms to the properties of standard statistical research. As noted in the previous article (Kennedy, 2004a), I have come to expect that the results of large confirmatory psi studies will not be more reliably significant than the results of small exploratory studies, which is contrary to the basic assumptions of statistical research including meta-analysis. In addition to the references in my previous article, a more recent meta-analysis of PK studies with electronic random number generators found that the *z* scores (significance level) did not increase with sample size and that effect size was negatively related to sample size (Bosch, Steinkamp, & Boller, 2006).

The authors of the meta-analysis proposed that the pattern of results was due to publication bias rather than PK, but admitted that they could not provide convincing evidence for that hypothesis. Such controversies are common in meta-analyses in parapsychology. It is also noteworthy that a similar pattern of results occurred in the Bem and Honorton (1994) ganzfeld meta-analysis when publication bias presumably could not have been a factor because it included all studies of a certain type from one laboratory.

I suspect that the basically universal disregard in parapsychology for power analysis and for the value of large studies reflects the fact that most psi researchers implicitly (perhaps unconsciously) recognize that psi does not conform to the assumptions for standard statistical research. However, efforts to provide convincing evidence for psi will fail if the experimental results have unexplained properties that are inconsistent with the statistical foundations for the claimed evidence. Cautious scientists will continue to favor methodological problems as the most likely explanation, particularly if the results are unpredictable and appear to be associated with certain experimenters.

The finding that *z* score does not increase with sample size implies that the standard methods for data analysis including binomial tests, *t* tests, and analysis of variance do not have their usual meaning and applicability in psi research. The experiment as a whole may be the appropriate unit of analysis rather than the individual trials or subjects as assumed for those tests. The hypothesis of goal-oriented psi experimenter effects is logically consistent with the basic assumptions for psi research (Kennedy, 1995) and now has strong empirical support that the outcomes of psi experiments are typically unrelated to sample size. Appropriate statistical methods for this type of phenomena remain to be developed. Using statistical assumptions that do not fit the phenomena will inevitably result in failure to make scientific progress.

A two-stage statistical strategy may be needed. The first stage would be based on normal statistical methods to provide evidence that something anomalous occurred. The second stage would utilize more novel statistical assumptions appropriate for the phenomena. The concepts of goal-oriented psi (Kennedy, 1995) and evasive psi (Kennedy, 2004b) may be useful starting points for developing relevant methods.

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To the Editor:

I am currently engaged in research into the relationship between religiosity (defined in a number of ways) and paranormal belief/experience. I was therefore very interested to see the results that Hergovich, Schott, and Arendasy (2005) recently presented in this *Journal*. They found differences in this relationship depending on whether the participant was Catholic (a number of significant correlations), was Protestant (almost no significant correlations), or was without any religious affiliation (many and high correlations).

I am in a position to examine in a small way whether this pattern of results emerges in a reanalysis of a sample of Australian psychology students (Thalbourne, 1995). Unfortunately, the only variable we have word-for-word in common is self-reported religiosity. For my entire sample of 242 students, the correlation between self-reported religiosity and the 13-item Australian Sheep-Goat Scale was $r_s = .12$, which is borderline significant, $p = .054$, two-tailed, and which thus shows a tendency to replicate the significant Austrian result where they used their entire sample. For my 53 Catholics, there was no such correlation, $r_s = -.05$, $p = .722$, and therefore no replication in this case. For the 60 Protestants, the correlation was $r_s = .17$, which is not significant, and therefore replicates the nonsignificant Austrian result. For the 122 persons without a religious affiliation, the correlation was $r_s = .20$, $p = .024$, and therefore the result replicates that of Hergovich and colleagues. Note that I had a further category, "Other," but this was heterogeneous, since it included Jews, Moslems, and members of Eastern religions, and is, as well, very small ($N = 7$). For both these reasons I did not analyze it.

