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When Public Narratives Collide With Data: Post Hoc Reasoning, Construct Confusion, and Pervasive Demand Characteristics: Reply to Rausch and Haidt [AQ2](#)

Short title : PUBLIC NARRATIVES AND DATA [AQ1](#)

FERGUSON

COMMENTARY

 **Christopher J. Ferguson**^{1,*}

¹Department of Psychology, Stetson University

Footnotes

Nancy Rhodes served as action editor.

Christopher J. Ferguson once consulted with the free speech group Foundation for Individual Rights in Expression on a social media case. He has no financial or other ties to the technology industry.

Christopher J. Ferguson served as lead for conceptualization and writing—original draft.

Corresponding Author

* Christopher J. Ferguson [AQ3](#), Department of Psychology, Stetson University, DeLand, FL 32729, United States CJFerguson1111@aol.com

In their comment on my 2025 meta-analysis of experimental studies, Rausch and Haidt criticize my work on several issues. As I show in this reply, their comments are mistaken on multiple grounds. First, there were no errors made in the inclusion of studies, and corrections to several effect sizes were minor and had no influence on the meta-analytic results. Second, outcomes used in the study

share factor structures suggesting a single latent dimension, share nomological networks, and splitting them risks the “jingle-jangle” problem. Thus, their shared evaluation is warranted, and splitting them, particularly with few and heterogeneous studies, risks false positives due to chance. Third, there is little evidence study duration influences effect size, and this moderator is confounded with researcher expectancy effects. Fourth, Rausch and Haidt neglected to consider a large point of the original article that was that demand characteristics rendered many studies of this type unable to answer fundamental questions. Fifth, my original meta-analysis has now been twice independently replicated. Far from being fundamentally flawed, my meta-analysis now joins other replications indicating reducing social media time does not improve mental health.

Public Policy Relevance Statement

Rausch and Haidt (this issue) objected to my meta-analysis of social media experiments, which failed to provide evidence for claims they have made about social media effects. Their comments are mistaken, often based in faulty post hoc reasoning, lack of conceptual awareness of the nature of mental health, a misread of effect sizes, as well as failure to understand the inherent limitations of experimental studies. The original meta-analysis as well as others that have now replicated it remain important for public policy decisions.

Keywords

meta-analysis, social media, mental health, adolescents, moral panic

I thank Rausch and Haidt (this volume) for their comments on my 2025 meta-analysis of social media reduction experiments. In the original meta-analysis, I concluded that, taken at face value, such studies were heterogeneous in outcome and, on balance, provided little evidence for the effectiveness of reducing social media time on mental health. Furthermore, I concluded that such studies were uniformly limited by demand characteristics due to obvious hypotheses participants would have been aware of, with a high risk of false-positive results. Rausch and Haidt challenged my results, suggesting I made errors of study inclusion, effect size, and mixing together heterogeneous constructs. In this reply, I demonstrate that Rausch and Haidt’s comments are

mainly mistaken, in some cases arguably reflecting post hoc arguments that may typify the kind of replication crisis decisions that lead to false-positive results. Furthermore, though I make no claim my original meta-analysis was beyond critique, it has aged well, now having been twice replicated by independent studies. As such, it remains an important contribution to public policy decisions.

Background Context

Dr. Haidt, with the assistance of Dr. Rausch, has published a bestselling book, *The Anxious Generation* (Haidt, 2024) and has been very active in meeting with world and state leaders (e.g., Governor SHS, 2025) regarding policy efforts often inviting government to regulate the speech and technology access of minors and, by extension, adults^{AQ4} (Stacey, 2026). However, considerable research evidence has challenged the causal and even correlational claims made in *The Anxious Generation* (e.g., Cheng et al., 2026; Yang & Feng, 2024). My meta-analysis was one piece of evidence conflicting with the causal claims of *The Anxious Generation*. Particularly given the high-level and international policy influence of *The Anxious Generation* this creates a unique context that is important to consider.

