



“Censorship”, early childhood research quarterly and qualitative research: Not so much aced out as an own goal?



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ABSTRACT

As its starting point, this article investigates claims published in *Qualitative Inquiry* by Ceglowski, Bacigalupa, and Peck (2011) that *Early Childhood Research Quarterly* censored qualitative research. Unfortunately they assert rather than demonstrate political bias against qualitative research, fail to show that its publication in *Early Childhood Research Quarterly* has actually declined and ignore alternate hypotheses compatible with their data. After breaking their argument into parts, I find their censorship claims completely unsupported by evidence. However, this article has two larger aims. The first is to show how mistaking hypotheses for evidence, arguing unconvincingly from quantitative data, and failing to consider alternative interpretations of evidence weaken qualitative research, lowering its credibility within social science. The second is to consider the wider academic ramifications of publishing a peer-reviewed journal article that totally fails to support its claims. Based on these concerns, the article offers some practical advice to avoid the negative outcomes demonstrated by the publication of Ceglowski, Bacigalupa, and Peck and considers the scientific implications of this rebuttal to their claims having been rejected previously by *Qualitative Inquiry*.

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In an article in *Qualitative Inquiry* (hereafter QI), Ceglowski, Bacigalupa, and Peck (2011, hereafter CBP) refer to three layers of censorship operating against qualitative research using, as an example, their experience of having a manuscript (hereafter ACS for “Allegedly Censored Submission”) rejected by *Early Childhood Research Quarterly* (hereafter ECRQ). CBP report that ACS was subsequently published elsewhere but it is not possible to establish from CBP (or the web) where it was published (or under what title and authorship) so it cannot be cited directly. The use of all these acronyms is unfortunate but reflects the complexity of the example. In particular, it is necessary to avoid confusion when referring to the two distinct texts CBP and ACS.

In one sentence, the first element of my argument is that CBP do not provide any evidence that backs up the claim that ACS was censored by ECRQ. However, it is important (because I agree with CBP that qualitative research can be valuable and needs to be taken seriously) to consider three distinct ways in which they fail to do this (and what their wider implications are). The fact that CBP was considered suitable for publication in a peer-reviewed journal despite complete failure to support its claims constitutes *prima facie* evidence that the failings of this article may be symptomatic of more

general weaknesses in qualitative research and its peer review process. To make this point as inescapably as possible, the arguments I use to undermine CBP’s claims exclude value judgements about how social science should be done and mere appeals to authority by citation. This article shows the weaknesses of CBP directly from what they write (or fail to write) using only critical analysis.

CBP’s first failure is confusing hypotheses and evidence. This occurs in the first two sections of their article where the authors merely assert that qualitative research is not properly understood, that advocacy of randomised control trials (hereafter RCT) in educational research is politically (rather than scientifically) motivated and that presenting RCT as a research gold standard is merely a rhetorical ploy to privilege quantitative methods. The second failure is unconvincing use of quantitative methods to support their arguments about censorship. This occurs in the fourth section of CBP where the authors attempt to show that qualitative publication in ECRQ is declining as a supposed consequence of censorship. In fact, even the claim of declining publication is not supported as CBP present it (being sensitive to unjustified assumptions made in their analysis) and the alleged cause of this decline (censorship) is not demonstrated at all. The third failure is (ironically given CBP’s stress on interpretation) not identifying (and adjudicating between) plausible alternative hypotheses compatible with the same data. This occurs in Section 5 of CBP where a discussion of reviewer comments about ACS is presented as if it clearly shows evidence of

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misunderstanding (and perhaps partiality) when it can be interpreted equally plausibly to show normal reviewing practice, recognised differences in perspective about what constitutes good qualitative research, and concern for adequate rigour compatible with the practices of ECRQ.

The second (and potentially far more important) element of my argument is to consider the significance of the fact that an article entirely failing to support its claims has appeared in a peer-reviewed journal. What wider implication does this have for academic quality and what can be done about it? This issue is considered both in terms of the logic of peer review (and its inevitable fallibility) and through a reflexive analysis of my attempts to get the present article published and their implications.

The article is organised as follows. The following section outlines the structure of the CBP article as background to detailed critique. The next three sections analyse in depth the three major failings identified in CBP: Mistaking hypotheses for evidence, arguing unconvincingly from quantitative data and failing to identify and adjudicate between different interpretations of the same evidence. The following section explains why CBP being published in a peer-reviewed journal creates a wider problem for the scientific process and why detailed critical rebuttals like the present article may thus be necessary. The next section considers suggestions for reducing the publication of qualitative research involving these failings in the context of a wider awareness of the scientific process as a whole. The final section examines the review process for the original version of this article sent to QJ, its implications for the maintenance of qualitative research quality and the wider ramifications for scientific progress.

