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Links between social media use and mental wellness in youth are an artifact of other factors: implications for public policy and meta-analysis

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Abstract

Recent years have seen considerable debate over the effects of social media on youth mental wellness. Thus far, evidence has been mixed. Some bivariate correlations show small covariances between social media and youth wellness in both directions, but these often disappear in multivariate analyses with theoretical controls. The current study examined this in a sample of several thousand youth from the United Kingdom. Consistent with prior research, small correlations were found between social media use and wellness, typically accounting for about 1–4% of the variance in various outcomes. However, also consistent with prior research, these correlations disappeared once theoretical control variables were employed in multivariate analyses. With such controls, social media accounted for effectively 0% of the variance in youth depression, anxiety, social phobia, mental wellness, quality of life, self-esteem and friendships. The only exception was for other activities, where social media use accounted for 4% of the variance in reduced activities (though typically closer to 1–2% for specific activities). This data raises the concern that reliance on bivariate correlations, which has occurred both in some books of pop psychology as well as meta-analyses, may misinform researchers and the public, upwardly biasing perceived effects. Further analyses should focus exclusively on multivariate effect sizes with theoretically relevant control variables. This data also conflicts with claims that social media use is associated with negative youth outcomes. Implications for public policy and for meta-analyses are discussed.

Keywords Social media · Adolescents · Mental health · Anxiety · Depression

The issue of social media's impact on youth has been the subject of debate for years now. Across the world, many countries such as Australia have either enacted or proposed bans on youth accessing social media, though such proposals are controversial and effectiveness is unclear. In the United States, several states such as Florida, California, Texas, Utah, and Ohio have enacted social media bans, although these have often been blocked by the courts over First Amendment concerns and lack of clarity whether such laws would be effective in promoting youth wellness. The debate has spilled into public consciousness, with popular psychology books such as the *Anxious Generation* (Haidt, 2024) promoting concerns about social media, though often being reviewed negatively by scholars (e.g., Ferguson 2025a; Gray 2024; Odgers 2024). As of yet, empirical research hasn't provided a clear answer to public concerns. Some studies find small correlations between social media use and youth wellness (in both positive and negative directions). However, some evidence also finds that, with the use of theoretical control variables, these correlations vanish. If this is true, those small correlations may be artifactual in nature rather than true effects. Public policy which is guided by bivariate effects may, as such, be misled. This study observes these issues in a large sample of youth from the United Kingdom, to examine the nuanced relationship between bivariate and multivariate effects and how the use or misuse of each may guide public policy and public narratives into uncharted and potentially dangerous waters.

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A brief history of social media and youth mental health research

Concerns about the potential impact of social media are nothing new. As early as 2011 the American Academy of Pediatrics (AAP) proposed the existence of a “Facebook Depression” concept (O’Keeffe, & Clarke-Pearson, 2011), wherein too much time on platforms such as Facebook, MySpace, and Twitter predicted depression[1]. However, at the time, this proposal was often received poorly, given concerns that AAP failed to cite studies that conflicted with this faux-diagnosis (Magid, 2011) and even some scholars who the AAP had cited complained their work had been used inappropriately (e.g., Davila, t2011).

If widely panned at the time, the AAP statement was nonetheless influential in setting the stage for broad causal claims, if arguably lacking nuance, and based upon weak data[2]. This may represent a significant missed opportunity. For example, around this time, some excellent work had been done examining nuances of *how* people used social media and how user patterns might influence both positive (Reinecke & Trepte, 2014) and negative outcomes (Davila et al., 2012). Yet these nuances largely evaporated as much of the international discussion and science with it shifted to a focus on time spent on social media as a risk factor for serious youth mental health outcomes, including gender dysphoria and suicide (e.g., PBS, 2024). 2017 saw social psychologist Jean Twenge ask in *The Atlantic*, “Have Smartphones Destroyed a Generation?[3]” (Twenge, 2017), concerns that would be echoed 7 years later in 2024’s *Anxious Generation* (Haidt, 2024). That, according to government statistics, youth of the 2010s were doing *better* on most outcomes such as violent crime, smoking, drug use, staying in school, pregnancy, etc. (childstats.gov, 2025) gave these concerns no pause. Indeed, with negativity bias, commentators, public officials and the general narrative focused on worsening mental health trends in the US, without crediting social media with positive trends on most other behavioral outcomes. Though causation does not equal causation, the cherry-picking of negative trends and ignorance of contemporary positive trends in youth behavior should arguably have raised red flags.