In their comment, Rausch and Haidt make several references to our correspondence. This reply will focus exclusively on substantive data and methods matters, as is most constructive for scientific discourse. If readers are curious for a full accounting of that time, I invite them to consider the Chronicle of Higher Education's detailing of this situation (Lee, 2024).

Construct Balkanization and the Jingle-Jangle Problem

One of the most interesting substantive critiques is the suggestion that it is wrong to include multiple constructs in a single meta-analysis. This is, of course, true if indeed the variables truly are unrelated. For instance, if we were considering the impact of video games, we would not include aggression and visuospatial cognition outcomes together, as these relate to entirely different hypotheses.

However, for experiments of social media reduction, the conceptual overlap of constructs such as

depression, anxiety, mental well-being, life satisfaction, self-esteem, and so on, is considerable. This has, in fact, long been recognized in the psychological and psychiatric literature. Specifically, although the *Diagnostic and Statistical Manual* places mental health diagnoses into a categorical system, evidence suggest that a single *p-factor* or “general distress” provides a single latent construct (Caspi et al., 2014). It is worth noting that this work considers not just depression, anxiety, and related disorders but a wide range of psychopathology, though considerable overlap between depression and anxiety as “general distress” is also noted (Snyder et al., 2023). We need not declare that all constructs are exactly the same, but rather that they all draw from a single latent dimension of psychological distress.

This can extend to other variables as well. It is not surprising to find that variables such as depression and anxiety correlate very highly both cross-sectionally and longitudinally with variables such as self-esteem, general mental wellness, happiness, and life satisfaction (e.g., Han et al., 2025; Martins et al., 2022). The assumption of independence is also violated because many measures of anxiety or depression ask directly about these other variables such as happiness, self-esteem, or quality of life. For instance, the Beck Depression Inventory asks specifically about self-dislike and self-criticalness (i.e., self-esteem). Both the Beck Depression Inventory and Satisfaction with Life Scale ask about perceived life success and failure. The Center for Epidemiologic Studies Depression Scale [AQ5](#) has questions about happiness, loneliness, and “I enjoyed life.” It is inconsistent to argue these are unrelated constructs when scales for one construct quite basically ask about the other construct as part of a symptom checklist. It is certainly reasonable to argue these are all elements of a larger “general distress” construct, but one with a core latent dimension. Given that these share a common nosological and predictive framework, treating them together in meta-analysis is warranted.

This issue is similar to that raised by Boster (2023) of construct proliferation wherein the same construct is pseudorecreated, given new names over and over despite lacking discriminant validity. Boster recommends a “construct dump” as a means by which redundant or irrelevant constructs might be retired.

By contrast, arguing post hoc to separate them out risks chance false-positive outcomes and cherry-picking, particularly when number of studies is low and outcomes are heterogeneous. This relates to the jingle-jangle fallacies by which constructs which are, in fact, the same are mistakenly treated as different because they were given separate names in different studies. I refer to this also as *construct balkanization*, in which a single construct is salami-sliced into tinier fragments that are falsely treated as discreet when they are not. This is a widespread problem. As [Anvari et al. \(2025\)](#) noted, thousands of new constructs and measures are published each year, creating considerable construct confusion. This is likely a point of serious rigor problems for our field.

Indeed, I would contend that Dr. Haidt's own work often combines many of the same variables in analyses. For instance, [Twenge, Haidt et al. \(2022\)](#), study one combines self-esteem, depression, life satisfaction, and "strengths and differences" into a single mental health measure. Study 2 uses "general happiness." Study 3 uses severe depression. All three studies were used to make the general conclusion, "... among girls, there is a consistent [AQ6](#) and substantial association between mental health and social media use."

Of course, there are nuanced conversations to be had about constructs in psychology and how they are used in meta-analysis. But data clearly support aggregating these constructs together into singular analyses. Given that, in his own research, Haidt has done the same, I am puzzled by their objections here.

