Like This Meta-Analysis: Screen Media and Mental Health

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The question of whether screen time, particularly time spent with social media and smartphones, influences mental health outcomes remains a topic of considerable debate among policy makers, the public, and scholars. Some scholars have argued passionately that screen media may be contributing to an increase in poor psychosocial functioning and risk of suicide, particularly among teens. Other scholars contend that the evidence is not yet sufficient to support such a dramatic conclusion. The current meta-analysis included 37 effect sizes from 33 separate studies. To consider the most recent research, all studies analyzed were published between 2015 and 2019. Across studies, evidence suggests that screen media plays little role in mental health concerns. In particular, there was no evidence that screen media contribute to suicidal ideation or other mental health outcomes. This result was also true when investigating smartphones or social media specifically. Overall, as has been the case for previous media such as video games, concerns about screen time and mental health are not based in reliable data.

Public Significance Statement
Considerable debate has examined whether exposure to screen media including smartphones and social media is associated with reduced mental health. This analysis suggests that, at present, the data are unable to support such a belief.

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continued

In recent years, intense debates have emerged among scholars, policymakers, and the general public regarding the potential impacts of screen media on psychology and behavior. A prominent area of debate is the extent to which screen media may be related to poor psychosocial functioning, such as depression, anxiety, and suicide ideation, particularly for young people. Such debates can focus on screen media generally under the somewhat nebulous term "screen time" or can focus on specific media such as types of social media platforms, or devices (e.g., smartphones). There is a substantial divergence of opinion on this matter. While some scholars suggest that screen media are a primary cause of a recent rise in teen suicide (e.g., Twenge et al., 2018, 2020), others argue that the evidence is mixed and insufficient, with effect sizes too small to illuminate clear relationships to mental health (e.g., Heffner et al.,2019; Orben & Przybykski, 2019a). Furthermore, other studies suggest that screen use, at least in some contexts, may have an association with positive mental health (e.g., Grieve & Watkinson, 2016; Reinecke & Trepte, 2014; Utz, 2015; Wang et al., 2014). This set of contradictory findings can make it difficult to parse what real effects may or may not exist. The possible social effects of screen time can be particularly pertinent during periods of social distancing due to the Coronavirus disease (COVID-19), wherein many people may increasingly turn to screen media to maintain social connections and fulfill a range of everyday tasks. Given inconsistencies in the research literature, meta-analysis can be an effective tool to help consolidate findings in this area and help explore discrepancies. This rationale forms the basis for the current article, which provides a consolidated analysis of the current state of the science in this field.

Why Media Effects Can Be Hard to Pin Down

Before considering the issue of empirical evidence, it can be helpful to understand the historical context of concerns over media and why it can often be challenging to elucidate what links do and do not exist between media use and adverse outcomes. It has been observed that new media and technology regularly elicit periods of *moral panic* in which societal stakeholders express considerable anxiety over alleged pernicious effects, even when available data are unclear or suggest such effects do not exist (Bowman, 2016; Kutner & Olson, 2008). Initially, incentive structures tend to place scholars and professional guilds such as the American Psychological Association (APA) under pressure to support the panic (Ferguson, 2013; O'Donohue & Dyslin, 1996). With time, evidence for the panic erodes, and society ultimately rejects links between the new technology and negative outcomes (Bowman, 2016). A recent example of this has involved the debate over whether video game violence could be associated with aggression (Markey et al., 2015).

Of course, this pattern of moral panic around new technology does not necessarily preclude the potential for some forms of screen media to have real influences on mental health. For example, compared to other media, social media may be integrated into more aspects of our daily lives (including at home and work). Similarly, while technologies still provide both synchronous and asynchronous forms of communication, the variety of interactions provided by smartphones is more expansive when compared against older technologies. It would be premature to assume that screen media concerns are related to past moral panics rather than real potential harm.

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Examining Terms: What Exactly Is "Screen Time?"

Many studies estimating the impact of screen media on mental health consider the concept of "screen time," a term that is controversial due to vague and shifting definitions, as well as conceptual and methodological limitations.