This may seem argumentative, and it should be clarified that no bad faith is implied. Normal human biases including the tendency to see correlation in ecological fallacies and blame technology for negative trends, while failing to credit that same technology for contemporary positive trends have replaced cautious empirical reasoning (Ferguson 2025a, b; Bowman 2016. However, this also shifted the focus of much of the research away from nuanced examinations of individual user motivations, to a one-size-fits all causal model in which mere exposure to social media was a dangerous risk factor for all youth. Once again, this need not, in and of itself, imply such concerns are de facto wrong…indeed the simpler hypothesis is worth testing as much as more nuanced testing. Yet it is possible for larger political and sociocultural narratives to influence the conduct of science.

Nonetheless, a decade and a half of research on social media effects have provided little clear evidence or consensus. Meta-analyses of studies of social media effects have either concluded that there are no correlations (e.g., Ferguson 2025; Yang & Feng, 2024) a mixture of very weak negative effects and null effects (Ahmed et al., 2024), both small positive and negative findings that are largely trivial (Liu et al., 2024), or even small positive effects on mental health, albeit also likely trivial in effect size (Godard & Holzman, 2023). In experiments, reducing social media time does not appear to improve mental wellness, though the quality of such experiments tends to be low (Ferguson, 2024; Lemahieu et al., 2025). Liu and Marciano (2024) in their analysis of social media use and youth wellness concluded “Results pointed towards minimal or no effects, challenging the moral panic on the detrimental impact of SMU on teen well-being.”

Despite heterogeneity between studies, meta-analysis either find zero or near-zero effects. The difference appears to be regarding whether meta-analyses employed bivariate correlations, with no theoretical controls, or standardized regression coefficients with theoretical controls or experiments. The latter categories are more likely to find null effects. As such, the use of bivariate correlations in meta-analyses may artificially inflate confidence in results, despite even these effect sizes being tiny. More caution should likely be used in the overuse and overinterpretation of bivariate correlations.

Theoretical controls

Given the potential misinterpretation of bivariate correlations, it may help to better understand which control variables are theoretically important. Several may arguably

[1] The term “Facebook Depression” is almost quaint 15 years later, given that Facebook is mainly popular with the very audience of older adults who tend to panic over new technology, and one of the other platforms listed, MySpace, has largely vanished from relevance.

[2] As of this writing, on PsycINFO, the AAP statement has been cited 1903 times.

[3] The generation in question was GenZ. Prior, Dr. Twenge claimed that the Millennial generation had been experiencing a “Narcissism Epidemic” itself often blamed on social media when youth self-esteem appeared to be rising (which was conveniently bad then, as declining self-esteem is bad now). However, the narcissism epidemic appears largely forgotten now, as Millennials have grown into parents with their own concerns (and money to buy books with), and the narcissism epidemic proved difficult to replicate by other scholars (e.g., Donnellan et al., 2009; Wetzel et al., 2017).

be valuable. These may include personality issues such as neuroticism or emotional regulation. Individuals with lower emotional regulation may both feel more stress and tend to turn more to social media when under stress. By contrast, youth with more resilience may be less inclined to use social media when stressed and, overall, respond to stress better than their less resilient peers. A sense of social connection or belonging may also be a critical control variable. Interestingly, evidence does not suggest that use of social media impedes real life relationships (Steinsbekk et al., 2024). Other variables such as family or school stress may also play a role.