Post Hoc Study and Effect Size Exclusions

In several cases, Rausch and Haidt suggest that studies should not have been included in my meta-analysis. However, a close read demonstrates that, in fact, these studies do meet the preregistered inclusion criteria. In their critique, Rausch and Haidt misquote my preregistration, failing to note the full criteria: "studies must examine time spent on [AQ7](#) social media use, *not other variables such as motivations for use, problematic use, etc.* [\[This should indeed be italicized for emphasis\]](#)" It is clear from this that social media time was used in a way to distinguish it from other possible variables such as problematic use. Indeed, whatever the outcome, I broadly included studies that

had some measure of social media use. This included [Deters and Mehl \(2013\)](#) who instructed their experimental group to put more effort into posting status updates on Facebook which, of course, takes time ([Rausch and Haidt](#) had argued that time was not an element of this study).

[Rausch and Haidt](#) claim [Gajdics and Jagodics \(2022\)](#) lack a control group, but this is simply false.

[Gajdics and Jagodics \(2022\)](#) do indeed have a control (this was a within-subjects design comparing regular vs. mobile-free days), and the authors themselves make the connection between mobile phones and social media use. Indeed, Drs. Haidt and Rausch have often made this connection themselves, tying reduction in smartphones and social media together, implying smartphones are one means by which children often use social media (e.g., [Bhaimiya, 2025](#)). In any event, removing the study would only have modestly changed the effect size ($d = .108$) and would not have changed the meta-analytic conclusions, as this remained well below the preregistered smallest effect size of interest of $d = .21$.

Rausch and Haidt's comments on [Lepp and Barkley \(2022\)](#) are puzzling as they misstate how randomized experiments and control groups work. In their comment, they make comparisons not between social media and the control group (where participants sat quietly, not engaging in any of the experimental activities), but rather two other experimental conditions (exercising, studying), but these were not preregistered comparisons. Indeed, although Rausch and Haidt's narrow read of the preregistration earlier may be defensible, that contrasts with their implication of abandoning it entirely now. Control groups are used to control for, among other things, time effects and, indeed, the social media group fared better over time than the control group. The authors themselves refer to it as a "control" condition approximately 27 times and, indeed, it fits the intent of a control condition in the preregistration. Why else would it be included? Trying to assert it is not "really" a control condition after the fact is a post hoc reasoning. There were no errors made in my analysis of this study. Naturally, we may critique the methods of the study and suggest it was poorly designed overall (which, indeed, was one of the larger points of my original meta-analysis), but it was included consistently with the preregistration.

Rausch and Haidt also suggest that short-term studies were less likely to find results than longer

term studies. They explain that shorter term studies may produce “withdrawal,” though the notion that technology use produces withdrawal is itself controversial at best and not clinically recognized (Panova & Carbonell, 2018). They cite the very constructive and thoughtful comment on my meta-analysis by Thrul et al. (2025). However, they neglect to mention either my reply (Ferguson, 2025) or the independent evaluation of Jané (2024) which both concluded this was not true when time was properly considered as a continuous variable rather than binary split. Furthermore, the study with the longest time length actually was the one providing the weakest evidence for the causal hypothesis. Lastly, as I noted in my reply to Thrul, time length was confounded with researcher expectancy effects, making it difficult to make definitive conclusions about intervention length.

Interestingly, as we’ll see below, none of the three meta-analyses to date (including one Rausch and Haidt cite positively), find that length of intervention matters to effect size.

Naturally, there are interesting debates to be had about study inclusion and exclusion, and meta-analyses using different approaches can certainly stress test hypotheses. However, it is important to note that all inclusion decisions were made *before* I had seen the results of the studies. By contrast, Rausch and Haidt’s comments come *after* they had seen the results.

