AMERICAN PSYCHOLOGICAL ASSOCIATION

Psychology of Popular Media

ISSN: 2689-6567

2025, Vol. 14, No. 2, 210–212
https://doi.org/10.1037/ppm0000592

REPLY

Longer-Term Interventions to Reduce Social Media Time Do Not Improve Mental Health: A Reply to Thrul et al. (2025)

Christopher J. Ferguson
Department of Psychology, Stetson University

In a reanalysis of data from my meta-analysis of social media experiments, Thrul and colleagues report that longer-term intervention studies produced more positive results. I appreciate Thrul and colleagues' constructive and interesting reanalysis. However, other issues reduce my enthusiasm for their conclusions. First, expected linear relationships regarding intervention length and outcome suggest that time is not a moderator of effectiveness, nor does a curvilinear trend support social media reductions as useful. Second, the time moderator is also correlated with citation bias, suggesting a confound of moderators. Third, even considered alone, longer-term interventions did not reach the preregistered minimal effect size threshold for potential noise results. Fourth, demand characteristics are likely to increase with study length. It is concluded that even longer-term intervention studies provide little compelling evidence that reducing social media time improves mental health.

Public Policy Relevance Statement
The comment by Thrul et al. (2025) suggests that the impact of social media reduction interventions may depend upon the length of the reduction. However, a reanalysis of these data suggests that the length of the intervention does not moderate treatment success. Even longer-term reductions do not meet the evidentiary threshold for success. Lastly, these studies have serious interval validity problems that, either way, render them largely unable to answer the central question of whether reducing social media time is useful. Nonetheless, taken at face value there remains no good evidence that reducing social media time is useful for improving mental health.

Keywords: social media, mental health, experiments, meta-analysis

I very much appreciate the comment by Thrul et al. (2025) on my meta-analysis of social media experiments and improving mental health. This was a very constructive comment with some interesting and rigorous analyses. This is an excellent example of the productive way emotional debates over technology and youth mental health can be conducted.

At the same time, I am less sanguine than Thrul et al., about the utility of social media reduction for improving mental health. I have four observations.

First, Thrul and colleagues analyze intervention time as a binary split and as a curvilinear continuous variable. However, intervention time is a continuous variable, and my impression is that interventions are assumed to be linear and positive. Jané (2024) analyzed the data from my meta-analysis using time as a continuous variable. In his analysis, intervention time was not a statistically significant moderator. Here, I replicate Jané's analysis. I independently coded intervention time in days. These perfectly matched those of Thrul and colleagues as reported in their supplemental material. My only difference is that I disagree about excluding the shorter laboratory studies and coded these as one-half day. Nonetheless, I ran the analyses both with and without those studies (updated effect size table provided at https://osf.io/w6xa5; Ferguson, 2024).

With all studies included, and time of intervention as a continuous variable, it was not a significant moderator ($Z = 0.374$, $p = .708$). Excluding the shorter laboratory studies, the time of intervention still was nonsignificant ($Z = 0.688$, $p = .491$). These analyses are linear rather than curvilinear, but I was unclear as to why we would expect a curvilinear relationship. Such a relationship would imply people should go back to using social media after a break of a week or so to maximize their mental health. Perhaps that is true, but I think we need clearer evidence to reach that conclusion. Indeed, using the data from Thrul's curvilinear analysis, we would conclude that any benefits from reducing social

Nancy Rhodes served as action editor.
Christopher J. Ferguson https://orcid.org/0000-0003-0986-7519
The author has consulted with the free speech group Foundation for Individual Rights in Expression (FIRE) on legal cases related to social media. The author has no funding from nor has done any consultation with social media or other technology companies. The materials are available as the additional online materials on the Open Science Framework at https://osf.io/w6xa5.
Correspondence concerning this article should be addressed to Christopher J. Ferguson, Department of Psychology, Stetson University, 421 North Woodland Boulevard, DeLand, FL 32729, United States. Email: CJFerguson1111@aol.com

media quickly wear off and actually reverse. Taken together with Thrul's results, my results and those of Jané (2024) suggest that time of intervention is not robust as a moderator and, as such, we do not have clear evidence that time of intervention is a useful indicator of success. Indeed, the study with the longest intervention length (Collins & Eggers, 2022) actually had a negative effect size. It is not my point to make any particular conclusions about what people should do. Rather, it is not possible to make any firm conclusions from such a messy pattern of results, and those who have made such firm conclusions, even basing public policy on them, have done so at great risk.

The concept of withdrawal is sometimes used to explain why interventions may initially be negative but may be positive with greater time lengths. This rationale often appears to be a post hoc one. However, applying the concept of withdrawal established for substance abuse to nonsubstance behaviors has always been controversial, and such borrowed symptoms appear to work poorly as clinical indicators where technology is concerned (Przybylski et al., 2017). Furthermore, the curvilinear trend reported by Thrul and colleagues does not support this explanation. One cannot use one poorly supported paradigm (i.e., behavioral withdrawal) to attempt to rationalize why another paradigm is performing poorly. I argue this creates a pseudoscientific tautology.

Second, I observed that the intervention time moderator is correlated with the citation bias moderator ($r = -.191$). Although small, this correlation is much larger than the effect size for social media reduction intervention studies and mental health. This suggests these two moderators are not entirely independent of each other. Why scholars who are more honest about the inconsistent results for social media interventions tend to conduct intervention studies with shorter lengths is unknown, though real-world field studies were also more common for shorter studies (e.g., Gajdics & Jagodics, 2022).

