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RECEIVED 09 July 2025

ACCEPTED 31 July 2025

PUBLISHED 13 August 2025

CITATION

Simha P (2025) The review paper boom: we
need fewer, better reviews in emerging water
research niches.

Front. Environ. Sci. 13:1662664.

doi: 10.3389/fenvs.2025.1662664

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The review paper boom: we need fewer, better reviews in emerging water research niches

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KEYWORDS

scientific publishing, circularity, wastewater innovation, source-separated sanitation, peer review practices, review articles, knowledge synthesis, nutrient recycling

Introduction

In rapidly evolving research niches like source-separated sanitation, how we communicate science shapes not only academic debates but also the adoption of new ideas in mainstream practice. This field, which focuses on safely recycling source-separated wastewater fractions, such as human urine and blackwater, has grown significantly over the past 3 decades (Larsen et al., 2021; Aliahmad et al., 2022). Many of its technologies have matured, with some already implemented at the neighbourhood scale (Skambraks et al., 2017; Drangert and Kjerstadius, 2023). I have worked in this area for over a decade alongside a research group that has driven its progress since the mid-1990s. Through this journey, I have seen firsthand both its successes and the hurdles it faces in gaining broader acceptance. In this brief opinion piece, I reflect on the role of scholarly publishing, particularly review articles, in advancing or potentially hindering innovation in this niche.

Are review papers helping or holding us back?

Interest in source-separated sanitation is on the rise, as industries and policymakers look for circular approaches to wastewater treatment (Aliahmad et al., 2023). But the field remains on the periphery of mainstream water research. Our work often stands apart at conferences, where its rationale and significance are not always widely recognised. The idea of using recycled fractions like human urine as fertiliser still makes many people uneasy (Simha et al., 2021) and is sometimes dismissed as impractical or even a relic of “hippie science”.

The publishing landscape reflects this. While there are now several hundred scientific papers on urine recycling, they still account for less than 1% of all wastewater treatment research (Aliahmad et al., 2022). Despite this, review articles on urine recycling have surged, with more than 50 published in the past decade, most within the last 5 years (a non-systematic scan of the literature is shown in [Supplementary Table S1](#), Supplementary Information). This rapid increase raises an important question: are these reviews truly advancing our understanding of the subject?

To be clear, I am not against review papers in principle. A well-timed, well-crafted review can provide real value, helping researchers make sense of a growing field. *Source Separation and Decentralization for Wastewater Management*, a book published by IWA in 2013, is a rare example of how a comprehensive resource can shape an entire subject area (Larsen et al., 2013). Written by researchers deeply embedded in the field, the book synthesised more than a decade of work, combining conceptual frameworks,

foundational knowledge, and lessons from real-world implementation. Each chapter provided a focused, in-depth treatment of a specific topic, structured almost like a standalone literature review. Taken together, the book helped define many of the core concepts in source-separated sanitation and continues to guide research today. When I first entered this niche as an undergrad researcher, diving into the book shortly after its publication helped orient me to the field and gave me a foothold in its emerging knowledge base. With foundational texts like this already available, I cannot help but question the value of every new review that gets published. Whenever I encounter a recently published review, or another request lands in my inbox to evaluate one, I find myself asking: does this paper truly add something new to the field? And at what point is there enough original research to justify writing yet another review?

In emerging fields, an overabundance of reviews can sometimes do more harm than good. When new reviews appear every few months, it can start to feel like the field is circling the same ideas rather than moving forward (Rotolo et al., 2015). Over time, this might give the impression that there are few novel ideas or unresolved questions left to explore, even when that is not the case. It can also make it harder for readers, especially those new to area, to figure out which reviews offer fresh insights and new conceptual directions. Some overlap between reviews is inevitable as consolidating knowledge is natural part of the scientific process (Dhillon, 2022). But at what point does building on past work start to resemble simply repeating it?