In many studies, respondents are asked to estimate the amount of time, either in raw hours or in categorical clusters, spent with screens. Such inquiries may or may not specify *entertainment* screens, adding a potential layer of confusion. Given that many users may multitask with screens, differentiating between entertainment and nonentertainment screen usage is conceptually difficult. This may be particularly true during the time of the COVID-19 which saw a rapid increase in the use of screens for work, education, socialization, and even mental health treatment (Branley-Bell & Talbot, 2020), tasks that became limited or impossible during lockdowns and social distancing.

Screen time as a concept is also confused for several other reasons (Kaye et al., 2020). Firstly, the extensive interchangeable use of terminology (e.g., screen time, digital media use, screen use), and the tendency to conflate many different forms of technology under one category. This may include clustering together screens such as e-readers for books, to television, to video games, to smartphones, and social media; each an expansive range of stimuli, served by an even wider range of screen displays and functions. It is possible that effects for different media may differ widely, observations about which may be lost when all screen use is clustered together (McDonnell et al., 2019).

Methodologically, our understanding of screen time is greatly restricted by reliance on subjective self-reports of screen use. An ever-growing body of work highlights that people are generally very poor at estimating their screen use, as evidenced from studies showing that self-reports often fail to accurately correspond to objective behaviors (Ellis et al., 2019; Parry et al., 2021). A further concern is that the relationship between screen use, such as smartphones, and mental health outcomes is substantially elevated when using subjective reports of smartphone attitudes or estimates relative to objective log data (Shaw et al., 2020). One alternative to self-report would be the use of time diaries wherein respondents are periodically asked or reminded to note current screen use in real time (Orben & Przybylski, 2019a) or note their screen time via the information provided in their device's screen time settings. It is also possible for some studies to obtain permission from respondents to simply track usage on respondent's own devices (e.g., via an app). Given the additional investment in developing this as a more standardized approach in the field, and increased methodological difficulty (e.g., a requirement for app installation and suitable hardware, the potential for respondents to be wary of data tracking, etc.), self-report is likely to remain common for the foreseeable future, despite its known issues.

A Brief and Broad Overview of Existing Research on Screen Time and Mental Health

Scholars have been investigating the broad construct of "screen time" for decades. A subject search for the term in PsycINFO reveals 642 hits (December 21, 2020). There was a sharp upturn in the usage of the term by the mid-2000s—in line with the introduction of Web 2.0 and many social media platforms. Of course, similar veins of research have existed for decades, even if not using the "screen time" concept by name. For instance, concerns about television viewing and mental health existed for decades, with research on these topics reaching a climax in the 1980s (e.g., Rubinstein, 1983) before largely switching to video games by the late 1990s. Concerns in the research literature about the alleged harmful effects of other mass media, such as radio (Preston, 1941) and comic books (Wertham, 1954), in the research literature date to the 1940s.

The literature on "screen time" has grown substantially over the last few decades. Such a large body of literature can be difficult to synthesize for several reasons. First, as definitions of "screen time" are vague and conceptually elusive, they may change over time. Second, technology has, itself, changed over time and what is encompassed by the term "screen" may differ today from 10 or even 5 years ago. Third, since the focus of media effects research shifted toward "screen time" in the early 2000s, concerns about problems with research replication have grown across social science disciplines, including psychology; this will undoubtedly affect perceptions and appraisals of the screen time research base (Simmons et al., 2011). Because of these issues, the current review of the evidence base focused primarily on the previous 5 years (at the time of data collection) to best reflect both the most recent science and the current technology of concern.

Is Screen Time Associated With Mental Health?

As aforementioned, the literature on screen time and mental health has produced inconsistent results. Where some studies find positive correlations between screen time and mental health, others find negative links, and some fail to find any relationship between these variables (e.g., Dennison-Farris et al., 2017; Ferguson, 2017; Heffner et al., 2019; Tamura et al., 2017; Višnjić et al., 2018). Statistical effects are generally small in size ($r < .10$, which corresponds to an overlap of variance of 1%) even when considered "significant." This realization is particularly true for studies which control for theoretically relevant third variables such as family environment, gender, and preexisting mental health difficulties. Thus, from a narrative review of existing studies, it is difficult to come to a clear conclusion about whether effects do or do not exist.