Third, it is worth noting that Thrul et al.'s reported effect size for longer-term intervention studies of $d = 0.169$, still does not meet the preregistered smallest effect size of interest of $d = 0.21$ (which would indicate that social media reduction is associated with a 1% improvement in mental health, still very tiny). Effects below $d = 0.21$ have a very high probability of being statistical noise, not real effects (Ferguson & Heene, 2021). Even ignoring the issues of noise or demand characteristics, this would indicate that restricting social media is associated with a 0.7% improvement in mental health—an amount so small as to be practically unnoticeable. Thus, even these longer-term studies fail to produce convincing evidence that reducing social media time is associated with improved mental health.

Lastly, many of these debates about social media experiments seem to focus on nudging effect sizes in one direction or another to meet some p value or cross some effect size threshold. However, often lost in these dialogues is a more central issue: The demand characteristics of these studies are such a clear problem that the internal validity of most of these studies is compromised. These demand characteristics occur because these studies focus on requiring participants in experimental conditions to restrict or eliminate their use of social media. This is not a subtle request. In many studies, respondents are also repeatedly asked to report on their mental health (sometimes before the study, during the study, and at multiple time points after the study). Under such conditions, it is not too difficult for participants to surmise the point of the experiment (assuming it was not blatantly laid out in the recruitment materials) which may cause them to alter their responses.

Related to this, participant expectations are likely already influenced by very public discussions of this topic in news media, among policymakers, and even in college classes, for undergraduate participant studies. In some cases, disparate findings may come down to luck … to what degree did a given sample have preexisting and ultimately self-fulfilling expectations about what social media reductions should do.

It is likely that, where demand characteristics occur, longer-term interventions may actually make them more salient. In conducting my original meta-analyses, my point had been that taking these studies at face value they provided little evidence for beneficial effects. Claims by some, particularly in the political and public sphere, of a causal connection that could inform policy were out of step with the available evidence. That remains true. However, it is also clear that we should not take these studies at face value and serious questions remain about their ability to inform larger dialogues and debates.

I think it is a fair question to ask: if these experimental studies are so poor, then why conduct a meta-analysis at all? Several examples of public discourse (e.g., Haidt, 2024; Hanania, 2023) had taken to claiming that there was reasonably clear and consistent evidence from experimental studies of causal effects that could be translated into real-world benefits from reducing social media. These claims were used to rationalize some policy interventions. Thus, there was value in identifying this as a miscommunication of the experimental evidence, even taken at face value, and we should be concerned when the public has been ill-informed (even if in good faith), given the public policy implications of such claims.

Ultimately, my original conclusions stand. There is little face-value evidence that reducing social media time improves mental health. And also, the current crop of experiments is so compromised by demand characteristics that their evidentiary value is limited. At the same time, I find the contribution of Thrul et al. (2025) to be a very valuable and thoughtful contribution to these discussions. In particular, I thank them for the cordial tone with which they have conducted themselves, whatever our disagreements may be. This is an example of scientific disagreement done right. Nonetheless the conclusion remains: this pool of studies should never have been used to make causal attributions about social media, nor to support public policy on the same.

References

Collins, A., & Eggers, F. (2022). Effects of restricting social media usage on wellbeing and performance: A randomized control trial among students. *PLOS ONE*, *17*(8), Article e0272416. https://doi.org/10.1371/journal.pone.0272416

Ferguson, C. J. (2024, October 2). *Social media experiments meta-analysis*. https://osf.io/hv7us

Ferguson, C. J., & Heene, M. (2021). Providing a lower-bound estimate for psychology's "crud factor": The case of aggression. *Professional Psychology: Research and Practice*, *52*(6), 620–626. https://doi.org/10.1037/pro0000386

Gajdics, J., & Jagodics, B. (2022). Mobile phones in schools: With or without you? Comparison of students' anxiety level and class engagement after regular and mobile-free school days. *Technology, Knowledge and Learning*, *27*(4), 1095–1113. https://doi.org/10.1007/s10758-021-09539-w

Haidt, J. (2024). *Yes, social media really is a cause of the epidemic of teenage mental illness*. https://www.afterbabel.com/p/phone-based-childhood-cause-epidemic

Hanania, R. (2023). *How I changed my mind on social media and teen depression*. https://www.richardhanania.com/p/how-i-changed-my-mind-on-social-media

Jané, M. B. (2024). *A statistical interrogation of "The case for causality, part 1" by Rausch and Haidt*. https://matthewbjane.github.io/blog-posts/blog-post-6.html

Przybylski, A. K., Weinstein, N., & Murayama, K. (2017). Internet gaming disorder: Investigating the clinical relevance of a new phenomenon. *American Journal of Psychiatry*, *174*(3), 230–236. https://doi.org/10.1176/appi.ajp.2016.16020224

Thrul, J., Devkota, J., AlJuboori, D., Regan, T., Alomairah, S., & Vidal, C. (2025). Social media reduction or abstinence interventions are providing mental health benefits—Reanalysis of a published meta-analysis. *Psychology of Popular Media*, *14*(2), 207–209. https://doi.org/10.1037/ppm0000574

Received October 7, 2024
Revision received January 15, 2025
Accepted January 29, 2025 ■