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Do Social Media Experiments Prove a Link With Mental Health: A Methodological and Meta-Analytic Review

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Whether social media influences the mental well-being of users remains controversial. Evidence from correlational and longitudinal studies has been inconsistent, with effect sizes weak at best. However, some commentators are more convinced by experimental studies, wherein experimental groups are asked to refrain from social media use for some length of time, compared to a control group of normal use. This meta-analytic review examines the evidence provided by these studies. All studies, regardless of outcome, have fairly straightforward weaknesses related to demand characteristics. Thus, it is unclear whether these study designs are capable of answering causal questions. Nonetheless, meta-analytic evidence for causal effects was statistically no different than zero. However, remarkable between-study heterogeneity was observed. Studies with citation bias produced higher effect sizes, suggesting a research expectancy effect in some studies. Better designs and closer adherence to open science principles and care not to exaggerate the importance of weak effect sizes may help improve rigor in this field.

Public Policy Relevance Statement

Considerable debate remains regarding whether social media use impacts the mental health of users. Recent commentary has focused on experiments of social media use as particularly valuable use. However, significant methodological limitations in their design may decrease their value to this debate, particularly as participants may easily guess the purpose of such experiments and change their behavior accordingly. Further meta-analytic evidence suggests that, even taken at face value, such experiments provide little evidence for effects. Put very directly, this undermines causal claims by some scholars and politicians that reductions in social media time would improve adolescent mental health. Thus, appeals to social media experiments may have misled more than informed public policy related to technology use.

Keywords: social media, mental health, depression, anxiety

Recent years have seen an increase in debates within both the public and academic spheres regarding the potential impact of social media use on mental health. Arguably, this shift in public concerns is coincidental with a decline in concern regarding video games and violence, as evidence emerged to suggest violent video games were unrelated to youth aggression (e.g., Bowman, 2016; Drummond et al., 2020, see also Orben, 2020). In this sense, it is possible to visualize an ongoing cycle of technological moral panics, with fear simply moving from one source to the next as each is discredited in turn. Nonetheless, this does not mean that hypotheses linking social media to mental health are unreasonable. This issue has been debated in government policy arms (e.g., House of Commons Science and Technology Select Committee, 2019; UK Parliament, 2023; US Department of Health and Human Services, 2023) and resulted in statements by professional guilds such as the American Psychological Association (2023). Nonetheless, vigorous debate has continued among scholars on this issue (e.g., Odgers & Jensen, 2020; Orben & Przybylski, 2019; Twenge, 2020). Meta-analyses of social media effects on mental health have, likewise, returned mixed results with generally weak effect sizes (e.g., Cunningham et al., 2021; Ferguson et al., 2022), and narrative reviews have similarly come to no consensus (e.g., Hall et al., 2021; Kaye, 2022). Experimental studies of potential effects are AQ3 comparatively few and may be limited by significant methodological issues (Satchell et al., 2021, see also Want, 2014 for relevant discus- AQ4 sion of methodological issues for experiments in a similar research domain). Nonetheless, some commentators have found them to be convincing (e.g., Hanania, 2023; Smith, 2023), often rejecting evidence from correlational and longitudinal studies in favor of experiments. With this article, a methodological and meta-analytic review of experimental studies of social media and mental health will be conducted to examine whether such confidence is warranted.

How Are Social Media Experiments Conducted? AQ5

Pretty much all experiments of social media effects on mental health take a similar form. Participants, adults in most cases, are

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The author declares no conflicts of interest. A preregistered plan for this study can be found at: https://osf.io/yntse. Data are available at: https://osf.io/jcha2. A list of included studies can be found at: https://osf.io/27dx6.

Christopher J. Ferguson served as lead for conceptualization, data curation, formal analysis, project administration, and writing–original draft.

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randomized to one of two conditions. In the control condition, participants are invited to continue using social media as usual. In the experimental condition, participants are asked to either reduce or eliminate social media use. Studies vary on whether this abstention is long term in real life (e.g., Brailovskaia et al., 2023) or short term in a laboratory environment (e.g., Yuen et al., 2019). Some studies may also include other randomized groups, such as groups asked to exercise, or manipulate passive and active use of social media, but all experiments follow the same basic design.

Studies using this design almost all have some straightforward benefits. Namely, most such studies use well-validated and standardized measures of mental health and employ some degree of plausible control group (i.e., use vs. nonuse of social media). Such studies also experience some fairly routine limitations which are quite serious as well, and it is to this issue this article next turns.