An understanding of "screen time" and mental health outcomes is often limited by the cross-sectional nature of many studies; with some studies using the findings of existing data sets to "detect" small associations between screen use and mental health outcomes. For example, Kleppang et al. (2019) found an increase in psychological distress from 2001 to 2009. However, in addition to reporting that "the associations, if any" (p. 7) between physical activity, sedentary behavior, and psychological distress were weak, there were possible estimation errors, and need for standardization of self-report measures for future research. In contrast, other research suggests that a focus on *how* screens are used is more important than the amount of time spent using them (e.g., Davila et al., 2012). Furthermore, some find nuanced positive and negative associations (Chan, 2015; Park et al., 2016), while some report minimal meaningful relationships (Ferguson, 2017). One recent meta-analysis, focusing on social media specifically, found that cross-sectional associations with mental health outcomes were generally weak (Huang, 2017). A recent study by Ferguson (2021) suggests that there is no evidence that associations between screen use and mental health issues among youth have increased in recent years.

The Debate Over Screens and Suicide

In this field, there is heated debate over whether screen use (including use of specific screens such as social media or smartphones) can be linked to a rise in teen suicides, particularly among teen girls. This debate among academics has captured public attention, particularly following the publication of an essay in *The Atlantic* titled, "Have Smartphones Destroyed a Generation?" (Twenge, 2017). This remarkable claim has touched off several years of intense debate (e.g., Orben et al., 2019; Twenge et al., 2020), with opposed groups often reanalyzing and debating over the same large data sets.

At issue is the observation that, at least in the United States,[1] teen suicides, particularly among girls, have been increasing (Centers for Disease Control [CDC], 2020). Such rates are still lower than they were in the early 1990s, but such a rise is undoubtedly worrying. Twenge et al. (2019) have attributed this rise in suicides to screen technologies, particularly social media and smartphones, as this rise began around the time that these specific technologies came into more widespread use. However, current estimates (CDC, 2020) suggest that both overall suicide rates and raw suicide increases are much higher among lower tech-adopting middle-aged adults than they are for teens (see Figure 1). During the past 20 years in the United States, middle-aged men had the greatest annual increase in suicides (mean yearly increase of .61 suicides per 100,000 people). Whereas teenage girls showed the lowest increase in suicide rates during the same time period (mean yearly increase of .14 suicides per 100,000 people). Although one cannot rule out differential causes across age groups that might implicate technology, it would be difficult to attribute causal influence to technology from this ecological data.

Further, data on suicide rates from nations with patterns of technology use similar to that of the U.S. do not consistently display an increase in suicides in recent years. For example, Eurostat highlighted an overall decrease in suicide rates between 2011 and 2017 for adults between 50 and 54 years and adolescents between 15 and 19 years of age. Within the 50–54 year age group, of the 32 European countries with statistics from this period, 21 countries reported a decrease, while 11 reported an increase in suicide rates; in parallel, within the 15–19 year age group, of the 19 countries with statistics from this period, 9 countries reported a decrease, while 10 reported an increase in suicide rates (Eurostat, 2020).

Possible Limitations of Screen Time Research

Other realms of research have done a thorough job in examining methodological issues which might influence results. In particular, these are considerations which may be associated with spuriously elevated effect sizes (Drummond et al., 2020; McDonnell et al., 2019; Want, 2014; Whyte et al., 2016). These methodological issues are shared in other areas of media research, such as violent video game research or research on thin ideal media and body dissatisfaction. Some of these issues work by unintentionally setting up demand characteristics in the study wherein it becomes possible for the respondents to either guess or be more subtly influenced by the study hypotheses (Orne, 1962). Such issues can include: (a) the failure to include distractor items, questionnaires, tasks items or tasks, etc., so that independent variables and dependent variables are not too closely paired together (Whyte et al., 2016), (b) failing to include multiple responders (e.g., parents and children) so as to avoid single responder bias (Baumrind et al., 2002), and (c) lack of careful probing for hypothesis guessing during debriefing. Without measures to counteract these phenomena, results may show spurious correlations in the direction of the hypothesis.

Other concerns involve the misuse of unstandardized and poorly validated measures that may allow for *p*-hacking or researcher degrees of freedom (Elson et al., 2014). Preregistration of studies (i.e., publishing hypotheses, materials, and analyses plans prior to data collection) can help reduce such researcher expectancy effects. Unfortunately, few studies in this realm are preregistered.

