

Review of: "Mediumship for Pets: A Pilot Study With a Triple-Blind Protocol"

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Paul F. Cunningham's Review of "Mediumship for Pets: A Pilot Study with a Triple-Blind Protocol" by Patrizio Tressoldi, Laura Liberale, and Fernando Sinesio.

One strength of the paper is its originality in that it builds upon an existing area of research (human mental mediumship) and extends it into another related one (nonhuman mental mediumship) that has been previously ignored or overlooked. Another strength is its *potential* impact in the fields of bereavement counselling and therapy, "continuing bonds" and "meaning-making" research by advancing understanding of the frequently occurring anomalous experience of "sensing the presence of the deceased" in bereavement (Steffen & Coyle, 2010). In the present case, the "sense of presence" experiences would refer to those of a deceased beloved "companion animal"—a term I prefer over the word "pet" used in the article's title that carries unwelcomed connotations of domination of one species over another. The notion of animal mediumship might make a potential contribution in the field of clinical parapsychology and the training needs of therapists who work with clients who report such "sense of presence" experiences (Roxburgh & Evenden, 2021). I say "potential" impact because much more revision needs to be done if the study is to even begin approaching such a possible application and if the results are to gain any plausibility in the parapsychological and wider psychological communities.

One key area for improvement is the conceptual and theoretical *clarity* of the article. First, there is not enough information contained in the present report of this quasi-experiment for the study to be replicated. Second, insufficient context is provided in the Introduction section to give the study any meaningful importance beyond the parapsychological community. Third, the lack of discussion of the results in the Discussion section makes the conclusions of the study unpersuasive.

Introduction Section

The issue of animal mediumship and the related question "Does *personal* animal consciousness survive beyond physical death" is a challenging topic to investigate because the whole notion of *human* mediumship remains controversial. There are so many assumptions hidden behind this research that are not addressed in either the Introduction or Discussion! So many issues are presumed settled, as implied by their lack of discussion in the Introduction. For example, what makes the researchers so convinced that (a) the nature of animal consciousness affords possible mediumship communication in the first place, (b) that animal consciousness includes 'personality' and psi capacities as a basis for *personal* contact between sitter and animal, (c) that anything,—human or nonhuman—in fact survives biological death, and (d) that mediumship is a

valid way of obtaining information from *human* survival personalities, never mind from deceased *nonhuman* animals.

An adequate “Context of the Problem” and “Problem Statement” needs to be formulated. On what basis might such a context and problem statement be formulated? It would arguably inform the reader about the status of scholarship related to (a) the controversy of human mental mediumship (e.g., Rock, 2013), (b) the existence of animal psi (e.g., Dutton & Williams, 2009; Sheldrake, 2004, 2015), (c) evidence in favor and against the survival hypothesis (e.g., Sudduth, 2024), (d) anomalous bereavement experiences (e.g., sense of presence of a departed beloved pet) (e.g., Steffen et al., 2017), (e) how human-animal communication works *in life* (Humphreys, 2023; Kulick, 2017), and (f) the “spiritual” bond that may be established between pets and people (e.g. Beck & Katcher, 1996; Randour, 2000). As it stands, the perceived plausibility of the present pilot study’s hypotheses and the validity of its conclusions stands in need of further development. Although some may regarded this activity (i.e., review of the pertinent literature) as relatively meaningless and to be treated lightly, it is, I would argue, a significant part of the *pilot* research process and necessary for establishing its credibility.

I recognize that this is merely a *pilot* study. I acknowledge its limited and specific focus: Test whether an established human mediumship protocol that is designed to contact human discarnates can be meaningfully and legitimately applied in a related context (i.e., to contact nonhuman discarnates) in order to obtain data for analysis to compare outcomes, perhaps leading to revision in the human mediumship protocol for use with a different (nonhuman) target population, based on the results of the comparative study. As a part of that focus, however, it could contribute much more to answering important questions that would advance understanding of mediumship in general, such as

- Did the pilot test uncovered any failings in the human mediumship protocol (e.g., lack of discrimination, preponderance of inappropriate responses, poor instructions, and administration problems) that would make it unsuitable for use in animal mediumship studies?
- What variables have proved important or unimportant in human mediumship research that makes the researchers convinced that “human mediumship” variables can be meaningfully extended or applied to “pet mediumship,” given the differences and similarities *between species* in terms of their possible nature as “spiritual” creatures (Cunningham, 2023a, 2023b)?
- What does studying animal mediumship using experimental protocols used to investigate human mediumship have anything to say about the nature of the potential survival of animal consciousness.
- What variables of human mediumship are/are not important when considering the possible success/failure of animal mediumship?