Pitfalls of Social Media Experiments

Arguably the most profound limitation of current social media experiments is central to their design: participants must be asked to refrain from using social media, then at some further date, are asked about their mental health. Given the very visible nature of debates regarding social media and mental health, it is likely that many participants will be able to guess the hypotheses of these studies. This issue of demand characteristics (Orne, 1962) has been well understood for decades, yet it remains an evident and consistent problem for pretty much all experiments in this field, given they share this same basic design. There are potential ways to address demand characteristics. Hypotheses may be disguised, distractor tasks can make hypotheses less evidence, and careful screening of participants can eliminate those who guess the study hypothesis. However, such approaches appear to be fairly rare in this field, likely leading to considerable overconfidence among scholars that experimental results are "real" as opposed to a consequence of hypothesis guessing.

A second problem arises from the potential for publication bias and questionable researcher practices. It is important to note that some studies do use open science principles such as making data transparent and preregistering hypotheses prior to data collection and should be commended accordingly (e.g., Hall et al., 2021; Mitev et al., 2021). However, such studies are rare. This means that scholars in this field may have considerable freedom to interject their own opinions into the data, either consciously or unconsciously nudging the data to better fit study hypotheses in ways that create false positive results. Such researcher expectancy effects may be identified through the presence of citation bias in literature reviews. That is to say, authors who cite only studies supporting their hypotheses fail to support studies that do not and, as such, present the reader with a distorted picture of the research field. Citation bias has been found to be associated with spuriously high effect sizes in other fields such as video game violence (Drummond et al., 2020).

The third issue relates to effect sizes. Some studies involve relatively high sample sizes and interpret the "statistical significance" of very weak effect sizes as hypothesis-supportive. However, recent scholarship has found that effect sizes below $r = .10$ have a very high false positive rate and are, in essence, indistinguishable from statistical noise (Ferguson & Heene, 2021). This occurs due to a fluke of large samples in null hypothesis significance testing. In very large samples, almost all associations become "statistically significant" even if due to noise effects. This problem has, again, been recognized for decades (e.g., Cohen, 1994; Wilkinson, & Task Force on Statistical Inference, American Psychological Association, Science Directorate, 1999), yet continues to cause misinformation, typically resulting in overconfidence in hypothesis support. One means of addressing this is to adopt a smallest effect size of interest (SESOI). As discussed above, it may be reasonable to suggest $r = .10$ for most social science research, as the methodological precision of social science studies are likely unable to distinguish true effects from methodological noise below this value. However, higher values may also be reasonable to demonstrate clinical significance.

A fourth potential issue is selective dropout. It is possible that participants asked to reduce social media time may drop out of the study should they find these instructions to be unpleasant. This can create response bias, wherein only participants who enjoyed reducing social media time remain at posttest to indicate mental wellness, artificially inflating wellness scores posttest for the experimental group. Some studies (e.g., Faulhaber et al., 2023) have unusually lopsided participant figures for the experimental and control conditions that are unlikely from random assignment, suggesting that unreported dropout might be a factor in these studies.

Finally, it is worth noting that experiments may benefit from a self-fulfilling prophesy. Given the plethora of news coverage of the issue of whether social media is linked to mental health, it is likely many participants may believe news stories that abstinence should promote mental health. This may cause respondents to behave in ways consistent with this hypothesis, particularly in face-obvious studies of social media abstinence. Assessing for expectations, hypothesis guessing as well as measuring other behavioral changes associated with well-being may help reduce this potential biasing effect.

The Current Analysis

Although some commentators have expressed valuing social media experiments as a means of demonstrating cause and effect with participant mental wellness, it remains unclear whether such studies are sufficient for such conclusions. As of yet, meta-analysis has not specifically considered this issue. As such, this meta-analysis will test the hypothesis that experimental manipulation of social media exposure is associated with improved mental wellness among participants in reduced social media time experiments. This meta-analysis will consider experiments only, thus distinguishing them from correlational and longitudinal studies. Furthermore, this analysis will use a SESOI of $r = .10$ as discussed above to reduce the potential for over-interpretation of noise-related results. This meta-analysis will also examine whether better quality studies as well as citation bias act as moderators of study effect size.

Method

Open Science Practices

A preregistered plan for this study can be found at https://osf.io/yntse. Raw data for the meta-analysis, including all studies and effect sizes, are located at https://osf.io/jcha2. A list of included studies can be found at https://osf.io/27dx6.

Selection of Studies

A search was conducted on PsycINFO and Medline using the terms ("Social Media" OR "Facebook" OR "Instagram" OR "Twitter" OR "snapchat" OR "social networking" Or "TikTok") AND ("depression" OR "anxiety" OR "loneliness" OR "suicide" OR "mental health" OR "mental well*" OR "mental illness" OR "mental well-being" OR "psychological well-being") AND experiment* as subject searches. Studies identified by previous commentators (e.g., Hanania, 2023; Smith, 2023) as this was valuable locating studies in other fields such as economics.

