

# Parapsychology in Psychology Textbooks

This article describes how psychology textbooks fail even to mention the closely-related topic of parapsychology, or else persistently misinform their readers about it. The effect is that generations of psychology students are being indoctrinated with unwarranted negative perceptions.

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## Dogma in Science

A survey of psychology textbooks reveals that where the topic of parapsychology is not completely neglected it tends to be misrepresented, giving the impression that its findings are unsound. The writers of such books tend to argue that the case for parapsychology is a matter of personal belief, and that 'psychic' effects disappear under more rigorous testing. Effects that cannot be dismissed as fraud, a common explanation, are accounted for in terms of errors and expectancy.

This tends to confirm philosopher Thomas Kuhn's argument that the credibility of a scientific domain is governed not by an idea of absolute truth, but by 'gatekeepers': journal editors, grant committee members and textbook writers. In Kuhn's view, scientific education has less to do with encouraging independent thinking and innovative research than with imposing a set of agreed practices and ideas. This is a process of socialization: trainee scientists are instructed about what questions they're allowed to ask and the techniques they may use to answer them.

Such 'dogma', as Kuhn calls it, does not eliminate bias and generate a truly objective science, but rather fixes preconceptions. The aim is to draw a clear distinction between legitimate and illegitimate, sense and nonsense. This can be an effective way to marshal resources in order to solve intractable problems. But it comes at a cost: the rejection of unexpected outcomes that threaten the consensus worldview.

If Kuhn is right, it helps to explain the antipathy demonstrated by sceptics of parapsychology, as demonstrated by the writers of psychology textbooks. Anomalies that challenge the status quo are rejected as trivial in the absence of a theoretical framework, or as artefacts created by some, as yet unidentified, methodological flaw.

A powerful means of imposing scientific dogma is through textbooks that do not passively describe a discipline but actively circumscribe it. By selecting topics and representing them in a particular way, authors determine the boundaries of

legitimate concern and appropriate practice. These are then policed and transmitted from generation to generation.

## Parapsychology in Psychology Textbooks

How does parapsychology fare in this social enterprise? I surveyed the psychology introductory textbooks in the University of Northampton library to see what our undergraduates could learn about parapsychology from sources that would be recommended to them as 'reliable' (unlike, say, Wikipedia). I focused on textbooks published within the last six years, which number eight in total.

Four make no reference to parapsychology at all.<sup>1</sup> This continues the trend described by McClenon et al.<sup>2</sup> who found that 37% of their 1980s sample omitted the subject altogether, compared with 46% of the 1990s sample, and 42% of the 2002 sample. Surprisingly, this group also includes the latest (16th) edition of *Atkinson and Hilgard's Introduction to Psychology*.<sup>3</sup> A co-author in a number of previous editions was Daryl Bem, a Cornell University psychologist and parapsychologist, and the text benefited from perhaps the most extensive and balanced overview of the field. However, following recent revisions, key terms such as 'parapsychology', 'psi' and 'ESP' are now missing from the index. Papers describing the 'ganzfeld' free response ESP paradigm still feature in the references list, but there is no reference to them in the text.

### Passer and Smith

One textbook that covers parapsychology is by Passer and Smith.<sup>4</sup> This contains a section on paranormal beliefs that includes coverage of ganzfeld ESP research.<sup>5</sup> It begins by focusing on levels of belief in ESP and goes on to ask: 'Does research convincingly demonstrate that ESP occurs? While many parapsychologists say "absolutely", many scientists and other skeptics say "absolutely not."' <sup>6</sup> This sentence construction subtly positions parapsychologists as being different from scientists and unskeptical when considering these claims.

They continue, 'when tested under controlled conditions in well-designed experiments and replications, claim after claim of psychic ability has evaporated', an argument they justify on the grounds that CSICOP (now the Committee for Skeptical Inquiry, CSI) has not judged any claims to be valid and that stage magician James Randi's challenge remains unclaimed. This reads very like standard rhetoric from the CSI, particularly in that the authors seem to feel no obligation to explain who conducts such replications or in what way are they methodologically superior. This is just as well, given that critics of parapsychology seldom bother to conduct formal replication attempts. In contrast, a number of substantive reviews have not shown any tendency for effects to disappear as methodological controls are tightened.<sup>7</sup>

They conclude 'while the burden of proof lies with those who believe in the paranormal, evaluations of their claims should be based on scientific evidence rather than on preconceived positive or negative expectations'.<sup>8</sup>

### Holt et al

Another psychology textbook is titled *Psychology: The Science of Mind and Behaviour*, by Nigel Holt and five co-authors (referred to here as Holt et al.).<sup>9</sup> This contains a chapter on studying behaviour scientifically, in which a subsection – headed ‘threats to the validity of research’ – covers ‘science, psychics and the paranormal’<sup>10</sup>, presuming the issue instead of considering it neutrally.

