

MATTERS ARISING

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Response to: Matters Arising from Salari et al. (2025): Why it is (currently) impossible to estimate accurate prevalence rates of the Impostor Phenomenon

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Abstract

The Impostor Phenomenon (IP) is conceptually a continuous construct on a spectrum, rather than a categorical or definitive diagnosis. Therefore, reference to its 'prevalence' in the epidemiological sense may seem to be conceptually incorrect. That said, this does not rule out threshold-based prevalence estimates. There are comparable constructs that are also on a continuum, yet thresholds are used to estimate prevalence. Instances of this include depression (PHQ-9), and anxiety (GAD-7), where empirically derived thresholds are used to provide estimates of prevalence. We would also like to emphasize that our article was a systematic review and meta-analysis. Systematic reviews and meta-analyses are studies that aim to synthesize existing evidence reported within original/primary studies in a specific field and produce a pooled estimate. In primary studies, the percentage or proportion (prevalence) of IP among health service providers was reported. We fully acknowledge that the current cut-offs for the Clance Impostor Phenomenon Scale (CIPS) are inconsistently applied in the existing literature and lack systematic validation. Demonstrating variability across arbitrary thresholds does not invalidate prevalence research and meta-analyses. The inconsistency in operationalization across the literature and not necessarily a flaw in the meta-analysis application. Meta-analysis allows us to quantify this problem and to highlight how different thresholds, samples, and instruments contribute to variance. Without such a synthesis, the field would lack a systematic appreciation of how widely prevalence estimates diverge.

Keywords Impostor Phenomenon, Prevalence, Heterogenicity, Thresholds, Meta-analysis

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We would like to thank our colleagues, Drs Brauer and Proyer for their considerate commentary [1], and constructive reflections on our systematic review and meta-analysis [2]. We value the opportunity to join this scientific dialogue and to address the key points raised, and we are grateful to the editorial team at BMC Psychology.

We agree with some of the arguments that colleagues have raised in their commentary and welcome their analysis, which adds important depth to the interpretation of our findings. Nonetheless, we respectfully disagree with the conclusion that approximating prevalence within the context is “impossible”.

We begin by explicitly agreeing with the primary argument of colleagues; They correctly emphasize that the Impostor Phenomenon (IP) is conceptually a continuous construct on a spectrum and not a categorical or definitive diagnosis. Therefore, reference to its ‘prevalence’ in the epidemiological sense may seem to be conceptually incorrect. That said, this does not rule out threshold-based prevalence estimates. There are comparable constructs that are also on a continuum, yet thresholds are used to estimate prevalence. Instances of this include depression (PHQ-9), and anxiety (GAD-7), where empirically derived thresholds are used to provide estimates of prevalence [3, 4].

We would also like to emphasize that our article [2] was a systematic review and meta-analysis. Systematic reviews and meta-analyses are studies that aim to synthesize existing evidence reported within original/primary studies in a specific field and produce a pooled estimate. In primary studies, the percentage or proportion (prevalence) of IP among health service providers was reported. This was mostly based on different cut-off thresholds on measurement scales, equating the proportion of high scorers with prevalence. In our study, we synthesized the reported proportions and pooled the estimate. Our use of the word ‘prevalence’ was to describe the cumulative proportion of individuals who scored above the various thresholds used in the included primary studies.

We fully acknowledge that the current cut-offs for the Clance Impostor Phenomenon Scale (CIPS) are inconsistently applied in the existing literature and lack systematic validation. Colleagues, Drs Brauer and Proyer correctly highlight variability across cut-offs, which we also reported in our work [2]. We therefore agree that until robust validation studies are conducted, any prevalence estimates should be presented with caution and accompanied by explicit caveats, to avoid over-interpretation. Nevertheless, the example provided in the commentary relates to an application on two convenience datasets, which are not population-representative and therefore should not be used to generalize instability of prevalence to broader populations. Furthermore, some of the cut-offs they apply are arbitrary, which artificially

inflates heterogeneity. Demonstrating variability across arbitrary thresholds does not invalidate prevalence research and meta-analyses; it highlights the need for consensus and validation studies. Nonetheless, as argued by colleagues, applying six different cut-offs to the same dataset produced very different proportions of cases. This therefore highlights a fundamental flaw in the existing literature, which our meta-analysis inherited. We thank colleagues for presenting this empirical demonstration, which reinforces the conclusion we had noted: “Prevalence [...] depends largely on the screening tools and cut-off values used” [2, p.13].

