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There Is No Evidence That Time Spent on Social Media Is Correlated With Adolescent Mental Health Problems: Findings From a Meta-analysis

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The issue of whether social media use does or does not influence youth internalizing mental health disorders (e.g., anxiety, depression) remains a pressing concern for policymakers, parents, and psychologists. Widespread claims suggest potentially harmful effects of social media use on youth. This was investigated in a meta-analysis of 46 studies of youth social media use and mental health. Results indicated that the current pool of research is unable to support claims of harmful effects for social media use on youth internalizing disorders. Some types of methodological weaknesses, such as evident demand characteristics and lack of preregistration, remain common in this area. It is recommended that caution is issued when attributing mental health harm to social media use as the current evidence cannot support this.

Public Significance Statement
Policymakers, parents and health care professionals continue to worry whether social media use contributes to mental health problems among youth. The present study finds that the evidence for such beliefs is lacking, and social media use does not predict mental health problems in youth. It is not unreasonable for parents to ask questions about children's social media use, however at present, parents may be misled by unsupportable rhetoric from policymakers and some professional guilds to believe that the evidence for harm is greater than it is. Policymakers and professional guilds need to adopt more cautious reporting standards when discussing social concerns for which evidence is weak.

Keywords: social media, youth, adolescents, mental health, suicide

Concerns about young people's mental health have become increasingly prevalent over the last decade. In response, commentators have been prone to attribute the reasons for this increase to technological innovations that occurred concurrently during the same period, such as social media. The tendency to blame perceived social ills on new technology is often associated with moral panic (Orben, 2020). Moral panic refers to significant societal fear that takes place around a perceived threat and a specific responsible agent or technology—but where the threat is exaggerated or misplaced (S. Cohen, 2011). Moral panics have been witnessed in relation to the introduction of radio, television, and video games (Orben, 2020).

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Data are available on Open Science Framework and can be accessed at https://osf.io/hv7us/. Linda K. Kaye was named a contributor to "An Open Letter to Mr. Mark Zuckerberg: A Global Call to Act Now on Child and Adolescent Mental Health Science" (https://www.oii.ox.ac.uk/an-open-letter-to-mark-zuckerberg/) published in December 2021. She has also contributed to proceedings of the U.K. Government Online Safety Bill (https://bills.parliament.uk/bills/3137).
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A prominent sociotechnological innovation that dominated the last decade was the advancement of social communicational technology, specifically the proliferation of social media sites into our daily lives (Ofcom, 2022). This has led to the suggestion that social media may sometimes be used as a "scapegoat" to mask more complex explanations for poor mental health in youth (Sharp, 2021). Relatedly, there has been a significant public and academic debate regarding the role of social media use on mental health, especially in relation to children and young people. Of course, a historical pattern of focusing blame on technology (see Bowman, 2016) does not necessarily mean that the current technology, social media, does not have an actual pernicious effect on youth mental health. It is the hope that rigorous science can help policymakers and parents answer this important question.

Discussion surrounding new technologies and mental health is commonplace in both public and academic debate and underpins numerous health and social policy agendas (House of Commons Science and Technology Select Committee, 2019; U.K. Parliament, 2023; US Department of Health and Human Services, 2023).[1] Specifically concerning academic debate, the issue of social media use and mental health remains polarized. While some researchers contend that social media presents a significant public health risk to children and young people (e.g., Alonzo et al., 2021; Brailovskaia et al., 2023; Haidt, 2020; Keles et al., 2020; Twenge & Campbell, 2019; Twenge et al., 2018), others express caution regarding the quality and consistency of the available evidence and argue that there is limited scope to establish the causality or universality of mental health outcomes associated with social media use and indeed screen use more generally (e.g., Ferguson et al., 2022; Kaye et al., 2020; Odgers & Jensen, 2020; Orben et al., 2019, 2022; Valkenberg et al., 2022; Vuorre et al., 2021). Within these discussions, the literature broadly tends to focus on mental health variables such as depressive symptoms (Cunningham et al., 2021; (Keles et al., 2019)), anxiety ((Keles et al., 2019)), loneliness (O'Day & Heimberg, 2021), self-esteem ((Rounsefell et al., 2020)), body image and disordered eating (Marks et al., 2020), and suicide ideation (Twenge, 2020), and these often exist alongside related discussion about physical well-being effects, such as sleep quality and quantity (Alonzo et al., 2021) and physical activity engagement (Brailovskaia et al., 2023; Shimoga et al., 2019).