Other issues can come from a lack of appropriate controls. For instance, in some studies, experimental and control conditions might vary on qualities other than those of interest to the hypotheses, such as engagement, excitement, emotional valence, etc. (Want, 2014; Whyte et al., 2016). Given that few studies in this realm are experimental, this may be less of a concern. However, well-designed correlational studies should carefully control for theoretically relevant third variables such as personality, family environment, gender, and, in the case of longitudinal studies, Time 1 (i.e., preexisting) mental health symptoms.

The Perils of Small Effects

It is difficult to know whether such small effects are "true" as opposed to artifacts of methodological problems. With large sample sizes, the opportunity for methodological errors to create spurious effects in the direction of the hypothesis is nontrivial. This problem has been recognized for decades. For instance, as far back as 1968, Lykken noted that "the effects of common method are often as strong as or stronger than those produced by the actual variables of interest" (Lykken, 1968, p. 153). Evidence for this problem was also demonstrated more recently by Ferguson and Heene (in press). The authors examined two large data sets involving aggression research. Examining nonsense predictors (theoretically unrelated variables), they found that "statistically significant" correlations below $r = .10$ were quite common, indicating a lack of precision in social science research with regards to distinguishing noise from signal. Some degree of false positives continued to the $r = .20$ level of effect size. The authors argued against interpreting any effect sizes below $r = .10$ as hypothesis supportive whether or not they were "statistically significant."

Given significant concerns about methodological limitations in this body of research causing spurious effect sizes, we express the concern that it may be impossible to separate any "true" effects below the $r = .10$ threshold from the noise created by common methodological issues such as demand characteristics or common method variance. Naturally, observing an effect size above $r = .10$ is no guarantee the effect is not noise, through the probability is likely lower, at least for rigorously designed studies. However, faulty overinterpretation of low r noise effects below the $r = .10$ threshold is likely a serious source of misinformation on social science.

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[1] This trend does not appear consistent across all high tech-adopting countries, which raises the concern of selective interpretation of data points.

Figure 1

Suicide Rates Across Gender and Age Categories
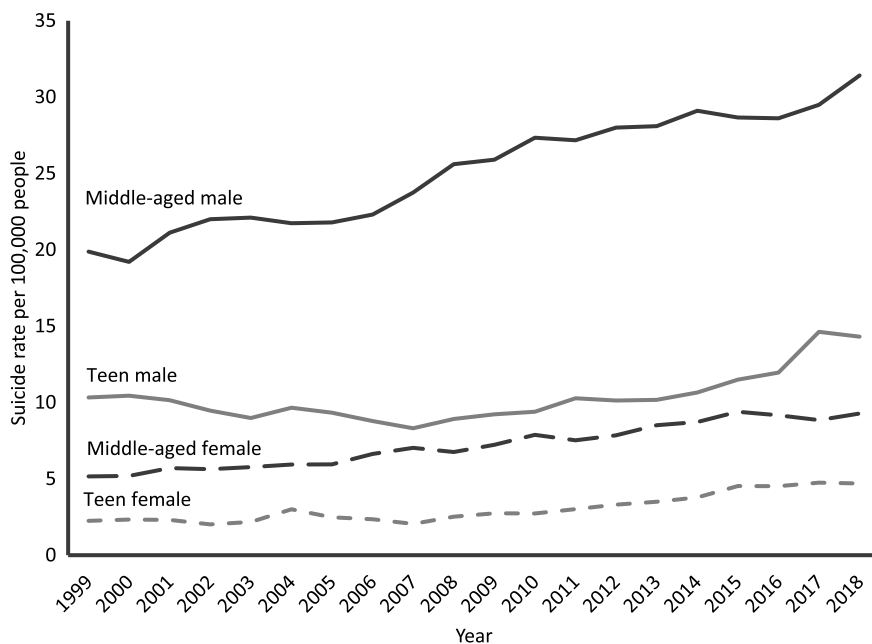
Note. Suicide rate per 100,000 population, teens (13–19 years olds), middle-aged persons (55–65 years old), by sex, CDC's Fatal Injury Reports 1999–2018. CDC = Centers for Disease Control and Prevention.

