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### Why we publish papers reporting findings we may not believe

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## Editorial

### Why we publish papers reporting findings we may not believe.

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This volume of *Cortex* includes a paper (Freedman et al.) that reported evidence that an aspect of Psi may be possible under specific experimental conditions. The authors hypothesized that Psi could be inhibited by frontal lobe processes and used rTMS to reduce cortical excitability in the frontal lobes following the logic that such reduction would improve Psi capabilities. There are many studies that have used this rationale to investigate cognitive control processes. Despite the apparently supernatural theory articulated by Freedman et al, the experiment seemed to have been carefully conducted, and was well reported, so it was sent for external review. After two rounds of revision, the reviewers and the action editor were reasonably satisfied with the changes made, including several caveats to the conclusions of the study, and eventually the paper was approved for publication.

Publishing Psi research is controversial as the theoretical framework challenges the conventional view of the limits of cognitive processes in humans. Of course, the cognitive neuroscience literature is rich in surprising phenomena; for example, the observations that patients with selective brain damage may outperform controls on certain tasks. Yet, the conclusions one derives from this paper are very close to impossible without contradicting accrued scientific wisdom as the hypothesis requires mechanisms outside of the conventional physical models regarding time.

There is therefore a tension between assessing scientific integrity only by the quality of the methods, or additionally requiring hypotheses to fall within conventional parameters. This issue was one that was grappled with in the decision process that led to publication. Accordingly, the dilemma is how to resolve the tension between the technical quality of the research (which could be acceptable) and the a priori plausibility of the hypothesis (which could be low). The same points were raised in the discussion that ensued following the online publication of the paper: Given the topic, should it be published? Is it methodologically sound enough to deserve publication in *Cortex*? We will share our reasoning which informed our decision.

We strongly maintain that editors (and reviewers) should not interfere with authors' freedom and should carefully refrain from imposing their own biases and views to determine what should or should not be published. They are gatekeepers for quality control. The over-riding principle is that editors and reviewers should not interfere with the scientific freedom of researchers to study (within ethical boundaries) whatever topic they and their funders deem worthy of investigation. If we allow our own theoretical preferences to circumscribe the topics to consider, there is a danger that science becomes stagnant, unable to move beyond dominant paradigms, and be dominated by dominant schools and individuals. Psychic abilities may be at an extreme end of the plausibility spectrum, but the action editor did not consider it his role to outlaw the topic, and it was subject to the usual standards of scrutiny.

Like any study, the paper itself has limitations, chief among which are the weakness of some discussion statements (e.g., the evolutionary argument), post hoc weighting (arousing the worry of p-hacking and its implications), the lack of replication, and not being pre-registered, which are noted explicitly as caveats in the paper itself. The main finding is not presented as an outright claim, but rather as a provisional finding in need of replication, ideally via a preregistered route.

Even so, the publication of research of this topic has caused legitimate skepticism amongst scientists; we have invited everyone who raised them to write a commentary. The commentaries of those who have agreed accompany the paper for a fuller appreciation of its implications.

While scientists may have doubts about the existence of Psi-like phenomena, the lay public does

not hold the same degree of skepticism. So, it may be useful for us to engage in a spirited and scientifically grounded debate about the reality of Psi in this context.

We considered whether we should not be applying the same editorial standards to all articles. Should we not increase the rigour of our science reporting, independently of the topic under investigation, be it Psi or some form of TMS treatment? Or should we be extra-careful solely when we think that the likelihood of positive findings is rather low due to our prior beliefs? Are our standards for evidence not too low in respect to what they should ideally be? We would be interested to receive informal comments submitted to the journal office, or even more formal submissions for *Cortex* Discussion forum, addressing these questions.

Of course, one isolated finding cannot establish any new general truth, and there is always the possibility of a false positive finding or misinterpretation of the results even in the most carefully conducted experiment. The bottom line is that our current knowledge is always provisional and open to revision, and any isolated finding needs to be demonstrated to be replicable before we can build confidently upon it. In the case of the paper by Freedman et al, given the explicitness of their methods and research design, it should not be difficult to try and replicate their findings, testing the generalizability and validity of their results. We welcome that educational effort.

*Jordan Grafman and Sergio Della Sala*  
*Editors of Cortex*