Ideas about variables that have proved important or unimportant in human mediumship and their relationship to variables in animal mediumship are totally ignored or overlooked in the present study.

As a *pilot* test of the suitability of using a mediumship protocol specifically designed for communicating with discarnate *human* animals to communicate with discarnate *nonhuman* animals (dog, cat, squirrel), this study has limited value in advancing the parapsychological cause in the wider psychological science community, unless it can broaden its frame of reference within the context of some of the above-mentioned issues. Discovering important variables, distinguishing

between what has been done from what needs to be done, establishing the context of the research problem, and showing why the research is important or significant can all be done in the Introduction section. A five-sentence Introduction just does not do the job.

Methods

Participants

A nonrandom, nonprobability *purposive* sampling technique used in a study that solicits 20 volunteers via social media who are already familiar with the researchers work introduces conscious or unconscious bias in the sample. How do the researchers know that a sitter's judgment of the "correctness" of a reading was not due to some subconscious collaborative assistance on the part of participants who may well want to please experimenters they already know by virtue of being "connected with" their research groups.

As the authors themselves note at the outset, any generalization of the study's results/conclusions to the population of interest is statistically not justifiable. So we are left with research findings that apply only to a sample which may or may not be representative of the target population (i.e., people who are interested in using mediumship to communicate with their deceased companion animal).

The pilot study data set (AnimalMediumship20) provides *no* information regarding the composition of this "sitter" sample and so its potential representativeness of the larger target population is impossible to determine. More demographic information about the participants (so-called "sitters") might alleviate this concern. How do the researchers know that any of the sitters did not fabricate or make up the pet they submitted for a reading? Did they control for this? With absolutely no demographic information about the so-called "sitters" who did not "sit" in the presence of the medium (and are more like "drop-ins" but not even that), but who were responsible for adjudicating the mediums' readings, it is difficult to assess the reliability of their judgments.

Perhaps more pertinent, little to no information is provided about the "pets" in this study. In the human mediumship data file (ListCommonQuestions.docx), the mediums have 10 items of information about the human discarnate that seek to contact, whereas in the animal mediumship study the only information provide to the mediums is the name of the pet and the owner. The human/animal mediumship protocols would be more equivalent if the list of information provided to the medium that related to the intended (requested) pet better matched the human mediumship protocol by providing a comparable amount of information about the deceased animal—for example, cause of death of the animal, what the animal likes to do most, likes/dislikes to eat, specific physical characteristics, characteristic natural skin marking. No information about when the pet died is solicited. That is not important?

I had a cat named "Blackie" who died two years ago. How many cats and dogs exist in the world named Blackie? It seems incredible to me that with *only* the name of the pet and owner, the medium is able to target exactly the precise animal (e.g., *my* Blackie) out of a population of *all* animals (after all, Blackie might be the name of my turtle) and come up with a

precise and specific correct reading. The definitely specified limitations of a “cold reading” may partially account for their failure to differentiate pet-control from pet-intended conditions. Usually some emotional contact with either the sitter or the deceased needs to be established for successful readings. Without that emotional bridge or conduit or connection established, what is happening here? How does this “animal mediumship” independent variable work anyways? Some theory needs to back up the practice (i.e., “There is nothing more practical than a good theory,” Kurt Lewin once opined) and without a good theory to explain the supposed results, it all seems like “magic.”

More background on the six mediums beyond the fact they were female would also be helpful since it is *their* Readings that are being adjudicated. Useful additional information about the mediums could include: How were they selected? How many years of mediumship experience and what sort of experiences did these mediums have? What training have they had? Have they ever conducted a pet mediumship before? How “expert” are they? How does that “expertise” in human mediumship translate or transfer to the discarnate animal domain? A look at the data file reporting the success rate of individual mediums disclosed that, although the overall correctness rating was 63%, the success rate of individual mediums ranged from 0% to 100% with two mediums accounting for half of the readings with 60%-80% correct readings. This, itself, is interesting data that requires further discussion. Any success in the animal mediumship endeavor may well depend on the “power” of this variable that will be reflected in the background experience, training, and especially the *ability* of the medium. It is evident that the six mediums were not all equally “expert.”