Methodological Concerns

This topic is notoriously difficult to navigate when drawing conclusions about how social media use might relate to mental health. This is largely because the literature presents mixed findings and highly disparate perspectives. We note that the existing literature on social media use and mental health is limited by several known conceptual and methodological shortcomings. These fall into several specific areas. First, measures of social media use often tend to focus solely on time spent on social media rather than *how* time is spent on social media as a way to conceptualize social media use. That is, scholars have yet to provide any consistent measurement (or indeed conceptualization) about what "social media use" might actually mean (Trifiro & Gerson, 2019). Second, studies often rely upon self-report, which can be notoriously unreliable as a measure of any type of screen use (Mahalingham et al., 2023). Third, studies rarely consider differences across platforms and overrely on vague concepts of "addiction" that remain controversial (Satchell et al., 2021). Last, there are well-known issues regarding best research practices that can falsely inflate effect sizes (Drummond et al., 2020).

Issue 1: Time Spent Using as a Measure of Social Media Exposure

There have been numerous theoretical explanations for why social media use might relate to mental (and physical) health variables. Many of these tend toward the notion of time or volume being the root of the explanation, in which time spent using social media is time displaced from engaging in other, presumably healthier, pursuits (see recent review by Hall & Liu, 2022). While this makes some conceptual sense for physical health variables such as sleep quantity (i.e., we cannot use social media while concurrently being asleep), it is perhaps less clear how this provides explanatory value for mental health variables. However, despite this, it is commonplace for research in this area (and the "screen-time" topic more generally) to obtain measures of volume as the primary, and often sole measure, of social media use. A recent meta-analysis has illustrated that when controlling for other relevant factors (e.g., best practice in research, age, family environment), the "blunt" instrument of amount of screen time was not found to be significantly related to mental health variables for young people (Ferguson et al., 2022); it is noted that this analysis examined screen-time broadly and including young adults, rather than social media use specifically for adolescents. The current meta-analysis differs in several important ways. First, the time frame is longer (2012–2022). Second, the 2022 meta-analysis focused on screen-time broadly, and some critiques offered suggested that it was less focused on youth and social media use specifically. This meta-analysis focuses specifically on social media use. Third, this meta-analysis focuses specifically on youth aged 18 and younger. There was an overlap of seven studies (15%) with a previous study of screen time more broadly (Ferguson et al., 2022).

We propose that it makes little sense to use blunt measures of screen time as a predictor for mental health and that any research in this space needs to be more nuanced in nature—in particular paying attention to the type of usage (e.g., content viewed, type of engagement) and how individual differences may influence potential impacts for users.

Issue 2: The Limits of Self-Report

As noted, many researchers adopt measures of social media volume—how they do so is often based mainly on their own interpretation due to the lack of consistent, validated measures. Researchers typically request users' time or frequency of social media use based on an average estimate or retrospectively from a recent time-frame reference point. This typically involves asking questions such as: "How many hours did you spend on social media last week?" However, previous research has shown that the reference point used for this brings about discrepancies in the accuracy of these estimates compared with objective log data or screen-time data (Ernala et al., 2020; Parry et al., 2021). Namely, Ernala et al. (2020) found that the reference point used when asking people to report their Facebook use

[1] We focus our review of policy on the United States and the United Kingdom, as we are most familiar with these, recognizing that other countries may follow myriad other policy approaches. We also note here that are our literature review highlights key studies and is not intended to be a systematic literature review.

made a difference in how accurate people were in their reports. People were found to be most accurate (albeit still not to an acceptable degree) when asked to report how much time per day, on average, they use Facebook when multiple-choice options were available for them to select. As such, when the research literature has no consistent measurement for social media volume, discrepancies in findings are likely to be attributed to measurement inconsistencies.

Related to this is the concern that self-report may be unreliable, owing both to poor memory and perhaps the social desirability of underreporting sedentary—or otherwise stigmatized—screen activities. For instance, Prince et al. (2020) found that self-report measures tend to perform poorly, in comparison to time diaries or devices that directly measure screen use or other sedentary activities. In fact, a recent study by Mahalingham et al. (2023) found *no* relationship at all between self-reported social media use and objective usage measures. It is possible, as such, that self-report studies may be particularly prone to returning inaccurate and/or biased results.

Issue 3: Failure to Distinguish Between Platforms and Misuse of the "Addiction" Framework

A further issue is that while self-report tools have been used to measure aspects of social media (e.g., Jenkins-Guarnieri et al., 2013; van den Eijnden et al., 2016), use can vary across platforms. Unfortunately, most researchers either do not distinguish between platforms in measurement tools or might focus on use in respect to only specific platforms (e.g., Facebook), which are unlikely to apply or be consistent with other platforms. To further confound the area, a disproportionate amount of research tends to adopt an "addiction" or "problematic use" perspective of social media, whereby measurement relates to users' attitudes about their (problematic) social media use. Equally, these types of instruments have brought about debate, specifically regarding their validity (Satchell et al., 2021), accuracy (H. Shaw et al., 2020), and researchers' inconsistent use of scoring methods (Connolly et al., 2021). These issues are not unique to social media research but also plague the wider topic of behavioral addictions (Billieux et al., 2015).