The structure of CBP

CBP is organised into seven sections mapping reasonably straightforwardly onto the critique presented here. The first section (CBP, pp. 679–680) discusses the Reading Excellence Act (hereafter REA), asserting political motives for advocating RCT in education research and questioning whether critics and reviewers properly understand qualitative research. The second section (CBP, pp. 680–681) criticises the concept of “gold standards” in education research as an inappropriate rhetorical device that privileges quantitative research. The third section (CBP, pp. 681–682) discusses the scholarly significance of ECRQ as a leading journal in its field and presents data supposedly showing a decline in its publication of qualitative research after the REA. The fourth section (CBP, pp. 682–684) reports having ACS reviewed and rejected by ECRQ. The fifth section (CBP, p. 684) offers advice on successful submission of qualitative research. The sixth section (CBP, p. 685) reiterates earlier claims and the seventh section (CBP, p. 685) concludes. The critique in this article focuses mainly on sections two to five (the substantive parts of CBP) with sections two and three confusing hypotheses and evidence, section four using quantitative methods unconvincingly, and section five failing to identify and consider the significance of alternative interpretations of data. The next three sections of this article more thoroughly analyse the evidence for each failing in turn leading to the overall conclusion that no argument for the CBP claim of censorship withstands analysis.

Hypotheses are not evidence

The first interesting thing about CBP is that the word censorship does not occur in the text anywhere but only in the title, abstract, and keywords. Censorship, one presumes, means rejecting research whose quality would normally justify publication for reasons other than quality. For example, CBP allege that ACS was rejected by ECRQ because it was qualitative, period. Clearly, non-publication results

from at least two other common causes. (Although this article will not explore the point further, the three failures of CBP overlap repeatedly. Here, as later in their article, they attend to only one possible explanation without evidential justification.) The first is that a submission *does not* have the relevant quality required for the particular journal selected. The second is fallible peer reviewing. This involves mistakenly publishing articles the journal should not (given its quality and the quality of the submission) and rejecting acceptable ones. On reflection, such errors would hardly be surprising and can be observed empirically (Peters & Ceci, 1982). CBP engage awkwardly with these possibilities. CBP imply that ACS was in fact good enough to be published by ECRQ (with the further implication that censorship is the reason ECRQ *didn't* publish it) because it was subsequently published (and in fact won a prize). However, it was published by a *different* journal about whose standards CBP tell us nothing. In fact, it does not seem possible to infer what that journal was from CBP and further web research also provides no likely citation. (As of 24 June 2013, Peck did not seem to have a homepage at all. Ceglowski had no CV or publications listed on hers and the online CV provided by Bacigalupa on her homepage listed neither an article by these three authors nor one that received a prize. The name of the prize and the university that awarded it were also not sufficient to identify the relevant article on the web. Dated PDF files for these searches are available from the author on request.) In these circumstances, the transparent thing for CBP to do would have been not only to make ACS available but also to cite its subsequently published incarnation. (After all, another possibility CBP don't discuss is that only improvements suggested by ECRQ reviewers rendered ACS publishable after all.)

Given the fallibility of reviewing, it is perfectly possible that the rejection by ECRQ correctly reflected the quality of ACS and that it was the acceptance by the *other* journal that was mistaken! In these circumstances, far from subsequent publication giving grounds for thinking there was anything wrong with the outcome of the ECRQ review process on ACS, the article could turn out to be poor even by the standards of the journal that did publish it! CBP cannot argue that an outcome is correct merely because it happens to suit them and problematic because it does not. In fact, of course, journals differ considerably in the proportion of submitted articles they reject and the average number of times the articles they do publish are cited. While caution must be used in interpreting these simplistically as measures of a quality – articles may be extensively cited for other reasons with the role of Mohammed El Naschie in the dramatic league table rise of the University of Alexandria as an interesting case (<http://www.nytimes.com/2010/11/15/education/15iht-eduLede15.html>) – they should broadly tell us something about the ranking of journals. A quantitative researcher might point out that a sample larger than one case is necessary to draw any conclusions about reviewing outcomes when reviewers are fallible. In fact, systematic empirical research on peer review effectiveness exists – see Cole, Cole, and Simon (1981) and Weller (2002) for overviews – but CBP do not cite it.

The correlation between quality and receiving prizes (the other part of the CBP justification for questioning the rejection of ACS by ECRQ) is not discussed and to my knowledge has not been researched. CBP only imply that a prize-winning article must be good though it does not follow self-evidently that it is good *enough* for ECRQ. As CBP point out on page 681, ECRQ, at the time, had an impact factor of 1.39 placing it 16th highest in its category according to Thompson Reuters and making it a “top-tier journal”. By contrast, the prize – which web search cannot conclusively identify from CBP's description – appears to be a single university award. We thus have no way of telling how many individuals (and at what level of experience) were actually eligible for it and thus how strong the competition is in any particular year. It seems perfectly possible that even a prize-winning article at the university level might

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