

Review of: "Social responsibility, disciplinary moral identity, and not-so-value-free biomedical research(ers)"

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The paper addresses an important topic with some insight, but it is not fully developed and needs some more work before it is suitable for publication.

Indeed, rather than “a theoretical and speculative article” (page 13), at this stage the paper reads as a (good) research proposal. Expressions such as: “It may be ...”; “it is possible...”, “it seems ...”; “there is the possibility...”, are recurrent throughout the text and the author doesn’t take a position for one over other possible developments/courses of action, either on the basis of empirical data or theoretical speculation. In contrast to that, there are some strong assumptions, which would need to be further justified: in particular the uniqueness of biomedical research (and perhaps those who practice it). Whilst one can easily agree that this research it is not value-free (page 5), its pursuit of socially desirable ends - defined by whom? how? - cannot go unquestioned. Recent and less recent scandals, including fraud, misconduct and other may be mentioned, even just in passing.

The call for reflexivity and openness is appropriate, whereas the distinction between regulation ‘from the outside’ and encouragement ‘on the inside’ (page 3) is not clear. Codes of conduct and similar, which are already in place, definitely belong to the former type and indeed may not be sufficient. But how is the latter type conceived? How does it differ, e.g., from Polany’s description of the Republic of Science?

The hypothesis that *members of different communities may share a more or less strong disciplinary moral identity*(page 2) – which is central to the paper - is certainly worth exploring, yet the idea of *disciplinary moral identity* is poorly defined and the related discussion inconclusive. Many components are mentioned, from personal inclinations to the *cursus studiorum*, etc., without a clear statement (or hypothesis at least) on how they and their final combination might be assessed. I take at face value the author’s statement that “there are not many empirical studies” (page 11), but the available ones deserve more space for supporting his work. The one study mentioned refers to “researchers who took special ethical training” (page 11). It remains unclear to me whether such type of special training is considered as constitutive of a moral identity over and above all previous disciplinary and professional experiences.

To address these weaknesses, I recommend: a) a clearer statement of the purpose and scope of the paper; b) a reorganization of the text (e.g. anticipation the key distinction between *prescriptive* and *descriptive*, which now is introduced only on page 7); c) a deeper exploration of the literature to find and analyze relevant sources (in particular empirical studies); d) an accurate definition of the terms used.

With regard to d), my strongest criticism regards the current use of the term “co-production”, without a clarification of the meaning attributed to it. Although the author makes reference to Jassanoff’s work (2004), his own understanding is clearly not the same as hers. Neither is the same as Olstrom’s, who originally coined the term <https://doi.org/10.1111/j.1541-0072.1981.tb01208.x>. Precisely because the term/concept is used with diverse meanings by different scholars (and not only the two previously mentioned), the author must make it explicit the one he adopts and stick to it consistently throughout the article. Otherwise, no little confusion may arise, especially in some passages. E.g. *the sense of social responsibility promoted by the co-production ideal requires the integration of social and value diversity in research (page 4); the co-production ideal was developed as a reaction against the Value-Free Ideal” (page 6)*. And in particular in the following passage, where the author makes reference to some scholars who never used the term in the sense here attributed to it, or perhaps not at all. *The demise of the Value-Free Ideal lead to the development of an alternative ideal, in which science is not regarded as inhabiting a detached and perhaps privileged ‘Ivory Tower’ but ... (page 3)*.

In another passage, the author seems to interpret co-production as a synonym of Citizen science (extreme CS perhaps), which is again not a commonly shared meaning. *In the co-production model of science, in fact, researchers together with other societal actors are ‘co-responsible’ for determining the direction of scientific research and for the implementation of its results into society (page 13)*. Finally, on page 8, he seems to equate co-production to “interdisciplinary science”, an expression, by the way, which he uses only when referring to Osbeck and Neressian’s work (2017), i.e. on page 8 and 11. In the rest of the paper he talks, more convincingly, of “interdisciplinary research”.

Just to make myself totally clear, I am not claiming that there is a “copyright” on the term/concept of co-production. Just the opposite: as there are multiple uses, the author must declare/clarify his own.

Some clarification is also necessary on how inclusive the category of “scientists” is considered to be, and whether the term is a synonym for “researchers”. For example, the author quotes a study on a *large engineering lab.*, concluding that *engineers ... etc. (page 7)*. Are scientists and engineers one and the same category? Does scientific research cover the same area as technological innovation? Are *scholars from the Humanities* (page 4) also scientists? Again, I don’t ask for the right answers; just for a clarification. I deem it important to understand whether the different terms are used just to avoid repetition or to identify (somewhat) different groups.

A final, minor observation. The term *demise*, referred to the Value-Free Ideal, is probably too strong, as that conception of science – though contested - is very much alive in some quarters.