This raises the concern that post hoc attempts to eliminate studies or revise effect sizes may indicate researcher expectancy effects of the sort likely to cause false positives. Indeed, psychology’s replication crisis was often the outcome of such post hoc decisions. One need not take my word on this. In a debate with Dr. Haidt, Dr. Candice Odgers spoke to this specific issue when my meta-analysis came up. Regarding Dr. Haidt’s post hoc decisions, Dr. Odgers said, “... there’s all kind of risks of doing this [AQ8](#) ... that’s one of the things we should probably not be doing to advance science is going in post hoc and pulling out the effects we think will most support our case.”

As for the corrections, postpublication corrections are a common and expected part of the science process, even when corrections were of trivial magnitude as was the case here, with the exception of one effect size I agreed I had misread. Corrections of this nature involving minor sample size adjustments or trivial adjustments are common in meta-analyses and reflect appropriate and

ethical ongoing scientific refinement rather than substantial error. The corrections did not substantially change the overall effect size or conclusions of the article. At my behest, all effect sizes were audited by an independent statistician (Patrick Markey) who agreed they were trivial in magnitude, and in cooperation with the journal editorial board. The corrected effect size table remains transparently available, and posting two effect sizes tables would appear to be an obvious road to confusion. That there were corrections is transparently noted, despite their mainly trivial nature.

Independent Replications

Many debates about inclusion or exclusion of studies or effect sizes can be reasonable. The question becomes: How does one analysis hold up over time, when other studies take different approaches? In this case, we have the answer: My original meta-analysis has now been replicated twice by independent research groups.

Curiously, Rausch and Haidt cite one of these themselves, implying its results support their position ([Burnell et al., 2025](#)). Although they adopt inclusion criteria arguably closer to those advocated by Rausch and Haidt, their effect sizes were only marginally larger, and, as the authors acknowledge, still below my preregistered smallest effect size of interest (and well below values I had suggested for clinical/practical significance well before the social media debate began ([Ferguson, 2009](#)). Furthermore, they found no evidence that length of intervention mattered. This suggests that, even with inclusion/exclusion decisions more favorable to Rausch and Haidt's hypotheses, effect sizes remain trivial, unreliable and of little clinical value. Indeed, [Burnell et al. \(2025\)](#) concluded, "Although significant, the pooled estimates [AQ9](#) were small in magnitude, suggesting only weak support for the effectiveness of restricting social media use." The authors further conclude,

Based on our findings, we argue that [AQ10](#), although well-intended, calls for blind reduction of social media use may distract from more fruitful action that targets more specific

sources of mental health strife ... We worry that such blanket calls may be potentially dangerous for some individuals, particularly young people. For example, a subset of LGBTQ+ youth report that social media provides an essential avenue for identity exploration, community connection, and social support.

Far from contradicting my meta-analyses, these conclusions appear quite consistent with my own.

Rausch and Haidt also failed to note a second meta-analysis which replicated my own. [Lemahieu et al. \(2025\)](#)'s conclusions were similar to my own. As they stated,

The analyses revealed no significant effects [AQ11](#) of social media abstinence interventions on positive affect, negative affect, or life satisfaction. Relationships between social media abstinence duration and the three outcomes were also non-significant.. The findings thus suggest that temporarily stepping away from social media may not be the most optimal approach to enhance individual well-being, emphasizing the need for further research on alternative disconnection strategies.

Likewise, they found no impact for length of intervention or differences in outcome based in Balkanized terms.

As such, we now have three meta-analyses which, using different methods, all come to effectively the same conclusion: That even taken at face value, the impact of social media abstinence on mental health is likely of limited practical or clinical value.

How the Entire Debate Missed an Important Point

However, this leaves the issue of whether such studies should be taken at face value. The entire unpleasantness described by [Lee \(2024\)](#) essentially focused on nudging effect sizes slightly this way or that, to reach $p < .05$. However, much of my original article focused on the concern that the studies themselves were of limited value. I thought it was interesting to examine whether, taken at face value, experimental studies of social media reduction could support the causal claims often made. Clearly, they cannot. But I also explained that such studies *should not* be taken at face value.