Procedure

There is not enough detail in the article to replicate the study. For instance, it would be helpful to see examples of the two lists of information (reading) related to the intended (requested) pet (i.e., pet-intended condition) *and* the second list of information related to the pet that served as control (i.e., pet-control condition). It is unclear who was responsible for comparing the two lists (Reading A and Reading B) to make sure that the same *type* of animal was referenced in both readings prior to being sent off to the sitters for their “correctness” judgments.” A squirrel or dog would be hard not to detect in the control list, if a cat was the sitter’s requested pet. Who fabricated the “fake” information in the pet-control condition and what control was in place to make sure that such mismatches did not occur?

The use of a “triple blind” procedure is a methodological operation that does not actually “allow for” the quantitative analysis of data but is merely a control that reduces the opportunity for “noise” to enter into the data collection process. If “super psi” does exist (Braude, 2002; Sudduth, 2009) or if psi is already operative in everyday life as some parapsychologists propose (e.g., Carpenter, 2012), then triple blind procedures merely address the *conscious* knowledge that is available to those involved in the pilot study and does not really control for or prevent access to that information on a *subconscious* basis by psi of the primary investigator, research assistants, participating “sitters,” or mediums. My point is that the triple blind procedure does not make the information unavailable. It just reduces some of the situational “noise” that may be going on in the study.

Dependent (Variable) Measures

One or two illustrations of how each of the three quantitative variables are calculated would go a long way to clarify what each quantitative measure means (i.e., “Overall percentage of correct information;” “Global reading score;” and “Identification of the intended reading”). As it stands, the three calculated dependent variables are vague and difficult to match to the variables in the open access data file (AnimalMediumship20).

Data Availability

The availability of the raw data in open access is to be applauded. A key to help decode all variable names in the (AnimalMediumship20) data file described more clearly in the body of the article would be useful in helping the reader *map* the data to the three dependent variables used in the pilot study.

Statistical Comparisons

I will not go into my own major concerns over the statistical comparisons that are conducted in the pilot study since one of the reviewers (Dr. Gosselin) has already done a good job pointing them out. For example, why the researchers continue to insist on using a one-tailed test that is ordinarily employed to predict the direction of a difference between groups (i.e., a directional hypothesis) instead of a two-tailed test that merely states that the population means are different (i.e., a non-directional hypothesis) remains unclear to me, despite all the justifications presented by the primary investigator. The whole point of the study is to assess whether the same protocol can be used in a different (nonhuman animal) context with the implied expectation that it can. Given that nothing is explicitly stated in the Introduction that implies or even hints at an expected difference between pet-intended and human-intended conditions, given that the only hypothesis that *is* explicitly stated is “the hypothesis that there are no statistical differences,” it seems clear that the non-directional (“no difference”) null hypothesis is the only one being tested. Statistical hypotheses are directional or nondirectional, not “focused” or “unfocused.” If one-tailed tests are to be used, then a directional prediction should be explicitly stated at the outset and the reasons why a direction is expected formally explain. Perhaps the one-tailed test was selected because it is one way of compensating for the lack of power due to sample size?

I do have a few additional observations.

One relates to the second dependent measure of **‘Global reading score’** (“OverallScoreIntend” and “OverallScoreCtrl” variables in the data file?). It has been adopted from Julie Beischel’s excellent empirical work of putting mental mediumship to the test of experiment. The assumption that Beischel’s scale is an equal interval scale—that is, that the distances between any two numbers on the scale are known and of equal size—needs to be revisited in the context of the present study. Questions arise as to whether this is not better treated as an ordinal (rank-ordered) scale. For example,

- Is the interval between option “5” (“Reading is good but **contains very few incorrect** points”) and option “4” (“The reading is good, but **contains some incorrect** information” really the same as the interval or “distance” between option “2” and option “3”?
- Can response options “5” and “4” be reliably distinguished or they are measuring the same judgment? The word “very”

in option “5” is imprecise and ambiguous.