To assess the relevance of studies, we identified that they should meet the following inclusion criteria: include an experimental comparison of social media with a control condition,[1] and studies must examine time spent on social media use, not other variables such as motivations for use, problematic use, etc. The studies must include enough information to calculate an effect size *d*. Only main effects or Time × Condition effects for pre/post designs will be included, not moderator effects. Ultimately, 27 studies, including two dissertations, were found that met the inclusion criteria.

Effect Size Extraction and Calculation

Effect sizes were calculated using the mean differences between experimental and control conditions to produce an effect size Cohen's *d*. Where means and standard deviations were not presented, *d* was calculated from *t* tests or *F* statistics. In places where the available data in a study were not sufficient, requests for data were made from study authors. For studies reporting multiple outcome measures, these were averaged together.

Jamovi[2] was used to calculate a random-effects mean effect size, as well as to calculate risks of publication bias including basic funnel plot analysis, Egger's regression, and trim and fill. Random-effects models were used. Given the high power of meta-analysis, almost all meta-analyses are "statistically significant." Nonetheless, many small effects may be statistical artifacts due to methodological issues such as demand characteristics or single responder bias. Consistent with the recommendations of Orben and Przybylski (2019), we considered an effect size of $r = .10$ (approximately $d = 0.21$) as the minimum for practical significance.

Best Practice Analysis

To analyze the prevalence of best practices adopted within the literature and test whether this moderated the observed effect sizes, we utilized the following criteria, from which a numeric score could be calculated and used in moderation analysis.

Experimental studies were given credit (one point each) for the following best practices:

1. Used a standardized outcome measure. This involves measures with clear administrative rules, reducing researcher degrees of freedom. Measures are administrated in the same way to all participants across studies.
2. Used a clinically validated measure. This indicates measures with a research base demonstrating utility in clinical diagnosis.
3. Used a closely matched control condition differing only in independent variable-related content. It is likely that the control condition differs from the experimental condition only in the independent variable of interest.
4. Used distractor tasks to reduce demand characteristics. This may include tasks or questionnaires that are not relevant to the hypotheses and are not similar to either the independent or dependent variables.
5. Included queries for hypothesis guessing. Attempts are made to assess for demand characteristics.
6. Preregistered the analysis plan.

This allowed us to calculate a score that could be tested for potential moderator effects with effect size. Such a score will allow us to examine whether study quality was associated with either increased or decreased effect size, thus allowing us to understand how methodological noise might impact results.

Citation Bias Analysis

Citation bias occurs when study authors only cite articles supporting their hypotheses, failing to inform readers of inconsistencies in a research field. Such bias may be an indication of researcher expectancy effects that may spuriously influence effect sizes. In cases where the literature review included no citations that conflicted with the authors' hypotheses, they were coded as having citation bias. However, if an article acknowledged at least one research study or article conflicting with the authors' hypotheses, they were not coded as having bias.

Moderator Analysis

Several moderators were considered as part of this study. First, as indicated above, both best practices and the presence of citation bias were considered moderators. Second, several moderators were included that were not preregistered. These included whether the experiment used some means of verifying participants' social media time or if self-report was used, as well as the ratio of the experimental group to the control group as a possible indication of selective dropout. This latter analysis will help to examine whether selective dropout might be a factor in artificially high effect sizes. Third, the age of the study's participants was considered possible moderators, as was the study year.

Results

Table 1 presents the results of all analyses. Figure 1 presents a funnel plot for all studies. As can be seen the overall estimate for *d* across studies was 0.088, which was nonsignificant and well below the SESOI ($r = .10$, $d = 0.21$). However, this masked a significant amount of heterogeneity between studies. Specifically, effect sizes varied considerably between studies meaning that the mean effect size is unlikely to be representative of population means. Indeed, effect sizes across studies ranged from those that moderately supported the harm hypothesis ($d = 0.797$) to those more modestly supporting that social media was beneficial ($d = -0.365$), albeit with most effect sizes below the evidentiary threshold ($d = 0.21$) in either direction.

[1] To reduce noise in the data, we focused on time spent on social media and so excluded studies which measured motivations for using social media, or for what purposes social media was used and studies measuring "problematic social media use."

[2] This does represent a slight deviation from our preregistration which mentioned using Comprehensive Meta Analysis and Shinyapps for calculations. We have switched to jamovi during this time, which was unrelated to the results of the meta-analysis.

