Qeios

Peer Review

Review of: "Toward a New Definition of Psychogastroenterology: From the Brain–Gut Axis to the Human Holobiont"

Alexandra Lacurezeanu[1]

1. University of Agriculture and Veterinary Medicine Cluj-Napoca, Romania

This manuscript proposes an updated definition of psychogastroenterology, arguing that existing formulations by Keefer, Knowles, Mikocka-Walus, van Tilburg, and Drossman remain confined to the Brain–Gut Axis paradigm and therefore fail to reflect the more recent Microbiota–Gut–Brain Axis, the distinction between brain and mind as separate levels of organization, and the broader biopsychosocial "holobiont" framing the author has proposed elsewhere. The manuscript is clearly organized, moves logically from a historical review (Figure 1, Table 1) to a revised definition, and closes with clinical implications and future directions. The writing is fluent, and the historical reconstruction of the field, from psychosomatic medicine through the biopsychosocial model to the Brain–Gut and Microbiota–Gut–Brain axes, is accurate and pedagogically useful.

That said, I have reservations about whether the manuscript's central theoretical claim is developed with sufficient rigor to support the definitional revision it proposes, and about the manuscript's novelty relative to the author's concurrent work.

My main concern is the treatment of the mind–brain distinction, the conceptual engine of the whole proposal. The claim that the mind is "grounded in, but not fully reducible to, neural activity" is asserted rather than argued: it occupies roughly half a page and rests on four citations spanning quite different traditions (Dennett, Hofstadter, Gazzaniga, Noble, Ellis) without specifying which notion of emergence is intended, weak/epistemic emergence, where mind-talk is a convenient higher-level description of processes that remain in principle reducible, or strong/ontological emergence, where mind possesses genuinely novel causal powers. These are not interchangeable positions, and mind–body reduction remains one of the most contested questions in the philosophy of mind and cognitive science, with reductionist accounts still widely defended. I would encourage the author to state explicitly which

 doi.org/10.32388/D192P3

position is being adopted and to acknowledge, even briefly, that competing accounts exist. Related to this, since the manuscript uses "axis" throughout to denote physiological and neurobiological communication pathways, it would help to clarify whether "mind" is meant as a further node within that mechanistic axis or as an emergent level at which the effects of the axis arehe manuscript's own logic (mind as non-reducible) suggests the latter, but the "Microbiota–Gut–Brain–Mind Axis" terminology suggests the former, and this tension is not resolved.

A second concern is the manuscript's relationship to the author's prior work. Three of the twenty-one references (2, 7, 10) are single-author Qeios preprints by the same author, apparently introducing the MGBM axis and the Bio-Psycho-Social Holobiont Model that the manuscript builds directly upon. As it stands, it is difficult to determine what this manuscript contributes beyond synthesizing and restating those companion pieces. I would ask the author to state explicitly, early in the manuscript, what is original here relative to refs 7 and 10, since this affects how the novelty of the definitional proposal should be assessed.

A third concern, related to a point I would also raise about scope, is that the proposed definition covering bidirectional interactions among mind, nervous system, gastrointestinal system, microbiota, and social environment, operating through psychological, neural, endocrine, immune, metabolic, and social processes, is comprehensive to the point that it is not obvious what would fall outside it, or what finding would count as evidence against the framework's usefulness. The manuscript would be considerably strengthened by a concrete example, even a brief hypothetical one, showing how this definition generates a different research question, hypothesis, or clinical decision than the earlier Brain–Gut Axis-based definitions it seeks to replace. Without such an example, the contribution risks being read as primarily terminological rather than generative.

I would also note that the two empirical examples offered in support of "bottom-up" causality (intestinal dysbiosis and fear extinction; intestinal inflammation and anxiety/depression via choroid plexus signaling) are both drawn from rodent studies. Given that the manuscript's argument rests on updating a clinical human discipline, it would be helpful to explicitly flag the preclinical status of this evidence, since translation from these animal models to human GI–psychiatric comorbidity is not yet established.

On the historical reconstruction: as with the companion literature this proposal builds on, it would strengthen the manuscript to briefly explain how the definitions discussed in Table 1 were selected, whether through a systematic search, key reference works, or another method, so readers can gauge the completeness of the review. I would also gently suggest that Figure 1's linear five-stage progression

slightly overstates the tidiness of the field's history; psychosomatic medicine and the biopsychosocial model, for instance, continue to coexist as active frameworks rather than functioning as strictly superseded stages, and a brief note that the figure is a simplified didactic schema would improve accuracy.

Finally, "holobiont" is a term drawn from evolutionary biology, where it denotes a host and its associated microbial communities as a unit of selection at the population level. The manuscript's use of the term for an individual patient's biopsychosocial state is a legitimate extension but is not flagged as such, and the definition's own inclusion of "social environment" within the holobiont framework risks blurring the host-microbiota unit with the broader environment it is embedded in. A brief clarifying sentence distinguishing these would preserve conceptual precision while still allowing the manuscript to fully address environmental and social interactions.

The following points are offered as minor suggestions and do not bear on the overall assessment. The Table 1 entry for the expanded Knowles, Keefer, and Mikocka-Walus definition is split awkwardly across the page 7/8 boundary and should be checked in production. The claim that "formal definitions of the discipline have been derived primarily from the work promoted by the Rome Foundation" (p. 5) is not accompanied by a citation. A short Limitations section is currently absent and would be a natural place to address the falsifiability and operational scope concerns raised above.

Overall, I consider this a clearly written manuscript addressing a genuinely relevant question of whether current definitions of psychogastroenterology keep pace with microbiome and systems-biology advances, but I do not think it yet establishes its central theoretical claim with sufficient rigor, or sufficiently distinguishes its contribution from the author's own concurrent preprints, to be accepted in its present form. I recommend major revision, focused principally on substantiating the mind–brain distinction, clarifying originality relative to refs 7 and 10, and adding a concrete illustration of the proposed definition's generative value.

Declarations

Potential competing interests: No potential competing interests to declare.