- What does it mean when someone circles option “2” that “some information is correct, but not enough to be certain of real communication with the deceased (pet)”? Why or in what sense is there not “enough” information?
- What does it mean when someone circles option “3” of “not enough information to be sure, but enough to indicate there was indeed communication”? On what basis does the sitter judge “not enough to be sure but enough to indicate...”?

My point is that the practice of *presuming* equal intervals on scales such as this is widespread because it justifies the use of parametric statistics such as Student's t test. In the present case, however, the global score data are better thought of as providing ordinal data (without the assumption of equal intervals), especially given the nonprobability sampling issue.

My second observation relates to the use of a nonrandom, nonprobability sampling technique in the present study that severely limits the techniques of statistical inference that are justifiably applicable. In general, all parametric *tests of significance* assume random selection of participants from a normally distributed target population. Given the absence of random selection from the target population of interest (or random assignment to groups), the assumptions of normality, homogeneity of variance, and that sampling errors will be random are not justified that precludes the use of parametric statistics such as confidence intervals and the t-test.

For example, I am puzzled by the researchers' use of the parametric **independent t-test** as a statistical test for comparisons between human-intended and pets-intended readings when neither group (or conditions) constitute a random, probability sample required by the t-test and do not meet all the criteria requirements for using Student's t test—i.e., traits being measured do not depart significantly from *normality* within the population from which the samples were selected [normal distribution requirement]; *standard deviations* of the two samples are fairly similar [homogeneity of variance requirement]; sample scores provide at least *interval scale* data).

Considering the Global Reading Scores to be *ordinal* ranks instead of equal interval magnitudes allows one to use the assumption-free nonparametric statistics. Nonparametric tests require few or no assumptions about the nature of the population from which the sample(s) was drawn and provide greater generality in the conclusions to be drawn. For example, rank-ordering data would permit the use of Mann-Whitney U Test in place of the independent t-test for two ordinal distributions with independent selection. One advantage of using the Mann-Whitney U test is that it does not make any assumptions regarding the parameters of the population they represent or the shape of the underlying distribution. The disadvantage is that it is less powerful (i.e., less sensitive to smaller differences and fail to reject H_0 when it should have been rejected), but when the assumptions of parametric tests cannot be reasonably met (e.g., random selection, normal distribution, homogeneity of variance, equal interval data), nonparametric tests are a safe bet. Even transformation into nominal data and the use of Chi Square or the sign test, given the small sample size, would be appropriate.

Nonparametric related-samples tests for comparing *pets-control* and *pets-intended* readings using the Sign Test which focuses on the median difference between groups rather than the mean difference would be analogous to the paired-t-test and is something to consider. The same comparison can be handled using the Wilcoxon T test—a statistic that the authors use in their statistical comparison between the intended and control readings. Both nonparametric matched-subjects designs for related sample requires that the data be converted into *ordinal (rank-order) data* in order for the

Wilcoxon T-test analysis to be performed. No mention of ordinal data, however, is made anywhere in the Results section.

A third observation relates to the issue of non-comparable groups in all pet-intended and human intended statistical comparisons. The exceedingly large difference between the size of the two samples (i.e., $n = 20$ for the pets-intended group and $n = 100$ for the humans-intended group; degrees of freedom = $n_1 + n_2 - 2 = 118$...not the still uncorrected “180” degrees indicated elsewhere that is attached to the two t-tests used) raises a red flag. Although it is best to come as close to the ideal of equal sample sizes in the two groups, putting 1 mediumistic reading in the first group (pet-intended) for every 5 mediumistic reading in the second group (human-intended) is not the best way to proceed in constructing comparable groups and likely sets up unequal variances in the two groups making the two groups incomparable further. Are we comparing apples with oranges here? How are 20 animal-intended readings provided by six medium equivalent or comparable to 100 human-intended readings provided by how many (?) mediums?