Table 1
Meta-Analytic Results of Social Media and Mental Health Outcomes

Random-effects model ($k = 27$)						
Estimate (d)	Z	p	CI lower bound	CI upper bound		
0.088	1.63	.104	−0.018	0.197		
Heterogeneity statistics						
Tau	Tau^2	I^2	H^2	df	Q	p
0.114	0.013 ($SE = 0.0053$)	75.2%	4.025	26.000	91.64	<.001

Note. Tau^2 estimator: maximum-likelihood. For the purposes of analysis in jamovi, values d were converted to r and then converted back again. CI = confidence interval.

In regard to publication bias, the results for both Begg and Mazumdar's ($r = -.13$, $p = .337$) and Egger's ($r = -.51$, $p = .610$) regressions suggested an absence of publication bias, albeit low power may have made bias difficult to find. Trim and fill suggested there might be two missing unpublished studies, but given an inconsistency in these outcomes, it is most conservative to interpret little evidence for publication bias.

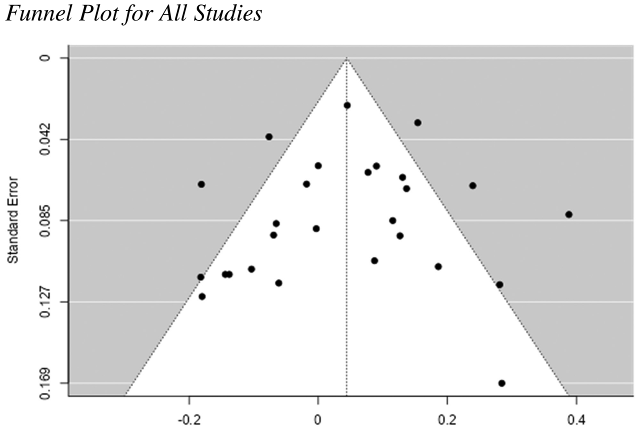
For continuous moderators, participant age ($Z = 2.25$, $p = .024$) but not study year ($Z = 0.364$, $p = .716$) were associated with higher effect sizes. Although best practice studies were observed to generally have lower effect sizes than did nonbest practices studies this did not achieve statistical significance ($Z = -1.85$, $p = .065$). The citation bias moderator did achieve significance ($Z = -2.00$, $p = .045$). For exploratory moderator variables, whether social media time had been verified or not did not predict effect sizes ($Z = -1.03$, $p = .301$) nor did the ratio indicating potential dropout ($Z = -0.092$, $p = .927$). It is worth noting that the small number of studies may have underpowered the moderator analyses, and issues related to both best practices and citation bias may be worthy of further study.

In exploratory analysis, Cook's distances identified Lambert et al. (2022) as a potential effect size outlier. Using a "leave one out" analysis, the effect size without this study would be $d = 0.064$.

Discussion

Concerns continue to abound regarding the potential impacts of social media on mental health of users of this technology. Recent commentary has sometimes focused on experiments of social media use as potentially convincing evidence for effects (e.g., Hanania, 2023; Smith, 2023). The current analysis suggests that this confidence is misplaced. Across experiments, the meta-analytic estimate of effects does not statistically differ from zero. Although this masks considerable heterogeneity between studies, it is also likely that the basic study design is insufficient to answer questions about social media effects.

Figure 1
Funnel Plot for All Studies



All extant studies use some variation on a basic approach, namely, asking participants to reduce or eliminate social media time if in an experimental condition. Unfortunately, particularly given the very visible public debate, this makes demand characteristics fairly obvious, introducing hypothesis guessing confounds. With that in mind, it's actually surprising that so many studies don't find ostensible evidence for an effect.

It's possible that better designs might reduce demand characteristics. For instance, scholars might include more distractor tasks to make hypotheses of the study less obvious. Researchers could also do a more effective job in querying for hypothesis guessing and eliminating participants who were able to guess study hypotheses. However, it may also simply be that such a blunt design as commonly used in these experiments is inappropriate for the task at hand.

Unfortunately, testing for design issues as moderators—a best practices analysis—was hampered by a fairly high degree of methodological uniformity between studies. On the positive side, most studies employed standardized, validated measures and included reasonable control groups. On the negative side, few studies used distractor tasks, queried sufficiently for hypothesis guessing or preregistered their studies. Although best-practices analyses were nonsignificant for the current meta-analysis, this may reflect both lack of variance between studies on study quality and low power for the analysis.

Citation bias did prove to be a significant moderator of effect size, with studies with citation bias producing higher effect sizes. This is one indication that researcher expectancy effects may be having some influence on studies in this realm. Greater use of open science principles such as preregistration and open data may help reduce this issue.