As such, it is important that models examining social media effects, including within meta-analysis, employ reasonable theoretical controls. Caution should be used both in regard to overspecified (using variables nearly identical to social media time, such as controlling for smartphone time) and underspecified (using control variables that are theoretically irrelevant) models.

The current study

The current study examines correlations between social media time and outcomes related to depression, anxiety, mental wellbeing, quality of life, self-esteem, social phobia, friendships and real-life activities. Control variables include emotional regulation and resilience, school connectedness and social belonging, as well as age and sex. These analyses are conducted with a large sample of youth using archival UK data. Hypotheses are that connections between social media use and outcome variables will persist after including control variables.

Methods

Data source

The source of data is the UK BrainWaves Project dataset. This dataset provides access to researchers regarding a large cohort of United Kingdom adolescents. Access to the data is controlled by DPUK (Dementias Platform UK) in cooperation with BrainWaves Project and is provided free of charge with application for a time-limited basis.

Note that an initial preregistration was done based on the data list, but without seeing the final data file. As such there are some differences in variables, both predictor and outcome (essentially fewer of each, particularly were others were not available as summed scale scores). The current analysis makes use only of variables that had been calculated by BrainWaves Project. Also, the current data is cross-sectional, as the longitudinal data are not yet available. These differences were made prior to seeing any data analyses. The preliminary preregistration is available at: https://aspredicted.org/see_one.php.

Participants

Participants in the current study were 15,443 adolescents (boys = 6033, 39.1%, girls = 8746, 56.6%, the remaining 4.3% did not report biological sex). Mean age was 16.67 (*SD* = 0.77). Participants were recruited from among UK schools and will be followed for several years. This paper reports on cross-sectional data from the first year of data collection.

Materials

Predictor variables

Note, all scales used below used full-scale scores calculated by BrainWaves Project unless otherwise indicated. As such, coefficient alphas were not available.

Social media time Social media time is reported simply as hours per day reported time on social media.

Emotional regulation Emotional regulation was measured using the Trait Emotional Intelligence Questionnaire (TEIque, Petrides et al., 2007, **reported internal consistency 0.89–0.91.89.91**). BrainWaves Project used a 6-item brief scale from this questionnaire to measure emotional regulation.

Resilience Similar to emotional regulation, resilience can indicate an ability to cope with stress. Resilience was measured using the 6-item Brief Resilience Scale (BRS; Smith et al., 2008. **Reported internal consistency 0.80–0.91.80.91**).

School connectedness scale A 6-item version of a scale of school connectedness (SCS; Resnick et al., 1997. **Internal consistency for the present sample was 0.70**) was used. This survey was used to control for social connections and social support within the school environment.

Social belonging Social belonging was used as a control variable regarding social support in wider contexts beyond the school. This was measured using the Belongingness scale (Goodenow, 1993, **internal consistency reliability reported as 0.82–0.88.82.88**).

Outcome variables

Depression and anxiety Depression and Anxiety were assessed using the Revised Children's Anxiety and Depression Scale (RCADS-11; Radez et al., 2021, **reported internal reliabilities were 0.87–0.94.87.94 in community samples**). This 11-item measure is a brief screener for mood and anxiety disorders in youth.

Mental wellbeing Mental Wellbeing was assessed using the Short Warwick Edinburgh Mental Wellbeing Scale (SWEMWBS; Koushede et al., 2019, **reported internal consistency was 0.88**). This study uses the brief 7-item version of the scale.

Quality of life Quality of life (QoL) was assessed using the Brunnviken Brief Quality of Life Scale (BBQ; Lindner et al., 2016, **internal reliability reported as 0.76**). This is a brief, 6-item version of the scale.

Self-esteem Self-esteem was measured using the 10-item Rosenberg Self-Esteem Scale (RSE; Rosenberg, 1979, **reported internal consistencies between 0.80 and 0.88**).