Issue 4: Best Research Practices

Across media studies some poor practices have been identified that are prone to inflating effect sizes. For instance, the use of unstandardized and poorly validated measures can increase effect sizes (Drummond et al., 2020). It is also important to control for theoretically relevant control variables such as age, gender, and, in longitudinal studies, Time 1 outcome variable. For screen-time measures, using the same respondent for the predictor and outcome variable, and close pairing of questions related to the predictor and outcome, can create demand characteristics and artificially inflate effect sizes (Ferguson et al., 2022). Thus, for social media studies, it can be worth considering whether these issues impact effect sizes in empirical studies. One recommendation is to preregister studies to help reduce concerns about artificially inflated effect sizes due to questionable researcher practices (Orben & Przybylski, 2019); however, despite being widely encouraged as a good practice, at the moment, the majority of studies are not preregistered.

Some of these issues, particularly Issues 1 and 2, arguably fall under concerns related to constructs and construct validity (e.g., De Boeck et al., 2023). Both issues related to social media use and mental health concerns inevitably incorporate a wide range of elements. Both the constructs of social media use and mental health incorporate heterogeneous components, and some caution is warranted in discussing them as if they were unitary constructs with clear boundaries.

The Present Study

Moral panic theory (Bowman, 2016; S. Cohen, 2011) posits that, among other stakeholders, social sciences often play a critical role in enforcing the panic, particularly during early stages. This may lead to selective attention to studies that support the panic and incuriosity or dismissal of studies that do not. It can also lead to overstatement of weak effect sizes or misapplication of poor-quality studies to real-life phenomena. Furthermore, it may lead to the poor use of meta-analysis, for example, where there is an overreliance on bivariate correlations rather than controlled standardized regression coefficients. An "average effect size wins" approach may effectively put a thumb on the scale in favor of the panic hypothesis.

Clearly, there are critical conceptual and methodological issues that persist and will continue to adversely affect the quality of scientific evidence available on the associations between social media use and mental health. It is important to note that evidence of social media effects on adverse mental health does not necessarily mean that positive effects cannot exist (e.g., social support; Branley & Covey, 2017; Sendra et al., 2020) and vice versa. We are sympathetic to some concerns relating to social media (e.g., age appropriateness, profit-driven attention economy, use of data and algorithms, negative and extreme content). There may indeed be harms that exist, but we remain vigilant to the nature of the scientific evidence that explores these. Of primary interest, we are committed to the need for *high-quality* science to explore relevant concerns, particularly regarding mental health and well-being impacts. Further, we are also committed to the need for this high-quality science to be the basis for informing policy guidelines and social technology innovation surrounding mental health and social media use. Our concerns are that current policy priorities are being influenced by an evidence base that primarily consist of studies with significant methodological limitations or failings. As such, our meta-analysis sought to establish the nature of the existing literature base with two overarching aims:

1. To establish the effect size between social media use and mental health variables in adolescents. As observed, during a period of moral panic, a potential arises that public discourse may differ from the magnitude of effect observed in published studies. Having a clearer understanding of that magnitude of effect may help inform public discourse and policymaking.

2. To establish the prevalence of best practice in studies in the existing literature and their impact on observed effect sizes. As observed in our literature review, methodological concerns ranging from overreliance on bivariate effects, self-report surveys, and so forth may impact effect sizes leading to over- or underconfidence in results.

Method

Open Science Practices

A preregistered plan was completed, which outlined the search strategy, criteria for inclusion and exclusion, and analysis plan. This can be accessed at https://osf.io/9kd4x. Data generated from the meta-analysis can be accessed at https://osf.io/gjt84. This includes the following information: full citation, sample size, effect size, best research practices analyses, and moderator variables. A PRISMA diagram for our search is available at https://osf.io/yaedq. A list of included studies can be accessed at https://osf.io/kx486.

Selection of Studies

We searched on APA 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 ("youth" or "teen*" or "adoles*") as subject searches. We limited our search to studies from 10 years old to the date of the search. The search took place in September 2022.

To enable us to assess the relevance of studies, we identified that they should meet the following inclusion criteria: include a measure of social media or experimental comparison of social media with a control condition,[2] have a sample only including participants between the ages of 12 and 18 and include sufficient information from which we could calculate an effect size "*r*."[3] We note a few samples with age ranges that were slightly younger ($n = 5$). Deviating slightly from the preregistration, we decided to retain these, but this decision was made prior to examining the data.

Regarding social media time, most studies included self-report surveys of time spent on social media, time diaries, or electronic recording systems. Most studies focused on platforms ranging from Facebook, Twitter, Instagram, Snapchat, and so forth, although some also included video or streaming platforms such as YouTube or Twitch.

