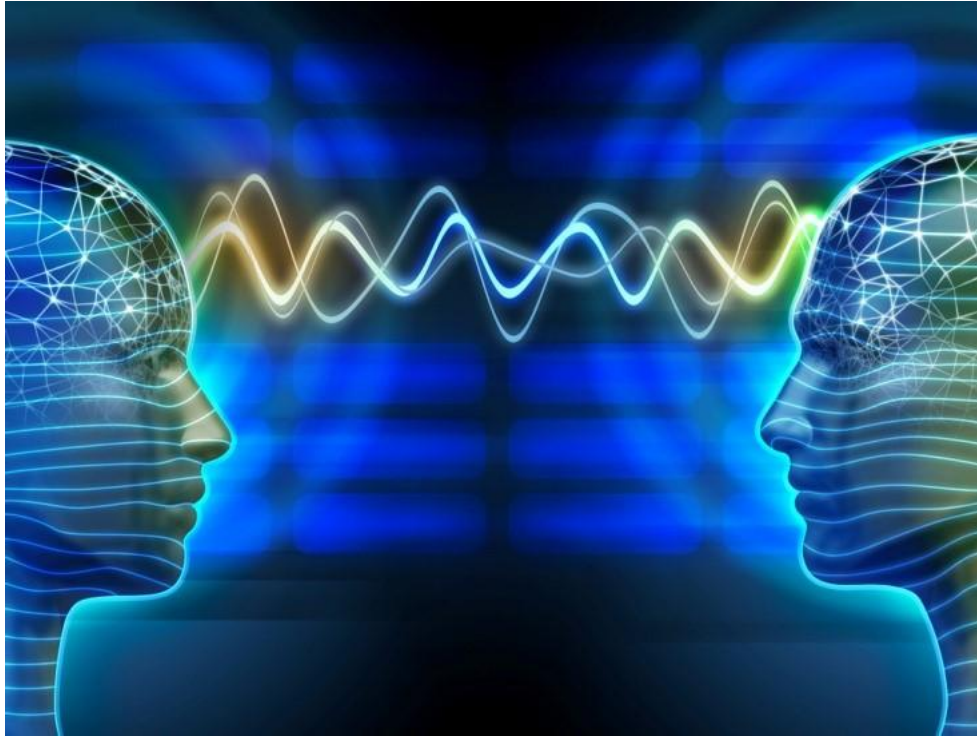


A Critical Commentary on Jonathan Jarry's (2024) Article "The Telepathy Tapes Prove We All Want to Believe"

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On December 13, 2024, McGill University's Office for Science and Society [posted an article on their website](#) by molecular biologist Jonathan Jarry (2024) that critiques the recently released podcast series *The Telepathy Tapes* hosted by journalist Ky Dickens, which has featured some of the research on ostensible psychic (or *psi*) phenomena in autistic children that has been conducted by first author D.H.P. (Powell, 2015, 2016). In response, we offer here a critical commentary on Jarry's (2024) critique that is comprised of two parts: The first presents D.H.P.'s rebuttal to the critical statements made by Jarry with regards to the podcast and her research, and offers some clarity on where she currently stands on the matter. In the second part, we focus our comments specifically on the second half of Jarry's critique (subtitled "A Psi of Relief for Scientific Rigour"), in which he tries to paint a wider (and rather misleading) picture of the field of parapsychology in general.

Rebuttal to the First Half of Jarry's Critique

In the first half of his critique, Jarry (2024) launches an attack on D.H.P. by delving into and attempting to smear her professional background, stating:

Powell is described on the show as a neuropsychiatrist, researcher and author, and she tells Dickens that she went to medical school, worked with "some of the greatest minds in

neuroscience,” and joined the faculty at Harvard. She is no longer at the vaunted university. Additionally, in episode 6, she claims her medical board revoked her license after she published her book, *The ESP Enigma*, which makes the case for psychic phenomena. Powell asserts that members of the board hadn’t read the book; after they reviewed her research, they reinstated her license.

This story of censure in the face of paranormal beliefs is not supported by the information I found. According to publicly available documents from the Oregon Medical Board, Powell’s license was indeed suspended around the time her book was released, but the reason was a pattern of practice including “poor management of therapeutic boundaries, incomplete chart notes [...], a disorganized approach to treatment, a failure to respond to significant patient symptoms, and concerns over her management of patient medications.” She was accused of relying extensively on phone consultations to manage complex psychiatric patients without seeing them face-to-face, thus posing “a significant risk of harm to these patients.” When asked to stop practicing medicine during the investigation, Powell declined. Her licensing board also asked her to undergo a psychiatric evaluation, which she did. She was subsequently allowed to resume practicing medicine under a strict list of conditions, which were waived in April 2012. The medical board’s website now lists her license as “lapsed.”