Social phobia Social phobia was measured using the Mini-Social Phobia Inventory (Mini-SPIN; Connor et al., 2001, Wiltinik et al., 2017, **reported reliabilities between 0.80 and 0.83**). This is a brief 3-item screener for social phobia.

Friendships A brief 5-item scale of the McGill Friendship Questionnaire (MFQ, Mendelson, & Aboud, 1999, **reported reliabilities 0.84–0.96.84.96**) was used to access friendship quality.

Real-life activities Participants were asked to respond to whether, when not in school, they were involved in a variety of activities ranging from religious services, to watching sporting events, to reading. One item, involving video game playing, was not included given this was another example of screen use. The remaining 10-items had a coefficient alpha of 0.60. Given the wide range of activities, a lower alpha is not unexpected and the scale still reasonably represents involvement in non-screen activities.

Smallest effect size of interest (SESOI)

The current study employs a very large sample. Large samples are excellent for generalizability and power. However, large samples often run the risk of false positive results when p-values are overutilized. This relates to the notion of "crud" in social science research where, on some level, actual correlations of r =.00000 (infinite) are actually rare, but most correlations that are near zero are meaningless or statistical noise (Meehl, 1991; Orben & Lakens, 2020). One analysis (Ferguson & Heene, 2021) was able to provide an estimate of likely "crud" or "noise" in large datasets. In effect, for effect sizes below r =.10, the ability to distinguish real from noise/crud effects was largely non-existent, meaning that the reliability of such effect sizes to represent true population effect sizes was lacking. This problem persisted, albeit with lesser frequency to approximately r =.20. As such this study will adopt a minimal threshold of r =.10 for hypothesis support with r =.20 for confident support or practical significance.

Data analysis

All data analyses were conducted with SPSS software. All analyses were performed on a virtual time-limited desktop provided by BrainWaves Project, with secure access. Hierarchical multiple regression was used. Sex and age were entered on step 1, the other control variables on step 2, with social media time on step 3. Pairwise deletion was used for missing data.

Results

Bivariate relationships

Table 1 presents bivariate correlations between social media use and all outcome variables, with Bonferroni corrections used for multiple comparisons as well as the established

Table 1 Bivariate correlations between social media hours and all predictors

Predictor	Depression	Anxiety	SP	QOL	Mental W	SE	Friends	Activities
Social Media Hours	.211*$_{(.19,.23)}$	.160*$_{(.14,.18)}$	.150*$_{(.13,.17)}$	-.127*$_{(-.13,-.17)}$	-.176**$_{(-.20,-.16)}$	-.182*$_{(-.20,-.16)}$	.007$_{(-.03,\ .01)}$	-.233*$_{(-.24,-.21)}$

SP = Social Phobia, QOL= Quality of Life, SE = Self-Esteem. * = statistically significant and also meets SESOI (at r = .10). Bonferroni corrections (p = .006) were used for multiple correlation models. Numbers in parentheses indicate 95% confidence intervals

SESOI. With the exception of friendships, most outcomes had small correlations with social media hours. Most of these were in the cautionary zone of the SESOI (0.1 to 0.2) meaning false positives are possible but less likely than for under 0.10. Only depression and activities exceeded the upper SESOI of 0.20. However, the question is whether these correlations remain, once several child trait variables are controlled.

Multivariate relationships

As noted earlier, all regression models were hierarchical, with 3 models. The first included only demographic variables, Model 2 included child trait control variables and Model 3 included social media hours. Note, that the highest VIF (Variable Inflation Factor) was 2.017 indicating a general absence of multicollinearity issues in the models.

For Depression, the third model was statistically significant [R =.687, $R^2 = 0.471$, $F(7, 4434) = 564.65$, p <.001). As indicated in Table 2, trait variables of emotional regulation, resilience and belonging were correlated with depression, but social media hours were not.

