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A dialogue between scientific health editors and scientists responsible for qualitative studies

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Taquette and Villela's article seems to me – a science researcher, reviewer and editor – to be an opportunity to promote further dialogue with editors regarding their implicit or explicit editorial policies, as laid out in the "Instructions to Authors". Editorial policies should indeed be intermittently reviewed, since it is on them that scientific dialogue is based.

The authors classify scientific journals as favorable, unfavorable or indifferent/ dubious, basically drawing on four criteria: 1) statistical testing requirements; 2) reproducibility; 3) generalization of results, and 4) highly limited length of the text. I was unable to identify the effective use of other criteria such as number of authors or scope, despite the fact that they are mentioned. The authors conclude that more than half of the journals, and medical journals in particular, set out conditions that are incompatible with the objective of disseminating qualitative research.

The requirement for statistical tests as a formal criterion for scientific validation undoubtedly excludes the possibility of scientific debate that draws on qualitative research for any given journal. I support Taquette and Villela's view that journals that make such a requirement deny their readers of research that may be valid and subsequently useful for clinical care. Even in quantitative research, Hill's[1] warning rings true: [...] *Yet too often I suspect we waste a deal of time, we grasp the shadow and lose the substance, we weaken our capacity to interpret data and to take reasonable decisions, whatever the value of P. And far too often we deduce 'no difference' from 'no significant difference'. Like fire, the* χ^2 *test is an excellent servant and a bad master*.

I have serious doubts as to whether the other three requirements (reproducibility, generalization and length) can necessarily be considered unfavorable criteria. My doubts are based on the fact that these three conditions are requirements for validity that are not unique to research that quantifies or qualifies human experiences and experiments.

We should remember that all discourse, whether scientific or not, should meet with validity requirements. Scientific discourses, be they quantitative or qualitative, should be aware of these requirements. As Ayres[2] explained, *Against a belief in a universal truth, that is absolute in its very first positivism and in its relative neopositivisms, Habermas makes a counter argument which is thus a consensual view of truth. The validity of objective knowledge rests on intersubjectivity upon which all rational constructs are founded. The pretention to truth of any discourse is therefore related to its 'value' among different discourses through an interaction at three levels* [...] *a) in its ability to express certainties that are shareable, i.e. in its propositional positivity; b) in the possibility of providing guidance on effective action relating to social projects relating to such realities, ie in their normative rightness; and c) in the demand to establish effective intersubjectivity between the different subjects involved in these actions, i.e. in their authenticity*. Science undoubtedly values the first level, and scientists from different fields seek to draw conclusions in a way that they might be accepted if not by consensus, then at least by the majority.

Reproducibility is often mistakenly understood as a fundamental requirement for validity of a particular scientific assertion. Researchers who stand by this misconception – as some editors or reviewers – are possibly those who have a conception of science that has already been dismissed by so many philosophers of science, such as Popper, Bachelard, Kuhn, Merton, Fleck, Latour and others. They would seem to be stuck in notions of empiricism that are from the 19th Century[3], also known as positivism. In the best of hypotheses, they abide by a logic of verificationism that emerged from the Vienna Circle[3]. Editors that act in this way are probably scientists with little training in philosophy or in the history of science and who cultivate epistemological beliefs without much critical reflection. An assertion should clearly not be considered as true by the scientific community simply because it was reproduced, nor should the opposite be true. The reproducibility of a study has more to do with methodological transparency and, thus, can and should be valued in any research modality. Readers of the sciences, whether they are scientists or not, have the right to the greatest level of detail possible in order that they might reproduce either mentally or materially a given study. It is my belief that reproducibility, when understood in this way, can serve as an important tool for expanding scientific knowledge. The authors compare the notion of replicability with that of reproducibility, but I believe that

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using a new term does not completely resolve the issue. Other authors have used alternative terms such as trustworthiness, quality or rigour[4]. These terms end up falling back into what Habermas calls the capacity to express comparable certainties. Without this capacity, any scientific study risks being forgotten.

With regard to generalization, again I understand that editors generally possess an ingenuous (or outdated) conception of science. A generalization is often understood as a theoretical conclusion that is atemporal and unhistorical. To apply the concept of singularity as a counterpart to that of generalization seems to create more problems than it solves. To take a radical position on singularity could lead to beliefs that conclusions serve solely for that particular data that was studies or for a given population. The belief in singularity is a choice, conscious or otherwise, of reducing the empirical reach of a scientific conclusion[5]. This is definitely not recommended. It is better to think of the conclusions of qualitative research as the creation of theoretical interpretations based on meanings that draw on human experiences and experiments. These interpretations can be extrapolated or generalized to other circumstances, but this must be done with care and with theoretical and methodological rigor. Otherwise, how else could the works of Freud, Geertz, Bourdieu, Foucault, Paulo Freire and so many others be valued?

Finally, the length of the text. This seems to me to be a criterion that is related more to the writing style than to epistemological differences between quantitative and qualitative research. I thus think it is not very valid to use it to classify an editorial policy as either favorable or unfavorable. Concision – where it does not affect clarity, density or coherence of a scientific text – should be exercised by all scientists. From my perspective as an editor and teacher of science writing, I would identify three recurring types of excessive text: long and unfocussed introductions, a large number of objectives and presentation of results in a way that lacks creativity. Introductions frequently fail to clearly formulate the research problem and to defend the importance of the research objective. This results in panoramic texts that seek to summarize the 'state of the art'. It is not uncommon to encounter more than two objectives, presented through the use of two or more verbs. More objectives mean more methods, more results and, eventually, more theoretical conclusions. Presenting results in a way that lacks creativity (or is even erroneous) often involves the transcription of quotations – the raw material of the research – without the necessary empirical and analytical categorization that draws on the theoretical framework[6]. It is important to aim for a more concise narrative that benefits from using sections with empirical and analytical categories and texts from the author that summarize the subjects' accounts and link the empirical with the theoretical. It is also important to avoid jargon, the use of the passive voice and of nouns instead of describing processes with explicit subjects and actions[7]. All these issues can lead to an unnecessarily long article. Editors, reviewers and above all readers are grateful for texts that are more concise and dense in their articulation of theory.

It is worth recalling George Orwell's[8] suggestions as to how to avoid "decadent language" (in his own terms), whether scientific or not:

(1) Never use a metaphor, simile or other figure of speech which you are used to seeing in print; (2) Never use a long word where a short one will do; (3) If it is possible to cut a word out, always cut it out; (4) Never use the passive where you can use the active; (5) Never use a foreign phrase, a scientific word or a jargon word if you can think of an everyday English equivalent; (6) Break any of these rules sooner than say anything barbarous.

It is, then, up to all of us, as editors and writers of science, to improve how we embrace the contributions that qualitative studies have to make.

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