Regarding outcomes, most outcomes were self-report indices of internalizing symptoms. These included inventories of depression and anxiety but also mental wellness, self-esteem, and satisfaction with life more broadly. Few studies employed clinical cutoffs or official diagnoses, so it may be best to think of the outcome as a general cluster of mental wellness rather than clinical disorders.

A PRISMA chart for our search is provided at the link in the Open Science Practices section. Our search ultimately netted 55 studies on social media use and youth mental health. However, nine were subsequently found to be missing important data needed to calculate an effect size, and either authors did not respond to requests for data or we were that informed the data were unavailable. This resulted in a final pool of 46 studies. Between them, allowing for different effects for boys and girls in some studies, these articles included 79 total effect sizes.

Effect Size Extraction and Calculation

Two authors extracted the effect sizes from each article from which we calculated interrater reliability. Effect size was calculated as a standardized regression coefficient (betas) based on the most conservative value (i.e., employing the most theoretically relevant control variables) available in each study or effects based on experimental results (*F* value, *t* test, etc.). Raw interrater correlations (*r*) between the recorded effect sizes was = .99. Kappa reliability for absolute agreement was .78.

Jamovi[4] 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, trim and fill, p-curve, and p-uniform. We used a restricted maximum-likelihood model with Fisher's *r*-to-*z* transformation. 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 $r = .10$ as the minimum for practical significance. Ferguson and Heene (2021) also provided documentation for how effects below this threshold are unable to be distinguished from noise due to methodological precision issues.

Best Research Practice Analysis

To analyze the prevalence of best research practices adopted within the literature and test whether this moderated the observed effect sizes, we utilized the following criteria (based on the criteria used by Ferguson et al., 2022), from which a numeric score could be calculated and used in moderation analysis.

Correlational/cross-sectional studies were given credit (1 point each) for the following best research practices:

- used a standardized outcome measure
- used clinically validated measures (e.g., Child Behavior Checklist) and, for social media use, those that were objective rather than subjective measures (e.g., screen-time apps or log data, time diaries being superior to estimates of use based on self-report.)
- used more than one type of respondent (e.g., parent and child)
- included distractor questionnaires to reduce demand characteristics
- controlled gender, age, and family environment (family conflict, stress, academic pressure from parents). For longitudinal studies, Time 1/baseline/pretest outcome variable was also controlled.
- preregistered the analysis plan

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

[3] "*r*" is used here to denote the most controlled/conservative effect size from each study, which in most cases (but not all) were standardized regression coefficients.

[4] This does represent a slight deviation from our preregistration that 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.

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

- used a standardized outcome measure
- used a clinically validated measure
- used a closely matched control condition differing only in independent variable-related content
- used distractor tasks to reduce demand characteristics
- included queries for hypothesis guessing
- preregistered the analysis plan

This allowed us to calculate a score that could be tested for potential moderator effects with effect size. This score allowed us to examine whether study quality was associated with either increased or decreased effect size, thus allowing us to understand how methodological noise might be impacting research 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 area. Previous meta-analyses have often identified citation bias as a predictor of inflated effect sizes, suggesting that citation bias may be one indicator of researcher expectancy effects (e.g., Ferguson, 2015). In cases where the literature review included no citations that conflicted with the authors' hypotheses, they were coded as having citation bias. However, if a 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 research practices and the presence of citation bias were considered moderators. Second, some data sets, such as "Monitoring the Future," have produced multiple articles, often from different author groups and sometimes giving conflicting results. Reusing such data sets across multiple articles may give undue weight to the methods in those studies. In the present study, we sought to address this by considering the data set as a potential moderator, particularly examining whether effect sizes differed between large multiuse national data sets versus bespoke data sets used in some individual studies that were not repeated.[5] Third, the age of the study's participants and differences between boys and girls were also considered possible moderators, as was the study year and whether the study employed self-report data or other methods such as time diaries.

Results

Table 1 presents the results for all analyses. Figure 1 presents a funnel plot for all studies. An analysis of all effect sizes suggested that the mean effect of social media on mental health across studies was near zero ($\beta = .061$) and below our threshold for evidentiary value. This suggests that observed effects are indistinguishable from statistical noise (Ferguson & Heene, 2021). However, there also was significant heterogeneity between studies suggesting the potential impact of moderators.

A table of all studies with the types of social media included is available at https://osf.io/cse9y. However, it is emphasized that these social media forms were typically mentioned in questions or prompts, and data for them were not typically collected or analyzed separately.