Although the overall meta-analytic effect size was nonsignificant, it is worth remarking on the sheer heterogeneity of effect sizes between studies. Although many studies had near-zero effect sizes, others were quite large, both positive and negative in direction. Despite having conducted numerous prior meta-analyses, this author cannot remember seeing such between-study heterogeneity in results in other fields and is at a loss to explain this. Nonetheless, other authors have communicated that large heterogeneity is common in media and communication studies (e.g., Levine & Weber, 2020), so this may be a wider problem for numerous research fields that warrants caution on the overinterpretation of mean effect sizes in meta-analysis, particularly as purported debate enders.

One possible explanation for the heterogeneity is that studies often examined widely different social media platforms. These may create very different experiences with potentially different effects. However, it's also plausible that differences in methodology between studies may produce significant heterogeneity. Of course, these two explanations are not mutually exclusive.

Is it possible that small effects may be meaningful if extended over a large population? This is a common rationalization of small effect sizes, but the simple answer is "no." This is for two reasons. First, effect sizes are an estimate of a magnitude of impact for the mean individual. They are not meant to be sprinkled as with pixie dust over a population, implying that some proportion of said population will experience clinically significant effects. Second, and perhaps of greater critical importance, it's now well established that social science simply lacks the precision, for small effect, to distinguish signal from noise. This means there is simply no reason to believe that such small effects are real as opposed to due simply to methodological noise. Thus, it is most conservative to discard them as hypothesis-supportive, even if "statistically significant" in large sample studies or meta-analyses (Ferguson & Heene, 2021).

If the current approach to examining social media effects experimentally is insufficient, a better mousetrap, such as it is, may not be evident. There may be no clear way to randomize people to social media conditions without making hypothesis guessing a critical confound. As such, our consideration of this issue may have to prioritize correlational and longitudinal findings.

Limitations

As with all studies, this one may have limitations. The current search strategy employed subject search terms and it is possible that this may have resulted in some missed articles that did not show up under a subject search. Given the relatively few studies included, both publication bias and moderator analyses may have suffered from reduced power. Although the best practices analysis considered several issues commonly explored in other best practice analyses, it is possible that other methodological issues were not considered. Although dependent variables were all under the umbrella of "mental health" and most were standardized and well validated, they nonetheless varied considerably. Measures ranged from clearly clinical measures of depression and anxiety to more general measures of well-being and self-esteem, often within the same articles. This may also introduce significant heterogeneity.

Better Studies

Given widespread problems with experiments in this area, particularly related to hypothesis guessing, it is reasonable to ask what better quality experiments may look like. Given the primary issue of demand characteristics, experiments which reduce these are tantamount. One possibility is embedding both the independent and dependent variables within a plethora of other, irrelevant, tasks. For instance, the social media reduction IV might be embedded within a host of other ostensibly healthy practices such as increasing exercise, changing diet, personal meditation, and so forth. The experimental and control group would only differ regarding whether social media was added to these other life changes. Any tracking measures would track all of these life changes, not only screen time. Likewise, questionnaires regarding mental health could be embedded among other surveys having nothing remotely to do with the study hypotheses. This, of course, places a larger burden on participants but may be worth considering with adequate IRB oversight (to be sure there are no health concerns with other life changes) and compensation for the time investment by participants. Studies should also clearly debrief participants and make debriefing procedures clear in published articles. I generally advocate that researchers directly ask participants, "If you had to guess the hypothesis of this study, what would you guess?" as this tends to produce more responses than merely asking if participants guessed the study hypothesis. Any responses remotely involving social media and mental health should result in participants being discarded from the analysis. Lastly, open science principles of preregistration and open data should be used for all studies.

It may be worth considering, as well, that time spent on social media may simply be a poor measure of potential effects, whether in experimental or longitudinal studies. Some prior research had examined processes, or how people used social media as potential issues. For instance, using social media for, effectively, self-promotion tended to be associated with positive outcomes (Reinecke & Trepte, 2014) whereas ruminating over deficiencies is associated with more negative outcomes (Davila et al., 2012). However, this degree of nuance appears to have been lost more recently in the rush to blame social media for teen suicide.

Conclusions

Currently, experimental studies should not be used to support the conclusion that social media use is associated with mental health. Taken at surface value, mean effect sizes are no different from zero. Put very directly, this undermines causal claims by some scholars (e.g., Haidt, 2020; Twenge, 2020) that reductions in social media time would improve adolescent mental health. However, mean effect sizes aside, there are reasons to suspect that the methodology of most such studies is simply not up to the task. Use of preregistration and other open science principles, and a greater reliance on good longitudinal designs may be more effective.

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