In response, D.H.P. offers these clarifying statements:

Jarry misrepresents what happened between me and the medical board. He says I have a lapsed license, but I got my license back at the very next board meeting after it had been taken away. I don’t have an active license now because I retired last year. I wrote a letter to the medical board at that time saying that I was retiring from medicine after 40 years of practice so they wouldn’t think it was an oversight on my part. I felt compelled to retire because I did not want to be practicing when *The Telepathy Tapes* came out, in large part because I did not want to go through anything like I did before with the board, but also because I wanted to devote my time to research instead.

If needed, I have a copy of the letter sent by a psychiatrist to the board that specifically tells them to take my license away on an emergency basis because I wrote a book on ESP, even though that is not what they put in the public record. It’s not that shocking when you realize that psychiatrists are trained to believe that any belief in ESP is a sign of mental illness. Here is the background: I became a subject of the board’s investigation after a depressed patient went against my advice and received a medical marijuana card from another provider. He became manic and was unwilling to stop smoking cannabis. He knew I’d be unhappy about what he did and didn’t return for follow-up. When I heard he was manic from his wife, I tried to talk him out of using the drug by phone, but he wouldn’t listen. I never charged him for these calls, so my notes for them were handwritten and not in the typical SOAP format. His son assumed I must be doing something wrong and filed a complaint against me when his father started crossdressing in public. He wanted me to hospitalize his father against his will and found it hard to understand that the laws did not allow it because he was not a danger to himself or others. When I initially heard of the complaint, I thought it would easily go away, but it didn’t. Once an investigation begins, they don’t just look at the complaint. They look at everything, including the Internet. They are also an administrative law court, which means that you are presumed guilty until proven innocent...not the other way around. When I met with them, the board asked me if I ever did any other sessions by phone. I admitted to having phone sessions with two other individuals who had been my patients in San Diego. Although I kept my California license active, I transferred their medical care over to another psychiatrist before I left. I acted as a consultant to her while she managed their medications and was willing to meet with them by phone when they needed it. Both of them were on the autistic spectrum and victims of sexual abuse by family members during childhood. They only felt comfortable talking with me when certain issues came up because I understood them so well and they had major trust issues and shame.

Because the board members were doctors from other specialties and didn't understand the therapeutic alliance/bond, they were mystified by my willingness to continue to be involved. They thought it was a boundary issue and said I was either narcissistic to think I was irreplaceable or suffered from "pathological compassion." Ky Dickens mistakenly stated in the podcast that the board gave me my license back because they reviewed my research, which I told her to correct upon hearing because they never looked at my research or my book. They gave it back to me because dozens of angry doctors and patients wrote letters and called them on my behalf to complain about this action. I agreed to a full outpatient forensic psychological assessment that took place over three days. It gave me a clean bill of health and I then practiced in good standing for 12 years before I chose to retire one year ago.

In discussing the testing conducted with autistic children, Jarry (2024) states:

Anyone who is familiar with facilitated communication....will be shaking their head in recognition. Facilitators hold a nonverbal person's arms or hands, thus pointing and typing for them, essentially ventriloquizing these individuals. What Dickens witnesses in *The Telepathy Tapes* are offshoots of facilitated communication, namely Spelling to Communicate (S2C) and the Rapid Prompting Method (RPM). Often, the facilitator holds the board up in the air and can, either consciously or subconsciously, move it to make sure the speller points at the right letter or cue the speller in ways they may not be aware of. Defenders of these methods will argue they're not touching the child's arms or hands, but subconsciously moving the board results in the same problem: it's not the child doing the selection.