Indeed, in her debate with Dr. Haidt, Dr. Odgers makes this very same point: That we have accepted a low standard of evidence in this field that we would never accept in most others. This is because, in these studies, the intent of the studies and their hypotheses are obvious to participants, and they occur in a sociopolitical environment in which individuals are constantly exposed to the message that social media is bad and abstaining from it should be good. As such, these studies are highly susceptible to demand characteristics and placebo effects. The fundamentally cannot answer the causal question we hope they could.

Nonetheless, the debate about study inclusion, effect sizes, and so on, proceeded largely without any reference to or curiosity about these issues. Certainly, the statistical debates are worthy to have, but they also have the appearance of eclipsing or distracting the more fundamental debates to be had about the limits of research methodology and standards of evidence. Put simply, these designs are ill suited to test the causal hypotheses about social media effects.

Conclusions

We now have three meta-analyses that should all be considered as policymakers weigh the real impact of social media. We must be cautious not to fall into another pattern of moral panic, wherein well-intended policies may do more harm than good. The instinct to protect minors is understandable, but defaulting to unproven policies has real opportunity costs and has been associated with real harms. Experimental evidence, even taken at face value, does not support the effectiveness of policies designed to limit access to social media. Empirical evidence should guide

public policy and public claims, not the reverse.

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Author Query

1.

Query [AQ1] : Kindly confirm the updated running head. Else, kindly provide a running head not exceeding 50 characters, including punctuation and space, without abbreviations. 

Response by Author: "Accepted"
2.

Query [AQ2] : Please link the citation to the corresponding reference in the reference list. 

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Query [AQ3] : Please provide complete mailing address for corresponding author Christopher J. Ferguson, including street address and postal code. 

Response by Author: "just add:421 N. Woodland Blvd"
4.

Query [AQ4] : Please note that “bold text” (emphasis) has been changed to “italics” throughout the text. Kindly check and confirm if okay. 

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5.

Query [AQ5] : Per journal style, factors, scales, subscales, and measures are capitalized and variables, theory, conditions, and models are not. Please check the text and amend wherever necessary. 

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6. **Query [AQ6]** : Please provide source details and page number for the quoted texts “among girls, there is a consistent ...”. 

Response by Author: "Here's the source:

<https://www.sciencedirect.com/science/article/pii/S0001691822000270>

These things are mostly online now and don't have page #s. "

7. **Query [AQ7]** : Please provide source details and page number for the quoted texts “studies must examine time spent on ...”. 

Response by Author: "Same thing, no page #, the source was the preregistration for the original article being discussed."

8. **Query [AQ8]** : Please provide source details and page number for the quoted texts “... there's all kind of risks of doing this ...”. 

Response by Author: "The source is this video: <https://www.youtube.com/watch?v=Ewx4pWOH-l&t=944s>

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9. **Query [AQ9]** : Please provide page number for the quoted text “Although significant, the pooled estimates ...”. 

Response by Author: "the version of the pdf I have just has it on page 1, but again, I think page #s are kind of a thing of the past so I'm not sure that's correct."

10. **Query [AQ10]** : Please provide source details and page number for the quoted 

texts “Based on our findings, we argue that, although ...”.

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11. **Query [AQ11]** : Please provide source details and page number for the quoted texts “The analyses revealed no significant effects ...”.



Response by Author: "same here, pdf does not have page #s. It would be page 1 of the pdf."

12. **Query [AQ12]** : “Hanfstingl et al. 2024; Said 2022; AAE Speakers 2026; and Anxious Generation Movement 2026” are not cited in text. Please add a text citation or delete the reference from the reference list.



Response by Author: "can be deleted."

Comments

1. **Comment by Author:** "This should indeed be italicized for emphasis"



[AUTHOR: FERGUSON - 8/4/2026 7:17:59 PM]