Sex Differences

A key potential moderator is sex, as it may be the case that girls have more vulnerability to social media effects than boys (Twenge, 2020). As such, we considered biological sex differences in effect size. Initial results using a mixed-effects model (k = 56) were nonsignificant ($p = .074$); however, resilience testing suggested that model estimator had an impact on p value with p values ranging from <.001 (Hunter-Schmidt method) to .111 (Sidik-Jonkman method). An examination of effect sizes revealed that effect sizes for girls were slightly larger ($\beta = .075$) than for boys ($\beta = .044$) but that both effects fell below the threshold for evidence.

Other Moderator Analyses

Contrary to other meta-analyses (e.g., Ferguson et al., 2022), best research practices were not a continuous moderator of effect size (though, as with biological sex, resiliency analysis suggested that this depended on the model used) nor was the study year. The age of the participants in the sample was also nonsignificant ($p = .053$). Citation bias proved not to be a moderator ($p = .319$) and study type (correlational vs. longitudinal) was also nonsignificant ($p = .067$).[6] The effect sizes for longitudinal studies were slightly smaller (β = .044) than that of correlational studies ($\beta = .072$), albeit once again all effect sizes were below the threshold for evidentiary value. Contrary to our expectations, the use of self-report versus time diaries and other objective methods did not prove to be a moderator ($p = .430$) though the type of data set (bespoke vs. large national vs. dissertation) did ($p = .043$). In this case, bespoke data sets ($\beta = .041$) and dissertations ($\beta = .045$) had smaller effect sizes than did national data sets ($\beta = .067$).

Best Research Practices

The utilization of best research practices varied. Some, such as the use of standardized and well-validated measures of mental health, were very common among reported effect sizes (approximately 95% and 92% of studies, respectively). The use of basic controls for gender, age, family environment, and Time 1 outcomes in longitudinal studies was also common (64%) though not as

[5] This differs from the intent in our preregistration where we had hoped to extract a single effect size from each data set. However, many of the debates in this area considered the most appropriate way to do so. As such, we decided to include all articles but include potential multiuse data sets as a moderator.

[6] There was only one experimental study; thus, this was not included in the analysis.

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

Effect size	k	β	95% CI	Homogeneity test	I^2	τ	Publication bias?
All studies	79	.061	[.047, .075]	$X^2(78) = 4404.45$, $p < .001$	98.8	.055	No
Biological sex							
Male	27	.044	[.025, .062]	$X^2(26) = 164.79$, $p < .001$	94.7	.040	No
Female		29 .075	[.050, .101]	$X^2(28) = 388.10$, $p < .001$	97.9	.063	No
Study type							
Correlational	48	.072	[.05, .090]	$X^2(47) = 4167.83$, $p < .001$	99.3	.057	No
Longitudinal	30	.044	[.023, .066]	$X^2(29) = 169.54$, $p < .001$	83.6	.049	No
Data set							
Bespoke	21	.044	[.012, .070]	$X^2(12) = 167.61$, $p < .001$	89.2	.046	No
National survey	53	.067	[.050, .084]	$X^2(52) = 99.21$, $p < .001$	99.2	.058	Yes
Dissertation	5	.045	[.016, .074]	$X^2(4) = 1.81$, $p = .770$	57.2	0	No

Note. k = number of studies; β = pooled effect size estimate; CI = confidence interval; I^2 = heterogeneity statistic.

much as expected. By contrast, the use of multiple respondents (19%), distractor questions or tasks (0% reported), preregistration (5%), or careful querying for hypothesis guessing (1%)[7] were very rare. Citation bias was present for 15% of reported effect sizes.

Publication Bias

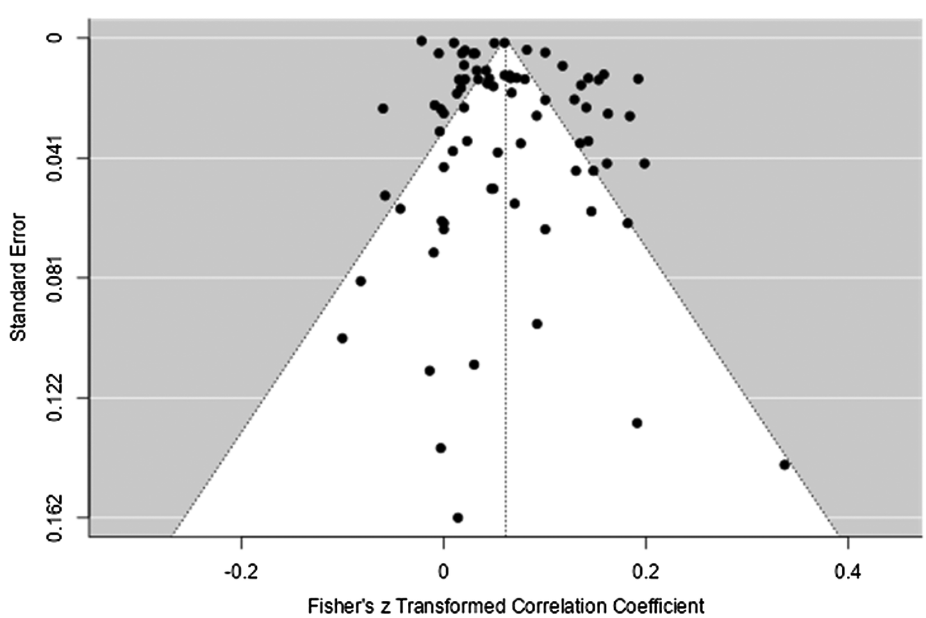
Evidence generally suggested an absence of publication bias in this area. However, this result is cautioned on the observation that publication bias measures tend to be underpowered, particularly concerning large data sets with small effect sizes wherein p values of .05 are easily surpassed, making bias more challenging to detect with measures dependent upon p values. Evidence from Egger's regression ($p = .021$) and trim and fill (missing studies: four) suggested potential for publication bias in this meta-analytic data set.