Being aware of the controversy surrounding facilitated communication involving any form of touch, D.H.P. has had similar concerns. As she has recently expressed elsewhere (Powell, 2024):

RPM is a controversial communication method labeled scientifically invalid by the American Speech-Language Association....because it requires prompts on the part of the facilitator who holds the letter board. The aim of the prompts is to keep autistic individuals on task because they often have a tendency to fixate on a particular letter or number when spelling out their answers. Due to the active role of facilitator, the communications obtained by RPM are considered by most speech therapists to be the facilitator's and not the child's. This is not always a correct assumption, because occasionally the child goes on to type independently and eventually proves that they were indeed the communicators. Furthermore, RPM has had some major successes and enabled some non-speakers to go to college. Perhaps the most famous is Elizabeth Bonker. (p. 279)

Speaking further within the context of tests conducted with a 13-year-old autistic girl known as "Elisa," D.H.P. goes on to add that with touch-based facilitative communication there also is

...the potential for subtle cueing. Being touched would be considered a red flag from the standpoint of skeptics. However, when many children are first learning RPM, they do best while being touched by a parent. At the time of testing, Elisa had been using the RPM boards for such a short period of time that she required far more assistance from her mother to communicate than desirable. Anxiety also played a role in creating more need for tactile support. That is particularly the case in initial experiments being filmed on camera. To avoid the issues caused by RPM, I have primarily tested under controlled conditions autistic children who are able to speak and/or type independently [Powell, 2015, 2016]. However, the documentary filmmaker following my research wanted to start by filming experiments that were being set up for the first time and all of my untested children were using RPM. (pp. 279 – 280)

In light of this, D.H.P. went on to state:

After our experiments the entire sound and camera crew walked away with the same impression [that the children were exhibiting telepathy]. No one visually detected an obvious pattern that could be considering cueing. All told, there were at least ten witnesses, some of whom were filming from multiple camera angles. Nonetheless, the conditions were clearly not optimal for proving telepathy and we cannot definitively say that there was no cueing without more tests and a detailed analysis. (p. 280)

Comments on the Second Half of Jarry's Critique

As he begins the second half of his critique, Jarry (2024) states:

Parapsychology is a grab bag of powers and experiences, like telepathy, telekinesis, and precognition, that involve some weird transfer of energy or information and that currently exist outside of our scientific understanding, if they exist at all.

Like other staunch skeptics (e.g., Novella, 2020; Reber & Alcock, 2019), Jarry seems to be characterizing psychic phenomena based on certain initial assumptions being made about those phenomena, and one crucial question about this is: How do we know those assumptions are correct? For instance, how do we know that telepathy involves "...some weird transfer of energy or information," as Jarry presumes? How do we know that these phenomena would "...currently exist outside of our scientific understanding?" Are these based on actual empirical findings gleaned from parapsychological data, or are they merely unfounded guesses? It would seem the latter is more likely here, for as pointed out previously (Williams, 2019, 2020), such assumptions do not appear to be valid, as they do not appear to be consistent with the empirical data that parapsychologists have gathered over the years. (For additional discussion of other issues related to opposing arguments made by staunch skeptics, readers should also [see the commentaries by Etzel Cardeña, Andrew Westcombe, George Williams, Bernard Carr, and this author](#) contained in Braude, 2019). This suggests that perhaps we should be mindfully cautious about acknowledging what we know (and what we don't know) about a certain phenomenon before making certain presumptive assertions about it.

Jarry (2024) then states: "Looking back almost 200 years, we can see that parapsychological claims have time and again been disproven by skeptics asking for better measures to prevent deception," Two counterpoints arise in relation to this statement:

First, Jarry's use of the term *disproven* harkens back to a point once made by the late Marcello Truzzi (1995, 1996) involving the conventional distinction between non-proof and disproof. As Truzzi (1995) stated: "If the burden of proof placed on a claimant in science is not met, fellow scientists presume the negative only in the sense that the claim has been 'not proved'" (p. 284). He further elaborated on this later: "In science, both those who claim a phenomenon and those who deny it bear a burden of proof. The position of science is that, in the absence of adequate evidence, something claimed (whether a positive or negative claim) is considered *not proved*; it is not considered *disproved*" (Truzzi, 1996b, p. 280, emphasis in original). And so as Truzzi (1996a, p. 190; 1996b, p. 280) argued, in using such a term, the burden of proof would be upon the critic (Jarry) to show that parapsychological claims have indeed been "disproven."