Method

Disclosures

A preregistered plan outlining the search strategy, inclusion and exclusion criteria, as well as an analysis plan can be found at: https://osf.io/rehys. Data from the meta-analysis can be found at: https://osf.io/mex4s/. This data includes full citation, sample size, effect size, best practices analysis, and moderator variables. This information allows readers to assess our analyses directly, as well as to see that our planned analyses were not altered to fit hypotheses. This open data approach can decrease false positives and increase confidence in research results.

Selection of Studies

Identification of relevant studies involved a search of the PsycINFO and MedLine databases using the search terms ("Screen time" OR "Screen use" OR "Screen engagement" OR "Smartphone" OR "Cell phone" or "Mobile phone" or "Tablet") AND ("depression" OR "anxiety" OR "loneliness" OR "suicide") AND ("youth" OR "teen*" OR "adoles*") as subject searches. The search was limited to the most recent studies (2015–2019) which would reflect both the most recent research, at the time of data collection, as well as the most current technology. Unpublished studies were excluded, and studies were required to meet the following inclusion criteria:

1. Include a measure of screen use, or experimental comparison of screens with a control condition.

2. Present statistical outcomes or data that could be converted into effect size "*r*." As per the preregistration, these were generally taken from standardized regression coefficients or, calculated from *F*-values, *t*-tests where required. Data from odds ratio were converted using formula provided by Bonett (2007).

3. Published between 2015 and 2019[2].

The original preregistered study design plan was limited to teen samples, but ultimately this broadened out to include all samples to get a wider view of data among young adults as well. Age is considered as a moderator. This decision did not affect the results or conclusions.

The initial search (carried out in October 2019) returned 213 matches, many of which were either nonempirical or otherwise did not meet the inclusion criteria. Eliminating such studies resulted in 37 papers which had both data on screen time usage (including social media or smartphones specifically) as well as mental health-related outcomes. A PRISMA chart which documents the study inclusion path is provided as a flow diagram (available at: https://osf.io/2bnc8/). All studies included in the final sample were either cross-sectional or longitudinal in nature.

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[2] In our preregistration, we were open to going 10 years out if too few studies were found. As the number of studies initially located surpassed 50, we limited the years to the past five as per our preregistration.

Effect Size Estimates

Effect sizes were operationalized using the metric of r. Particularly for correlational and longitudinal studies, analyses used results which were based upon multivariate analyses resulting in standardized regression coefficients (betas). The benefits of using betas in meta-analyses are plentiful, including the fact that they make sense theoretically given that most multivariate analyses include theoretically relevant controls. Additionally, from a statistical point of view, solely relying on bivariate r may showcase high effect size estimates that do not reflect real correlations once important factors are controlled (Pratt et al., 2010; Savage & Yancey, 2008). Correspondingly, Furuya-Kanamori and Doi (2016), note that betas produce a closer estimate of underlying effect size than bivariate rs. Using Monte-Carlo simulation, these authors confirmed that betas are appropriate for use in meta-analysis and do not produce erroneous effect size estimates. The use of beta has become increasingly common in meta-analyses and has been strongly advocated among scholars (Bowman, 2012; Pratt et al., 2010; Savage & Yancey, 2008).

In cases where articles presented more than one effect size estimate, they were aggregated for average effect size. Generally, for the included studies, when multiple outcomes were used, heterogeneity in effect sizes was low, suggesting aggregation was appropriate. Given that such measures were typical of the same construct, the assumption of a high correlation between conceptually similar outcomes warrants simple aggregation (Pustejovsky, 2019).

Moderator Analyses

Several moderators were considered potentially important to the current analysis. These included: study year, age of participants, type of study (correlational vs. longitudinal), culture of participants (West/European, Asian, and Hispanic), type of media (smartphones, internet/social media, or general screen time), and whether studies only cited evidence supporting their hypotheses despite inconsistencies in the literature (e.g., citation bias). Note that culture, type of study, and type of media were not preregistered as moderators. Thus, their inclusion should be considered exploratory. Studies were also coded for best practices. Papers were considered adherent to best practices if they:

1. Used standardized and well-validated measures. Measures were considered standardized if they had a clear protocol that is followed without deviation. Standardized tests reduce the potential for researcher degrees of freedom that create false-positive results. Validated measures are those that have been demonstrated to predict outcomes related to clinically significant mental health (e.g., *Child Behavior Checklist*, *Beck Depression Inventory*).