I also used the Haraldsson (1981) eight-item religiosity scale, and it is of interest that the entire sample correlated with the sheep-goat scale significantly, $r(240) = .23$, $p < .001$, and was significant for Protestants, $r(58) = .31$, $p = .016$, and to a similar degree for the nonaffiliated, $r(120) = .37$, $p < .001$, but not at all for the Catholics, $r(51) = .05$, $p = .735$. There is evidence that the Haraldsson scale correlates moderately with Tobacyk's Traditional Religious Belief Subscale, $r(125) = .44$, $p < .001$ (Thalbourne & O'Brien, 1999, p. 117), a subscale that Hergovich and colleagues used, but they found uniformly strong correlations for all affiliations between paranormal belief and Traditional Religious Belief, and thus their Catholics behaved differently to mine in this respect.

My suggestion for future research is that the authors perhaps repeat their study, but using also the Haraldsson Religiosity Scale—a research direction that would link their studies to a large database of studies that have used this scale (Thalbourne, submitted). There is, however, a cloud hanging over the Tobacyk Paranormal Belief Scale and its subscales, a cloud that seems to have been dispersed by the finding that Rasch top-down purification of the scale leads to just two factors, called New Age Philosophy and Traditional Religious Belief (Lange, Irwin, & Houran, 2000). The 18-item Australian Sheep-Goat Scale has likewise been top-down purified and is available for researchers who want one of the most sophisticated psychometric measures of the paranormal belief variable that there is (Lange & Thalbourne, 2002).

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To the Editor:

Richard Broughton's obituary of John Beloff does proper justice to the work of an outstanding scholar and a gentle and sensitive human being, and I hope he will not mind if for historical reasons I comment on one of the subjects upon which he touches. Richard writes that when attempts were made to find a home for the Koestler Chair the 'leading universities [in the UK] were consulted. In some cases the universities were not interested or not willing to take the risk, in others the universities wanted an inappropriate definition of parapsychology or too much control over the appointments. For this reason, the Chair went to Edinburgh University. In fact my University (Cardiff) was certainly prepared to take the risk, and to my knowledge we made no attempt to provide an inappropriate definition of parapsychology or to demand too much control over the appointments. The fact of the matter is that the University received a four-page handwritten letter from Prince Charles (the heir to the throne and Chancellor of Cardiff University) encouraging us to offer a home for the Chair. In response the Principal of the University appointed a four-man committee, of which I was a member, to submit our offer, and John Beloff came to Cardiff to meet the committee and discuss matters with us and view the facilities available. We were able to offer exclusive use of a large and suitable building opposite the main premises of the University, and the Chair would have had virtual autonomy. Given also the University's geographical position, its status in one of Britain's three capital cities (London, Cardiff, and Edinburgh) and the interest of Prince Charles, we considered we had a good case.

In the event John decided in favour of Edinburgh. He was the best judge, and the success of the Koestler Unit at Edinburgh under Bob Morris proved him right. My concern therefore is simply to emphasise that Edinburgh was not the only University with a genuine and accommodating interest in the Chair, and to draw attention to the very positive attitude of Prince Charles to Koestler's vision and to psychical research in general. These facts are, I think, worth placing on record.

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To the Editor:

In the first paragraph of her response (*JP*, **70**, No. 1) to my letter dealing with her review of my book *Is There an Afterlife?*, Emily chides me with the statement "we cannot afford to go public—whether in writing or in speech—with anything less than complete, accurate, and fully documented accounts of our observations and research." The relevant point she raised in her review was concerned with proofreading errors in the book and had nothing to do with my own "observations and research," and I wonder if she now really means to imply criticism of my descriptions of this research. If she does, I should be grateful for the details.

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To the Editor:

I am not sure I understand why my general phrase "our observations and research" led David to infer that I meant "his" observations and research specifically. By "our observations and research" I was referring to the 120 years of research that collectively represent the efforts of those who have contributed to survival research, and of which David's book was meant to be a "comprehensive overview." In my review I described some of the deficiencies that I found in this book, many of which were not simply "proof-reading errors," but there is no need to go over those again here.

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