The section begins usefully by defining psi phenomena in terms of [CD Broad’s](#) basic limiting features.<sup>11</sup> It also justifies a psychological interest as a response to widespread belief. But it goes on quickly to assert that, ‘when tested under controlled conditions in well-designed experiments and replications, claims of psychic ability have failed to materialize’.<sup>12</sup> This formula closely echoes Passer and Smith, who are among the book’s co-authors.

When discussing ganzfeld psi research, sceptical authors tend to refer to a single meta-analytic review that found no effect,<sup>13</sup> while avoiding mention of several others that found a consistent above-chance effect. That is not the case here, as two others are mentioned,<sup>14</sup> ostensibly leaving the case unproven. However, the authors’ presumption of falsehood is laid bare when they note that, ‘claiming psychic powers is no worse really than claiming any ability you do not have. It only becomes a problem when vulnerable people are being taken advantage of’.<sup>15</sup>

### **Cacioppo and Freberg**

Cacioppo and Freberg’s book is titled *Discovering Psychology: The Science of the Mind*. It contains a short section headed ‘do you believe in ESP?’<sup>16</sup> This gives an overview of the types of phenomena associated with parapsychology, in which the authors note public poll results, but report that ‘ninety-six percent of the scientists who are members of the National Academy of Sciences do not believe in ESP’.<sup>17</sup> No reference is given, but the source is presumably a 1991 survey by McConnell & Clark published in a parapsychology journal,<sup>18</sup> in which 5% reported belief that ESP/PK occurs and a further 19% had no opinion to offer. The statement is therefore false: the true proportion of scientists declaring disbelief in this survey is actually 76%. Oddly, in this survey those declaring belief were expected to refer to empirical evidence while those declaring disbelief were not, so that negative views could literally reflect prejudice rather than any familiarity with the scientific evidence.

Cacioppo and Freberg use Daryl Bem’s suite of experiments published in the *Journal of Personality and Social Psychology* as their exemplar of parapsychological methods.<sup>19</sup> They focus on an experiment that involves selecting one of two curtains to be opened to reveal either a blank wall or a picture, which may or may not be erotic in order to create arousal. The commentary notes a protocol change that was made in mid-study and comments, ‘it is quite unusual for researchers to change their methods in the middle of an experiment and more troubling when there doesn’t seem to be a good reason to do so. ... The change to the protocol (switching from having 12 erotic pictures, 12 negative and 12 neutral to 18 erotic and 18 non-erotic positive pictures) seems relatively minor, but is not justified in Bem’s paper.’<sup>20</sup>

Evaluating the statistically significant outcome, the authors further note a complaint voiced by CSI sceptic [James Alcock](#) in the *Skeptical Inquirer* that Bem was ‘capitalizing on chance by performing multiple analyses’.<sup>21</sup> But they refer vaguely to the ‘questionable use of statistical analysis’, and fail to mention that Bem made a robust response in the same magazine, pointing out that the complaint is invalid, since ‘it does not apply to any of the analyses in my article’. Bem goes on to say, ‘Alcock has memorized the right words about multiple tests, but does not appear to understand the logic behind those words’.

In a thinly-veiled allegation, Cacioppo and Freberg note that ‘replication provides an important check on possible researcher bias, and failure to replicate indicates serious flaws in an experiment. So far, the three known replications of Bem’s experiments have failed to produce significant results’.<sup>22</sup> The implication is that claimed effects in the original study are thus spurious, possibly attributable to the methodological problems they’ve identified. But this is an odd assertion given that presumably Cacioppo and Freberg are referring to the Ritchie, Wiseman and French<sup>23</sup> attempt that received a lot of coverage – it was the subject of a special issue of *The Psychologist* in May 2012 devoted to replication issues<sup>24</sup> –but is concerned with facilitation of recall and so has nothing to do with the study being critiqued (and rather looks like an unplanned change in the scope of the review midway through -- the very thing they object to in Bem's study!). In fact, according to an updated meta-analysis by Daryl Bem, Patrizio Tressoldi, Thomas Rabeyron, and Michael Duggan<sup>25</sup> there have been 14 studies that looked to replicate what they now call the precognitive detection of reinforcement, and these have given a highly significant cumulative effect ( $z = 4.22, p = 1.2 \times 10^{-5}$ ).

## **Richard Gross**

The most extensive coverage is by Richard Gross, titled *Psychology: The Science of Mind and Behaviour* (2010), which devotes a whole chapter to parapsychology.<sup>26</sup> It begins with an account of its historical origins in psychical research, through field investigations to experimental approaches beginning with Rhinean card guessing. However, it characterizes the early investigators as ‘gullible, incompetent, or both’ – hardly an appropriate characterization of early pioneers who included numerous Fellows of the Royal Society and Nobel laureates.