With respect to heterogeneity, Drs Brauer and Proyer interpret the high heterogeneity in our meta-analysis ($I^2 = 98.6\%$) as evidence that pooling prevalence rates is invalid. We would argue that this heterogeneity is itself an important empirical finding. It reflects the inconsistency in operationalization across the literature, and not necessarily a flaw in the meta-analysis application. Meta-analysis allows us to quantify this problem and to highlight how different thresholds, samples, and instruments contribute to variance. Without such synthesis, the field would lack a systematic appreciation of how widely prevalence estimates diverge. Colleagues also suggest that if 62–70% of individuals report impostor feelings, IP should be regarded as a “normative experience”. However, we would need to be mindful that high prevalence does not imply triviality; for instance, a significantly high number of physicians report burnout symptoms [5], yet these findings have clear implications for health care delivery. Similarly, impostor feelings even if common are associated with impaired well-being, reduced job satisfaction, and burnout risk [6]. Thus, prevalence estimates are still useful for identifying the scope of potential consequences.

Beyond the argument surrounding prevalence, our review summarized findings on factors associated with IP, such as its relationship with self-esteem, anxiety, depression, stress, and burnout. These findings, based on correlations using continuous scores, are conceptually reliable and retain their value because they are less dependent on categorical cut-offs, which was a central argument in the commentary.

Our meta-analysis does not endorse any one cut-off, rather demonstrates the consequences of their inconsistency; it functions as a warning, and the interpretation of colleagues, Drs Brauer and Proyer, provides the conceptual framework for understanding it. We would therefore very much welcome the commentary, as it highlights the need for large-scale validation studies of CIPS thresholds against external criteria, representative samples to avoid biases of convenience datasets, as well as the need for building consensus among researchers on cut-off

standards, similar to practices in depression and anxiety research.

The presented cases show the nature of a mental disorder and the difference in the tools that have evolved to suit the personality and mind of the human being and have provided different ranges and thresholds for the tools. We can never examine a disorder with certainty because the use of tools has always faced limitations, these tools are tools that screen for a disorder and then the doctor deals with this disorder. This is the nature of the tool and as we know, different tools have false positives and false negatives and this is the limitation of the tool and therefore different thresholds in the tool can also be used to overcome the limitations of the tool and make it more efficient for screening. Therefore, our goal in examining this issue is to first pay attention to this disorder that has received less attention and secondly to provide information and explanations to clinical specialists. Also, presenting the opinions of Drs Brauer and Proyer and our response emphasizes the same issue so that science can improve in this field and in future studies, the effect of the threshold of screening tools in different studies and its effect on heterogeneity should be considered.

Finally, we would like to thank colleagues, Drs Brauer and Proyer, for their valuable commentary, which enhances the interpretation of our work and supports a better understanding. We view their input as an essential scientific contribution that clarifies the context and importance of the research. Our meta-analysis documented a problem and reported a pooled prevalence. Colleagues' interpretation expertly highlighted interpretation challenges, and in our view, together they point to the possible next steps. We hope this exchange will nurture a more rigorous methodological and conceptual approach in future research on the Impostor Phenomenon.

Abbreviation

IP Impostor Phenomenon
CIPS Clance Impostor Phenomenon Scale

Supplementary Information

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Supplementary Material 1: Reviewer reports

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Author contributions

NS and HRH and MM contributed to the design, MM and AHF and ARF and PH prepared the manuscript. MM and AHF and HRH and SR and PH helped in the interpretation of the study. All authors have read and approved the content of the manuscript.

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