Discussion

We explored the existing literature on social media use and impact on adolescent mental health. We contend this to be important to gauge the quality of the existing evidence, especially as this research often informs public policy, health guidelines, and social technology innovation relating to user well-being on social media. It also has the potential to fuel moral panic if results are overstated or misconstrued. As such, our meta-analysis addressed two overarching aims:

- to establish the effect size between social media use and mental health variables in adolescents
- to establish the prevalence of best research practices in studies in the existing literature and their impact on observed effect sizes

Figure 1
Funnel Plot for All Studies



Overall Findings

Overall, our findings indicate that the current research literature is unable to provide strong evidence for a clinically relevant link between time spent on social media and mental health issues in youth. This was true for both boys and girls and across both correlational and longitudinal studies. As such, we observe that public statements coded toward warning language, such as those provided by the American Psychological Association (2023) or the US Department of Health and Human Services (2023), are not faithful to the research data as it currently stands. To be fair, the American Psychological Association (2023) did note "Using social media is not inherently beneficial or harmful to young people" (p. 3), though its narrative generally covers evidence for harm, playing less attention to critiques of that view or null studies.[8] The US Department of Health and Human Services (2023) also briefly mentions benefits of social media use, but neither report outlines how they searched for studies, suggesting that their coverage of research may be selective and nonsystematic. Put more plainly, the current concerns about social media appear consistent with a pattern of overstatement by scholarly and governmental groups; this may be related to moral panic (Bowman, 2016).

Compared with other research topics that have experienced moral panic (e.g., Ferguson, 2015), our meta-analysis suggested two interesting differences. First, citation bias was not a predictor of effect sizes nor were best research practices. It appears that even advocates of the technology harm approach (e.g., Twenge, 2020) faithfully acknowledge inconsistent research and, for that, deserve considerable credit. On the contrary, the issue of best research practices is mixed. Some best research practices, such as the use of well-validated measures or control variables, appear to be more common than in other areas, such as media violence (Freedman, 2002) or body dissatisfaction and media (Want, 2014). However, other best research practices, such as preregistration, were rare, and the area remains dependent on studies with high-demand characteristics and few checks for unreliable or mischievous responding. In this sense, our best research practice approach may have lacked much by way of variances as both strengths and weaknesses of the area are spread rather evenly.

Thus, the main issue for this field of research appears to be a failure to accurately communicate two key things: *between-study inconsistencies* and overall *weak effect sizes* that are largely universal between studies. In this sense, this area appears to have fallen for a common problem in research psychology of overinterpreting statistically significant but near-zero effect sizes in large samples. Though these problems have been known for some time (e.g., J. Cohen, 1994; Wilkinson & Task Force on Statistical Inference, American Psychological Association, Science Directorate, 1999), they persist particularly when narratives of "harm" have political and social purchase during periods of moral panic.

We note that this work extends the findings of Ferguson et al. (2022). This current meta-analysis clarifies that negligible evidence for effects also extends to the specific concerns related to social media use among youth.

Moral Panic

We observe that there is a divergence between the quality, consistency, and effect magnitude of research results in this field and the public statements of some scholars, advocates, and policymakers. This pattern of discrepancy between research results and exaggerations of evidence in support of a panic theory is consistent with other media fields that have gone through periods of moral panic, such as those related to television (Freedman, 2002) or violent video games (Ferguson, 2015).

We believe it is incumbent upon researchers to be more aware of this historical pattern. Indeed, though we understand that teaching time is limited, it may be worth specifically including it in curriculum related to social psychology, developmental psychology, media psychology, and cyberpsychology specifically. Academic-fueled moral panics have significant capacity to distract societies from more pressing issues, giving this matter at least some degree of urgency. Given that this pattern repeats with each new technology, it is clear that researchers have not learned to take this historical view and become more cautious in advancing theories related to the pernicious impacts of new technologies.