Broader trends in scientific publishing

This trend in the source-separated sanitation sector mirrors a larger shift in academic publishing. Across disciplines, the number of review papers has surged, partly because they are an attractive option in the “publish or perish” culture (Suart et al., 2022). Reviews typically attract more citations than original research, making them a strategic choice for early-career researchers and those under pressure to meet publication targets. I know this firsthand. Early in my career, I co-authored a review that, in hindsight, was not my strongest work. At the time, simply getting published felt like an achievement. That experience has stayed with me and gives me empathy for others navigating similar situations. But it also reinforces my belief that review papers should be written for the right reasons: to genuinely advance a topic, not just to fulfil publishing quotas.

The path forward: fewer, better reviews

My goal here is not to discourage review papers. When done well and at the right time, a thoughtful review can synthesise fragmented knowledge, provide clarity to researchers and practitioners, and accelerate scientific progress (Dhillon, 2022). But in emerging fields, where primary research is accumulating slowly and foundational concepts are still taking shape, a premature or superficial review can inadvertently add noise rather than clarity. Naturally, as niches like source-separated sanitation grow, more researchers will join,

bringing valuable new perspectives and, in turn, a legitimate need for periodic synthesis of knowledge. Precisely because this growth is inevitable, the community needs clearer guidelines for deciding *when* and *how* to publish reviews. To that end, I propose three practical, qualitative guidelines that could help:

1. **Make publishing decisions more transparent:** Many journals already ask authors to justify the need for a review in their cover letters. For instance, *Water Research* expects authors of critical reviews to outline their expertise, cite relevant publications, and explain how their review advances existing syntheses. But in emerging niches, where the literature is limited, uneven, and evolving, the decision to publish a review should be both clearer and more transparent. To achieve this, journals could adopt the practice of publicly sharing (as Supplementary Material or linked content) both the authors' original cover letters and a concise editorial statement clarifying why the manuscript was sent for peer review. This practice would increase editorial transparency and establish expectations for future review submissions.
2. **A mandatory “Review synopsis and added-value statement”:** Journals should require authors of review papers to include a brief “Review synopsis and added-value statement,” placed near the abstract. This ~50 word summary should clearly articulate: (a) what the review uniquely contributes compared to recent publications, (b) why the timing of the review is appropriate, and (c) how it sets an agenda or identifies concrete next steps for the field. Editors and peer reviewers should evaluate the strength and clarity of this statement as part of the review process. Similar formats already exist in many journals; for example, *Environmental Science and Technology* requires a ~20-word synopsis that outlines the environmental context and impact of the research.
3. **Explicit forward-looking agenda:** Emerging research fields are dynamic, often undergoing shifts in methods, paradigms, and approaches. Because of this, reviews should actively help shape the direction of future work. Authors should dedicate a substantial section of the review to identifying research needs, gaps, priorities, or proposing conceptual agendas. For example, our mini-review in this journal on the use of synthetic versus real human urine in experimental research (Simha et al., 2024) offered practical guidelines for researchers entering the niche—clarifying when synthetic urine is appropriate, why real urine is generally preferable, and outlining best practices for preparing artificial urine to directly inform experimental design.

While these three recommendations are particularly critical in emerging niches, applying their underlying principles could benefit scientific publishing more broadly across water sciences, sustainability research, and beyond. Ultimately, the goal is not fewer reviews *per se*, but better, more purposeful ones. Across science, we should always ask ourselves: are review papers being written because they are needed? If not, perhaps original research articles or targeted commentaries would make for more valuable contributions. The next time a review paper crosses my

desk—whether as an editor, peer reviewer, or author—I will pause to reflect on these questions. I hope others will too.

Author contributions

PS: Writing – original draft, Formal Analysis, Methodology, Writing – review and editing, Conceptualization, Resources.

Funding

The author(s) declare that no financial support was received for the research and/or publication of this article.

Conflict of interest

The author declares that the research was conducted in the absence of any commercial or financial relationships that could be construed as a potential conflict of interest.

The author(s) declared that they were an editorial board member of Frontiers, at the time of submission. This had no impact on the peer review process and the final decision.

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Supplementary material

The Supplementary Material for this article can be found online at: <https://www.frontiersin.org/articles/10.3389/fenvs.2025.1662664/full#supplementary-material>