2. Controlled for theoretically relevant third variables (e.g., gender, age, family environment, and Time 1 mental health in longitudinal studies) in correlational/longitudinal studies.

3. Used multiple respondents to avoid single responder bias.

4. Employed distracter questions or tasks to reduce hypothesis guessing.

5. Carefully queried for hypothesis guessing at the conclusion of the procedure.

6. Were preregistered.

As a note, the preregistration had criteria for both correlational/longitudinal as well as experimental study best practices. However, ultimately, no experimental studies were included in the analysis. Thus, criteria that applied to experiments only are not repeated here.

There were two types of moderator variables: continuous moderator variables (e.g., age, year of study) and categorical moderator variables (e.g., gender, culture). Continuous moderator variables (age, date) were examined using meta-regression. This technique allows for the examination of a correlation between a continuous moderator and study effect size using regression techniques. Categorical moderators can be examined for subgroup differences in effect size that are significant (i.e., unlikely due to chance). This can be done with their fixed-effect or mixed-effect models. With mixed-effect models, as with random-effects models for overall meta-analysis, the equal variance between studies is not assumed across subgroups. As such, mixed-effects models in fields with heterogeneous study methods tend to be more appropriate, although both fixed-effect and mixed-effects models are reported in the Results section. Where differences occurred, mixed-effects models were preferred to fixed-effect models, although no substantial differences emerged between models.

Analysis

The Comprehensive Meta-Analysis (CMA) software program was used to fit random-effects models. The potential for publication bias was assessed using the Tandem Procedure (Ferguson & Brannick, 2012) which looks for concordance among several funnel-plot-related tests for bias (Orwin's Fail-Safe N, Egger's Regression, Trim and Fill). This procedure is an empirically demonstrated, conservative estimating procedure for assessing publication bias, with low Type I error rates. However, it should be noted that by reducing Type I error rates, Type II error rates for the Tandem Procedure are increased. Thus, it should be considered a very specific, but less sensitive measure for detecting publication bias. A negative result on the Tandem Procedure does not ensure the absence of publication bias. Assessments of publication bias were used based on the concordance of Orwin's Fail-Safe N (how many studies it would take to reduce effect sizes to $r = .10$, indicating fragility in the evidence base), Egger's regression for effect size and sample size, and the Trim and Fill procedure. Trim and Fill corrections for publication bias, where warranted based on the Tandem Procedure decision, are reported as r_c. The traditional Fail-Safe N, by focusing on statistical significance, typically vastly overestimates confidence in meta-analyses, but Orwin's version improves upon this through an examination of effect sizes rather than statistical significance. Trim and Fill, like most methods, typically has low power and the potential for Type II error (Ferguson & Brannick, 2012).

Interpretation of effect sizes has been controversial in psychological research. Many effect sizes are near zero but may be "statistically significant" due to the high power of meta-analyses. This may result in miscommunication as trivial effects become "statistically significant" (Ferguson & Heene, in press; Orben & Przybylski,

2019b). Although any cutoff threshold is arbitrary, the present analyses determined the cutoff as $r = .10$. This mitigated against the issue that any values below this would be explained primarily as due to study artifacts rather than real population-level effects (Ferguson & Heene, in press; Przybylski & Weinstein, 2019).

Results

Main/Preregistered Results

Main results for the meta-analysis are presented in Table 1. As can be seen from these results, the effect sizes for relationships between screen time as well as specific screen media such as smartphones and social media were very small and in no case passed the $r = .10$ threshold for interpretation as hypothesis supportive. Significant heterogeneity existed in all data sets, although this was particularly true for correlational studies and those which examined general screen time, as opposed to longitudinal studies or those examining specific screen media. Longitudinal studies did not provide any more evidence for effects than correlational studies, suggesting there is little evidence for a cumulative effect.

Although there is significant between-study heterogeneity, this did not appear to relate to our main moderator variables. For instance, there was no significant difference in effect due to ethnicity ($Q = .358$, $p = .836$), technology type ($Q = 1.121$, $p = .571$), study type ($Q = .050$, $p = .823$), or presence of citation bias ($Q = 1.596$, $p = .207$). Meta-regression for continuous moderators were nonsignificant for participant age ($Q = .001$, $p = .969$) or best practices ($Q = 2.223$, $p = .136$) although, curiously, study year was a significant moderator ($Q = 15.721$, $p < .001$). Effect sizes were slightly smaller in more recent years. It should be noted that the statistical sensitivity to detect these moderator effects was relatively low due to the small number of studies.