And that would then lead to the question: Where are the negative studies by the skeptics? Jarry cites none in his article, and if one digs a bit deeper, another underlying issue may be found with the argument. Back in the 1980s, well-known mathematician (and staunch skeptic) Martin Gardner (1983) had made a similar argument:

How can the public know that for fifty years skeptical psychologists have been trying their best to replicate classic psi experiments and with notable unsucccess? It is this fact, more than any other, that has led to parapsychology's perpetual stagnation. Positive evidence keeps coming from a tiny group of enthusiasts, while negative evidence keeps coming from a much larger group of skeptics. (Gardner, 1983, p. 60)

But as the late Charles Honorton (1993) later pointed out: "Gardner does not attempt to document this assertion, nor could he. It is pure fiction. Look for the skeptics' experiments and see what you find" (p. 194). And indeed, when one does look, they may find a relative handful, but not a "much larger group." And when those handful of studies are examined, one may find that they tend to be problematic, as well: For instance, even though a number of staunch skeptics have criticized the use of cards in ESP tests, some skeptics have still made use of them (Polidoro, 2018; Rinolo & Schmidt, 1999), and it may not be clear from the reports as to what method of shuffling was used to achieve adequate randomization. Some telepathy tests conducted by skeptics (e.g., Polidoro, 2018) have had the telepathic "sender" & "receiver" located in the same room, and not in full sensory isolation from each other as one might ideally expect. And the well-known tests conducted with Natasha Demkina (often popularly referred to as "The Girl with X-Ray Eyes")(Hyman, 2005a; Skolnick, 2005) contained a number of issues with sensory leakage and statistical evaluation (e.g., Abbott, 2005; Hall, 2005; Hyman, 2005a, 2005b; Nagy, 2005; Sheskin, 2005; Stokes, 2009, pp. 182 – 184). And some ESP test methods designed by staunch skeptics have not been adequately controlled (e.g., Rao, 1984). At most, these would all lead to inconclusive findings, not disproving ones.

Moreover, not all of the skeptics' studies have led to null results: In a series of studies (Delgado-Romero & Howard, 2005; Howard et al., 2009; Lau et al., 2009), a collaborating group of skeptical researchers associated with Norte Dame and Columbia Universities had produced (to their chagrin) several significant positive results in their attempts to replicate the ganzfeld ESP experiment.

In considering these points, Jarry's own stated claim of "disproven" does not seem to be very well supported.

Second, Jarry does not seem to recognize that parapsychologists have already been long aware of "...skeptics asking for better measures to prevent deception," and that they have actually taken steps to address that particular issue. Several points relating to claimant deception and how to minimize it in designing tests for psychic ability have been raised before by a number of parapsychologists (e.g., Auerbach, 1988; Cox, 1984; Hansen, 1990), and stage magicians have been consulted for this purpose (e.g., Ford Kross and Daryl Bem in the procedural design of the PRL autoganzfeld ESP experiments reported by Bem & Honorton, 1994, p. 10). The ganzfeld system utilized at the University of Edinburgh's Koestler Parapsychology Unit was also designed with the help of skeptic Richard Wiseman (Dalton et al., 1996). Yet in spite of these steps, parapsychologists have continued to obtain significant overall results using the ganzfeld ESP test design (Tressoldi & Storm, 2021).

Jarry (2024) then goes on to state:

The history of research into "psi" phenomena, as they are often called today, is the history of how scientific research became more rigorous. You say your client can see through envelopes? Let's see if they can do so in front of a group of scientists, and let's make sure those envelopes are opaque. You say they can read the mind of the person next door? Let's make sure the room they are in is properly insulated so they can't hear what their partner is saying. You say they can pick a card that was chosen by someone else? Let's ensure the card-picker's fingerprints aren't visible on the card they chose.

One thing that Jarry does not mention is that it is also the history of how parapsychological research has continually improved in its methodological rigour. If we go point by point:

“You say your client can see through envelopes? Let’s see if they can do so in front of a group of scientists, and let’s make sure those envelopes are opaque.”

Merely take a look at the methodological designs and results of the various ESP card tests reported by J. B. Rhine and colleagues in the book *ESP After 60 Years* (Pratt et al., 1940);

“You say they can read the mind of the person next door? Let’s make sure the room they are in is properly insulated so they can’t hear what their partner is saying.”

Look at the steps taken to improve sensory isolation between participants in the methodological designs of the aforementioned ganzfeld ESP tests (Bem & Honorton, 1994; Dalton et al., 1996);

“You say they can pick a card that was chosen by someone else? Let’s ensure the card-picker’s fingerprints aren’t visible on the card they chose.”