One thing that is evident is that in some cases, different scholars are examining the same data and coming to very different conclusions. This, too, is expected under moral panic theory. Part of the issue is that social science lacks a clear rigor in regard to evaluating effect sizes. Thus, when large sample studies find statistically significant results with weak effect sizes, it can be possible for scholars to (in good faith) ignore warnings about the overinterpretation of weak effect sizes (e.g., Wilkinson & Task Force on Statistical Inference, American Psychological Association, Science Directorate, 1999), so long as statistical significance has been achieved (which it almost always is in large sample studies, no matter the variables; see Ferguson & Heene, 2021). Thus, beliefs in a harm hypothesis can be maintained, even if the evidence is negligible, particularly when there is social pressure and incentive to do so.

In noting that a cycle of moral panic has emerged among policymakers and professional guilds[9] such as the American Psychological Association and American Psychiatric Association, we do not mean to imply that parents are wrong to have questions regarding the potential impacts of social media use. Nor are scientists wrong to study hypotheses about potential relationships between social media use and mental health (as indeed we study authors do ourselves). However, parents arguably have been let down by societal leaders and, as often as social media is (rightly) criticized for misinformation, so too many societal leaders (whether policymakers or mental health experts) have misinformed parents regarding effects of social media use on young people. There may be legitimate concerns regarding some aspects of social media, particularly the poor quality of information that youth are exposed to. However, it is difficult to offer concrete solutions to these legitimate concerns until those who speak on behalf of science

[7] It is possible that queries for hypothesis guessing or distractor tasks may have been used in some studies but went unreported in the manuscripts.

[8] See also the November 2023 letter from the American Psychiatric Association to the Department of Commerce: https://www.psychiatry.org/getattachment/8861f2e3-2fbc-4d8b-9a26-b5e49dd7eaab/APA-Letter-NTIA-Social-Media-Youth-RFI-11162023.pdf

[9] We are aware that such organizations may object to being referred to as "guilds," perhaps preferring to viewing themselves as having complex roles including professional guilds but also science organizations and publishing houses. However, given the long-standing concerns about how well such organizations function as science organizations, not limited to this issue (e.g., O'Donohue & Dyslin, 1996), we are confident in using the term guild rather than "science organization."

(even if these are professional guilds, not science organizations) are not guilty of providing significant misinformation on the technology at the core of the concern.

Future Directions and Policy Implications

Social media use may arguably be better understood not just from "how much" (i.e., volume and/or frequency of use) but also through understanding the *what*, *how*, and *why* behind users' use and behaviors on such platforms (Kaye, 2022). Namely, what type of content users are accessing or engaging in (what), the types of behaviors or interactions in which they engage (how), and why they might be using social media at a given point in time (why). As such, we recognize that our inferences regarding the lack of strong evidence about the relationships between social media use and mental health variables are based on the way scholars have operationalized measures of these constructs which might, in some cases, not fully capture the complexities of the social media behavior or engagement. For example, recent research has noted the relevance of understanding specific interactions and behaviors when measuring social media use (Meier & Reinecke, 2021; Trifiro & Gerson, 2019). Furthermore, recent findings have established that these may bring about differential impacts on psychosocial functioning (D. J. Shaw et al., 2022; Valkenberg et al., 2022). For example, when objectively measuring social media-related behavior, more interactive behaviors relative to more passive or reactive ones are associated with greater feelings of social connectedness and social capital (D. J. Shaw et al., 2022). As such, research that exclusively measures the volume of social media use is failing to capture key nuances that might otherwise elucidate varying relationships between social media use and mental health.

Acknowledging nuances in user behavior and influence on outcomes indicates that approaches that focus on *banning* or restricting social media by age are unlikely to be of value. First, there is little empirical support for such approaches, and they may backfire insofar as limiting youth ability to adjust to this technology when parental and teacher influences on good practices might be most influential. Banning approaches may also create a forbidden fruit phenomenon that can make access more enticing; may result in users hiding access from their parents, guardians, or teachers (Kerr & Stattin, 2003); or may impact on positive outcomes related to usage such as peer support and/or mental health recovery information (e.g., Branley & Covey, 2017). Approaches that focus on education, media literacy, and helping adolescents develop good practices on social media are likely to be more constructive. Just as abstinence approaches in sex education often fail to reduce teen pregnancy, abstinence approaches to social media may likewise be less than productive.