Publication Bias

Results from the Tandem Procedure indicated that there was not strong evidence for publication bias in this research field. The Tandem Procedure, it should be noted, is less sensitive with regards to large samples with smaller effect sizes, so it is possible that some bias remains in this sample of studies. However, such bias, if it exists, does not appear to be driving effect sizes up above the threshold for trivial effects.

Supplementary/Exploratory Results

The prevalence of best practices in the field was examined. These results are presented in Table 2. As can be seen, some best practices were quite common (e.g., the use of standardized and validated outcomes measures), whereas others were virtually absent (e.g., preregistration, the use of distractor items, etc.). Aside from controlling for confounding variables, there was little variance in whether most best practices were employed or not. This likely explains the failure of the best practice analysis to predict effect size, contrary to other fields where best practices are associated with lower effect sizes (Drummond et al., 2020).

The use of theoretical controls was the only best practice variable with significant variance, therefore, this was examined as a categorical moderator. The effect size for studies which did not use controls was higher ($\beta = .064$) than for studies with controls ($\beta = .038$), although neither exceeded the threshold of $r = .10$ for interpretation as hypothesis supportive. Whether this difference was significantly differed whether fixed ($Q = 14.172$, $p < .001$) or mixed-effects ($Q = 2.343$, $p = .126$) modeling was used.

Discussion

Scholars, policymakers, and the public continue to debate on the impact of social media, smartphones, and so-called "screen time" on psychosocial functioning. The current meta-analysis sought to examine the strength of the data in support of these arguments. On balance, the current results found that the current data fail to support the contention that exposure to screen media generally, or social media and smartphones specifically, is associated with negative mental health symptoms. Specifically, effect sizes were below the threshold of $r = .10$ used for interpretation of the findings as hypothesis supportive. Given that some methodological limitations are endemic to the field, it remains likely that such small, albeit "statistically significant" effects are likely to be explained by systematic methodological flaws rather than true effects. This possibility is supported by evidence that those studies which used proper controls, generally found lower effect sizes than those which did not. This, alone, should give scholars reason to

Table 1

Meta-Analytic Results Screen Time and Mental Health Outcomes

Effect sizes	k	r_+	95% CI	Homogeneity test	I^2	tau	Publication bias?
All studies	37	.052	(.036, .068)	$\chi^2(36) = 310.72$, $p < .001$	88.4	.040	No
Ethnicity							
Asian	5	.060	(.015, .104)	$\chi^2(4) = 10.78$, $p = .029$	62.9	.038	No
Caucasian	31	.051	(.034, .069)	$\chi^2(30) = 297.78$, $p < .001$	89.9	.040	No
Study type							
Correlational	25	.051	(.031, .071)	$\chi^2(24) = 285.47$, $p < .001$	91.6	.041	No
Longitudinal	12	.055	(.032, .077)	$\chi^2(11) = 21.88$, $p = .025$	49.7	.028	No
Technology type							
General screens	20	.059	(.036, .083)	$\chi^2(19) = 269.02$, $p < .001$	92.9	.046	No
Smartphones	11	.041	(.011, .071)	$\chi^2(10) = 24.36$, $p = .007$	59.0	.035	No
Social media/internet	6	.043	(.004, .082)	$\chi^2(5) = 11.69$, $p = .039$	57.2	.036	No

Note. k = number of studies; r_+ = pooled effect size estimate; I^2 = heterogeneity statistic; publication bias = decision based on the Tandem Procedure.

Table 2
Proportion of Studies Employing Best Practices

Best practice	Proportion
Standardized outcome variable	91.9% ($n = 34$)
Validated outcome variable	91.9% ($n = 34$)
Multiple responders	8.1% ($n = 3$)
Distractor questions/tasks	0% ($n = 0$)
Control age/gender/family/T1 outcome	62.2% ($n = 23$)
Preregistration	5.4% ($n = 2$)
Queried for hypothesis guessing	0% ($n = 0$)

pause when interpreting results as linking screen time to mental health.