In accounting for the success of card guessing studies, Gross attributes them to methodological weaknesses in earlier studies that allowed for sensory leakage or recording errors, and when these were controlled for in later studies the effects were much reduced – arguably a simplistic and distorted picture of the history of forced choice ESP studies (compared with, say, that presented by Mauskopf & McVaugh).<sup>27</sup>

Worse, in accounting for the residual effect, Gross focuses on the tendency for sceptics to fail to replicate effects and segues awkwardly into the issue of fraud, pointing out that ‘[t]he director of research at the Duke University Laboratory... was later caught flagrantly modifying some experimental data in a pro-ESP direction’.<sup>28</sup> But this episode has nothing to do with the ESP studies that Gross is attempting to account for.

Gross goes on to say ‘this wasn’t an isolated example’<sup>29</sup> and quotes Coleman as describing the history of parapsychology as ‘disfigured by numerous cases of fraud involving some of the most highly respected scientists, their colleagues and participants’.<sup>30</sup> In fact only one further case is explicitly referred to, that of SG Soal, which still remains contentious. Gross returns to this when he claims that ‘accusations of fraud – the deliberate invention or modification of procedures or results – have been a feature of the history of parapsychological research in general. Arguably, this makes the study of psi unique as an area of psychological enquiry’.<sup>31</sup> This is untrue and unfair, particularly in an era that has seen severely damaging cases of fraud in mainstream psychology. (See [Fraud in Science and Parapsychology](#))

Gross goes on to introduce free response ESP and links it to the Ganzfeld technique, giving a fair review of its history but only up to 1994. The section ends, ‘despite many parapsychologists believing that the Ganzfeld is a genuinely repeatable experiment, most other scientists seem to reject the evidence,’<sup>32</sup> again representing the outcome as a matter of belief rather than sound evidence.

Parapsychology is further discussed as a means to introduce general issues around replication and file drawer effects. Gross acknowledges the Parapsychology Association’s policy of publishing non-significant results, and goes on to stress the need for replications to be conducted by those who are unsympathetic to psi. But he fails to mention that sceptics are generally reluctant to conduct empirical research that directly tests the psi hypothesis. He also overlooks the well-documented ‘experimenter effect’,<sup>33</sup> acknowledged in social cognition research, in which extremely subtle manipulations of the social-psychological environment have been shown to have large effects upon participant behaviour.

The usual method for preventing expectancy effects from being communicated to participants, and in the process affecting their behaviour, is to use single and double blind methodologies. According to Sheldrake’s 1998 survey of published papers, experimenter blinding is virtually unknown in the physical sciences (0.8%), is still rare in psychology and animal behaviour (4.9%) but is quite typical of parapsychology research (85.2%), suggesting it is much less susceptible to these effects, quite the reverse of the picture he paints.<sup>34</sup>

## Conclusion

It hardly needs to be stated that this kind of misinformation will deter undergraduates from taking a serious interest in parapsychology. Equally obvious is that parapsychology organizations must take educators to task when they fail to provide accurate and balanced information. They have an obligation to the next generation of researchers to ensure that their peers are better informed about the case for parapsychology than their predecessors.

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## Endnotes

### Footnotes

- [1](#). Eysenck (2009); Gerrig et al. (2012); Kantowitz et al. (2015).
- [2](#). McClenon et al. (2004).
- [3](#). Nolen-Hoeksema et al. (2014).
- [4](#). Passer & Smith (2011).
- [5](#). Passer & Smith (2011), 55-56.
- [6](#). Passer & Smith (2011), 56.
- [7](#). Cardena, Palmer, & Marcusson-Clavertz (2015); Cardena, Lynn & Krippner (2015).
- [8](#). Passer & Smith (2011), 56.
- [9](#). Holt et al. (2012).
- [10](#). Holt et al. (2012), 59-60.
- [11](#). Broad (1953).
- [12](#). Holt et al. (2012), 60.
- [13](#). Milton & Wiseman (1999).
- [14](#). Schmeidler & Edge (1999); Palmer (2003).
- [15](#). Holt et al. (2012), 60.
- [16](#). Cacioppo & Freberg (2013), 76-77.
- [17](#). Cacioppo & Freberg (2013), 76.

- [18.](#) McConnell & Clark (1991).
- [19.](#) Bem (2011).
- [20.](#) Cacioppo & Freberg (2013), 77).
- [21.](#) Alcock (2011).
- [22.](#) Cacioppo & Freberg (2013), 76.
- [23.](#) Ritchie, Wiseman and French (2012a).
- [24.](#) Ritchie et al. (2012b).
- [25.](#) Bem et al. (2016).
- [26.](#) Gross (2010), 82-94.
- [27.](#) Mauskopf & McVaugh (1980).
- [28.](#) Gross (2010), 85.
- [29.](#) Gross (2010), 85.
- [30.](#) Coleman (1987).
- [31.](#) Gross (2010), 88.
- [32.](#) Gross (2010), 88.
- [33.](#) Rosenthal (1966).
- [34.](#) Sheldrake (1998).