Interestingly, since this meta-analysis was originally conducted, 41 state attorney generals in the United States have sued the company Meta (of Facebook and Instagram), claiming that the social media sites do harm to minors (see (Lima-Strong & Nix, 2023)). This case rests upon multiple concerns, not only mental health and "addiction" but also related to privacy concerns. However, mental health claims appear central to the case.[10] We do not seek to dismiss the possibility that some harms outside the realm of internalizing disorders studied in the current analysis may be possible. However, it is interesting to note that our meta-analysis does not suggest that the empirical literature supports a relationship between social media use and mental health impacts in youth. It is our observation that such efforts by the government, to the extent that they rely on such claims, are out of sorts with the current available data that do not suggest any currently support claims of "harm" by social media for youth wellness. We are cautious about the fact that public policymaking and discussions are unfolding without the robust backing of empirical research. This leads to a situation where the consequences, especially regarding government regulation of media under the pretext of shielding from "harm," remain largely uncertain.[11]

Limitations

We note some limitations of our research. One limitation that extends to any meta-analytic research includes the fact that the quality of the research is determined by the quality of studies they incorporate. As previously noted, this topic is plagued by a range of methodological issues such as demand characteristics, lack of preregistration, and few reliability checks, and so it is likely that effect sizes are inflated by these issues, which is equally attributable to our findings reported here. However, promising advancements in the area are beginning to overcome some of these methodological shortcomings. Namely, while the literature has historically been overreliant on cross-sectional designs (which fail to capture causal effects) and retrospective self-report measures of social media use (which can lead to inaccurate and ill-defined measurements), more recent research is making use of more advanced study approaches such as experience sampling to better capture person-specific factors in social media use over time and context (see Valkenberg et al., 2022). These will be better positioned to offer a more authoritative account of the nuances of these issues and explore the possibility that there is no linear or uniform relationship between social media use and mental health, with outcomes instead being based on a complex and diverse range of factors. Although samples vary regarding their representativeness of a wide range of ethnicities and cultures, more studies with underrepresented groups would be helpful, as ethnic differences in outcome may be possible. An additional limitation was that we observed a low level of variance within the best practice analysis, which limited our ability to provide a full exploration of these potential effects. Best practices were either almost ubiquitous (e.g., standardization) or entirely lacking (e.g., preregistration). We dutifully report our best practices analysis as it was preregistered but observe that our results are inevitably impacted by this lack of variance. We note that controlling for best practice typically reduces observed effect sizes (Ferguson et al., 2022), so the current findings might not fully represent the potential magnitude of these expected effects. Last, we acknowledge that by focusing our search on APA PsycInfo and Medline, it is possible that studies from other fields may have been missed.

One possible explanation for the lack of findings may be that all youth have been exposed to social media and we may be looking at a

[10] For example, Florida Attorney General Ashley Moody released a statement: "Meta has gone unchecked for too long, and our children are suffering the consequences of these unlawful practices. Today, I took action to stop Meta from targeting minors with addictive features to keep them online for hours, collecting their data and other unlawful actions that harm teens' mental health," (from: https://www.myfloridalegal.com/newsrelease/attorney-general-moody-takes-legal-action-against-meta-protect-children).

[11] We observe that censorship efforts often come in the guide of protecting one "vulnerable" group or another.

cohort effect that is difficult to detect using between-subjects designs due to saturation. However, we find this argument unconvincing for several reasons. First, most critics of social media companies, including professional guilds and policymakers, *do* rely on between-subjects studies, albeit we observe often in exaggerated or selective ways consistent with past patterns of media moral panics such as for video games (Bowman, 2016). It is unscientific to rely on such studies when convenient but dismiss them when not. Naturally, youth will vary among each how much time is spent using social media. If simple exposure to social media is a key issue of concern as has been expressed by policymakers, professional guilds, and some scholars, then it is not unreasonable to see some appreciable relationship between time spent on social media and mental health concerns. Second, neither cohort comparison studies (Ferguson, 2021; Vuorre et al., 2021), nor time-series analysis (Padmanathan et al., 2020), nor, for that matter, studies of functional brain organization (Miller et al., 2023) support the concern that social media use has had an appreciable adverse impact on youth well-being. Thus, arguments about a cohort saturation effect appear to be unwarranted at present.

Conclusion

Our findings illuminate that based on the analyzed literature, the observed effect size for the relationship between social media use and adolescent mental health is below the evidentiary threshold and is as likely due to methodological noise as any actual effect. Although some best research practices are widespread in the area (i.e., well-validated outcomes measures and employment of control variables), others are not (i.e., controls for demand characteristics, preregistration, reliability checks) and do not meet standard expectations to be considered high-quality research. This is somewhat concerning given that many of the lower quality studies are those which are drawn upon as an evidence base for informing policy and practice surrounding social media and mental health. We emphasize that improvements must be made to ensure that the future scientific evidence base is of sufficient quality to ensure that our understanding of the potential risks and benefits has been explored appropriately and rigorously.

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