Discussion and Conclusion

	Pets-Control vs. Pets-Intended	Pets-Control vs. Human-Intended	Pets-Intended vs. Human-Intended
Overall percentage of correct information	$p = .11$	$p = .01^{**}$	$p = .13$
Global Readings Score	$p = .076$	$p = .047^{*}$	$p = .43$
Readings Identification	---	$p = .01^{**}$	$p = .43$
	No significant differences (null results)	Significant differences	No significant differences (null results)

Some sort of data table presented in the article would be most helpful for interpreting the data and drawing plausible conclusions about the meaning of the results of the study. Putting aside the issue that inappropriate inferential statistical tests have been used and momentarily giving the researchers the benefit of the doubt regarding their choices, I've summarized the *p-values* of the various statistics comparisons in the table above for discussion purposes only.

First, the statistically significant differences between the pet-control vs. human-intended groups is logically expected, but the reason or purpose for making this comparison in the first place is unclear and seems trivial (i.e., much ado about nothing).

Second, the absence or lack of a significant difference between the pet-intended vs. human-intended groups is more interesting, but difficult to interpret. The researchers argue that the inability to detect a difference (low power) is due to small sample size, but there are a lot of other possible reasons for the null result—for example, (1) ineffective manipulation of the independent variable—i.e., psi ability of the medium; (2) insufficiently sensitive measurement of the dependent variable to detect group differences —i.e., Beischel's scale, sitters' self-reports; (3) measurement error—i.e.,

imprecision of measurement in sitters' ratings; questionable construct validity of Beischel's scale; (3) individual differences in motivation and ability of mediums and "sitters"; (4) situation noise. If, and this is a big IF, everything about the two protocols are the same except *who* the mediums are communicating with (human vs. nonhuman animals), then do the null results really indicate human and nonhuman discarnates are communicating in the same way? This is what the researchers imply when they stated in their conclusion that their "results suggest that animal mediumship may share similarities with human mediumship." How on earth can the researchers reach this conclusion without any idea how human mediumship works in the first place? This remains an uncertain hypothesis, given species differences in the way human and nonhuman animals communicate in life and what we don't know about the nature of psi or how human mediumship actually works.

Third, what is most puzzling is the fact that no significant differences were detected between the pet-control vs. pet-intended conditions. The "animal mediumship" readings and the "fake" readings were judged to be equally true and equally false. No *statistically* significant difference between the two! This finding stands in need of further interpretation. Perhaps animal mediumship does not operate at all. The absence of differences between pet-intended and human-intended mediumship could mean that human mediumship does not operate either just like the animal mediumship from which it does not differ. It is comforting to know, though, that human mediumship can be distinguished from "fake" animal mediumship.

My point is that there is simply not sufficient discussion of the results in the Discussion or Conclusion sections to advance understanding of what is happening in the pilot study in a meaningful way. It may be the case that a human mediumship protocol might be able to be used in animal mediumship research, but the pilot study presents "not enough information for an assessment." The functions of a Discussion section are important ones that the pilot study in its present state does not fulfill:

- Summarize the findings of the study in the form of conclusions supported by the evidence;
- Interpret the findings and tell what they mean;
- Put the pieces together to achieve meaningful generalizations;
- Integrate findings into an already existing theory or use them to formulate an original theory;
- Make some recommendations and suggest extensions.

The pilot study arguably does the first of these functions but only in five sentences and in a declarative fashion without further explanation. None of the other purposes that a Discussion is meant to fulfill are addressed. The two-sentence conclusion at the end of the study makes claims that may be considered unwarranted simply because the data does not support them.

The Discussion and Conclusion sections may be the most difficult to write because they are the least structured. Yet these sections follow and build upon the Introduction section that provides a frame of reference. The Discussion section is intended to address the points raised in the Introduction in light of the study's results. The absence of an adequate Introduction inevitably leads to an inadequate Discussion. In the Discussion section, the researchers have a chance of tying the results to both theory and application in a way that pulls it all together. The present study, unfortunately, does

neither.

Conceptually, the pilot study is a good one but there is basically not enough information contained in the article to replicate the study, not enough context provided in the Introduction to give support to plausibility of the hypotheses, and not enough discussion (or data) to give convincing support to the correctness of its conclusions. The topic is an important one and I would encourage the researchers to continue to puzzle their way through the complexity of issues (e.g., survival of animal consciousness after biological death and the possibility of post-mortem communication between the living and the dead) that such “hard” problems of consciousness inevitably present. I would not recommend publication of this pilot study in any journal in its present form, however.

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