This issue, casually referred to as the “greasy fingers” hypothesis, was addressed way back in the 1980s by John Palmer (1983, 1986) in order to see what effect it might have. While he found that some telepathic “receivers” were able to notice such physical clues (when specifically asked to look for them), there was no clear indication from the results that they significantly affected the test outcomes. Moreover, this became a moot issue in more recent ganzfeld ESP experiments, where the telepathic “receivers” viewed digital images on a monitor screen (and thus, did not look at copies that were physically handled).

All of these would seem to work against Jarry’s (2024) further claim that: “As psi research became more stringent, the errors became harder to spot by the average person.”

Jarry then goes on to comment on the well-known series of precognition experiments conducted by Daryl Bem (2011), mentioning that the magazine *Scientific American* had fun with their headline: ‘Extrasensory pornception.’” As staunch skeptic Michael Shermer (2011) had mentioned in his op-ed piece on these experiments, this was a playful joke used by comedian Stephen Colbert when Bem appeared on his show. But arguably, if you have to resort to using it in an *ad hominem* way to dismiss Bem’s findings off-hand (as some staunch skeptics seemingly have), then you don’t really have much of a sound argument.

Jarry next brings up the recently reported results of the large-scale Transparent Psi Project (Kekecs et al., 2023), which was designed to replicate the first of Bem’s (2011) four types of precognition experiments. He makes light of its chance outcome, which should indeed be taken into account in a future meta-analysis alongside the other replication results that have been reported with this particular experiment in the latest analysis so far (Bem et al., 2016). But it is also important to recognize that this would only affect the significant results for one of Bem’s experiments, and not the other three that have been reported (also in Bem et al., 2016). Moreover, even if Bem’s overall results were greatly affected by this large study, there are still several lines of notable experimental results relating to precognition (such as the presentiment studies) that staunch skeptics would have to contend with (for a good overview of them, see Mossbridge & Radin, 2018). Even moreover, the experimental evidence for psychic phenomena in general is even larger (Cardena, 2018; Tressoldi & Storm, 2021, 2024) and would have to be further addressed by staunch skeptics.

Incidentally, Jarry (2024) also seems to make an error in his relative comparison of the Kekecs et al. (2023) study to Bem’s (2011): Jarry correctly mentions that Bem’s study had 863 participants, but then goes on to claim that Kekecs et al. had replicated Bem’s study “using 20 times more participants.”

This would not be accurate: Twenty times more would amount to around 17,260 participants, which is not how many participated in Kekecs et al.'s study; it was actually 2,220 participants (Kekecs et al., 2023, p. 14).

Jarry then mentions the studies by Rupert Sheldrake (1999a) of ostensible psychic ability in pets and other domesticated animals, noting that:

These findings were even reproduced by skeptics, says Dickens. I don't know which skeptics she's referring to because world-famous skeptic Dr. Richard Wiseman and colleagues famously did replicate Sheldrake's experiments but failed to show any telepathic ability in pets.

Jarry apparently does not realize that Ky Dickens actually is referring to those very same replications conducted by Wiseman, Smith, & Milton (1998). At first glance, it would appear that their results are not in line with Sheldrake's. But upon re-examining the replications himself, Sheldrake (1999b) noticed that their test design had been based on an exaggerated description given in a media report, and that they had not taken all of the data collected over the entire test period into account. When he then re-analyzed their data across that full time period, Sheldrake (1999b) found that the replication findings of Wiseman and his colleagues were actually in line with his own. So in that sense, his findings were reproduced by skeptics.

Jarry closes the second half of his article by stating that "...real answers on the topic of psi phenomena will not be arrived at through wishful thinking. Rigour should be de rigueur." Indeed, but Jarry seems to not be aware that such rigour has actually been in practice within parapsychology for a while now. His statements seem to reflect a glaring lack of familiarity with the parapsychological research literature, which happens to be line with a claim recently made by Harvard University science historian Naomi Oreskes (2024) that "...scientists are specialists, and their training rarely prepares them to evaluate claims beyond their particular areas of focus," and that seems to especially be the case when it comes to parapsychological claims. And so she suggested: "In matters of scientific debate, look to the relevant experts, not the armchair ones." And in evaluating those claims, second author B.J.W. further suggested in a response to her (Williams, 2024) that one should ideally "...strive to be as objective as one can be in making their own assessment by weighing both sides of the issue [proponent and skeptic] as equally as possible." That is especially important to keep in mind when considering Jarry's (2024) arguments.

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