Of particular, concerns are claims by some scholars that appear to link screen time (in particular, social media) to a rise in suicide among teen girls; a claim which made its way unchallenged into the recent Netflix documentary, *The Social Dilemma.* Current results found no support for such a claim in the database and even those papers which raise this claim (e.g., Twenge et al., 2019) appear to do so based largely on conjecture rather than solid data. Further, such claims appear divorced from a fuller understanding of suicide rates across age categories in the United States and other nations which generally point away from technology being a likely cause. Interestingly, age as a moderator was found to be nonsignificant in our analysis. Finally, although scholars appear inclined to point toward increasing suicide in one group of individuals (teen girls) as evidence for the dangers of technology, they have provided no guidelines for how societal data might be used to falsify such claims. For instance, were suicide rates among teen girls to fall in subsequent years despite screen usage remaining high, would this falsify concerns based on prior societal data? It is possible that the use of screens and social media specifically may be associated with idiosyncratic outcomes depending on *how* screens are used, as opposed to time spent using. However, on balance, it is concluded that current data do not support claims about suicide. Such claims are more likely to misinform than inform and may distract from real causes of suicide, which could be dangerous in and of itself.

Best Practices

This field of research deserves both some praise and concern regarding best practices. First, the use of standardized and validated outcome measures is highly prevalent. Although this may seem obvious, this is not the case for other fields of research such as media violence studies (Elson et al., 2014). Further, unlike other fields where citation bias has been found to be associated with elevated effect sizes, this was not the case for this field. This appears to be because supporters of causal effects have been more honest about disconfirmatory research that has been the case in other media effects fields such as video game violence, thin ideal media effects, or sexualized media, and these scholars should be commended for their honesty.

At the same time, other best practices were worrisomely absent. Preregistration was rare, though this might be understandable as the practice is relatively new. However, the use of multiple responders, distractor questions and tasks, and rigorous querying for hypotheses (including reliability checks for unreliable or mischievous

responding) were virtually absent from research in this area. It would be important for future research to improve designs using these best practices to get a clearer understanding of true effects. This study had initially, in the preregistration, also sought to examine whether experimental studies closely matched conditions, but the absence of experimental studies from this sample of studies made this impossible.

Clinical Implications

The available data suggest that management of screen time, in and of itself, is unlikely to be an effective, primary factor in addressing mental health concerns. Misplaced concerns about screens, social media, and smartphones could actually be detrimental due to distracting attention from other, pressing causes of mental health decline—such as economic issues, family stress, and bullying, all areas for which the evidence is more solid. Clinical approaches focusing on technology at the expense of other issues could potentially do more harm than good for patients in therapy. An additional worry is that misplaced concern could lead to positive aspects of technology use being overlooked or negatively impacted. For example, clinicians may neglect to note that screen media actually is often being used to access valuable social support for mental health issues or remote health treatment (e.g., Branley-Bell & Talbot, 2020).

Limitations

As with all studies, the present study does have some limitations. First, all meta-analyses are limited by the quality of the studies which are included within them. As noted, some methodological limitations are endemic within the field. There is some potential for effect sizes to be spuriously inflated by these issues. Second, the best practices analysis was limited by relatively low variance. Only the use of control variables varied significantly between studies and evidence suggested that this best practice approach may result in lower effect sizes. However, given the lack of variance, a full exploration of best practices effects was not as robust as hoped. Third, reflecting a wider issue within the field, the definition of "screen time" is vague and unsatisfactory, often including numerous conceptualizations and operationalizations and typically relying on self-report. Studies which used more precise measurements such as time diaries were very few in number. This issue is also reflected in the general screens predictor variable which, by nature, includes a wide range of divergent screen activities.

Concluding Thoughts

At present, there does not appear to be robust evidence to suggest that screen time is associated with, let alone a cause of, mental health problems. This applies to social media and smartphones specifically, as well as screen time generally. To the degree scholars and practitioners are focusing on screen time, particularly in relation to issues such as suicide, they are at high risk for following patterns of moral panic seen for other forms of media. This may further erode the confidence the public has regarding psychological science. We call upon our colleagues, whose good faith we do not doubt, to take a more cautious and conservative approach to making causal attributions regarding screens and mental health.

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