For Anxiety, the second model was statistically significant [R =.696, $R^2 = 0.484$, $F(6, 4435) = 693.23$, p <.001) but the R^2 change for the third model including social media hours was not. As indicated in Table 2, trait variables of emotional regulation, resilience and belonging were correlated with anxiety, but social media hours were not. Anxiety was also higher among girls than boys.

For Social Phoba, the second model was statistically significant [R =.519, $R^2 = 0.270$, $F(6, 4400) = 270.98$, p <.001) but the R^2 change for the third model including social media hours was not. As indicated in Table 2, trait variables of emotional regulation, resilience and belonging were correlated with social phobia, but social media hours were not.

For Quality of Life, the second model was statistically significant [R =.584, $R^2 = 0.340$, $F(6, 4392) = 377.89$, p <.001) but the R^2 change for the third model including social media hours was not. As indicated in Table 2, trait variables of emotional regulation, resilience, school connectedness, and belonging were correlated with quality of life, but social media hours were not.

For Mental Wellbeing, the second model was statistically significant [R =.693, $R^2 = 0.480$, $F(6, 4435) = 682.33$, p <.001) but the R^2 change for the third model including social media hours was not. As indicated in Table 2, trait variables of emotional regulation, resilience, school connectedness and belonging were correlated with mental wellbeing, but social media hours were not.

For Self Esteem, the second model was statistically significant [R =.697, $R^2 = 0.486$, $F(6, 4415) = 694.55$, p <.001) but the R^2 change for the third model including social media hours was not. As indicated in Table 2, trait variables of emotional regulation, resilience and belonging were correlated with self-esteem, but social media hours were not.

For Friendships, the third model was statistically significant [R =.387, $R^2 = 0.158$, $F(7, 4359) = 116.62$, p <.001). As indicated in Table 2, female sex, school connectedness and belonging were correlated with friendships, but social media hours were not.

For Activities, the third model was statistically significant [R =.264, $R^2 = 0.070$, $F(7, 4434) = 116.62$, p

Table 2 Main standardized regression coefficients for all outcomes for all predictors

Predictor	Depression	Anxiety	SP	QOL	Mental W.	SE	Friendships	Activities
Age	.023(-.01, .05)	.015(-.01, .04)	.004(-.02, .03)	.020(-.01, .05)	-.014(-.04, .02)	.004(-.03, .03)	.009(-.02, .04)	-.051 (-.08,-.02)
Sex	.068(.04,.10)	.185*(.16,.21)	.081(.05,.11)	.074(.04,.10)	-.028(-.06, .00)	-.002(-.03, .03)	.165*(.14,.19)	.030 (-.00,.06)
Emotional Regulation	-.262*(-.29, -.23)	-.297*(-.32, -.27)	-.198*(-.23, -.17)	.191*(.16,.22)	.310*(.28,.34)	.269*(.24,.30)	.051(.02,.08)	.019 (-.01, .05)
Resilience	-.225*(-.25, -.20)	-.267*(-.29, -.24)	-.234*(-.26, -.21)	.108*(.08,.14)	.242*(.21,.27)	.296*(.27,.32)	.052(.02,.08)	.049 (.02,.08)
School Connectedness	-.055(-.08,-.03)	-.007(-.03, .02)	.000(-.03, .03)	.208*(.18,.24)	.162*(.13,.19)	.074(.04,.10)	.215*(.19,.24)	.075 (.05,.10)
Belonging	-.300*(-.32,-.28)	-.205*(-.23, -.18)	-.176*(-.21, -.15)	.262*(.24,.29)	.175*(.15,.20).	.256*(.23,.28)	.162*(.13,.19)	.005 (-.02, .03)
Social Media Hours	.056(.03,.09)	.002(-.02, .03)	.034(.00,.06)	-.003(-.03, .03)	-.013(-.04, .02)	-.023(-.05, .01)	.054(.02,.08)	-.211* (-.24, -.18)

SP = Social Phobia, QOL= Quality of Life, SE = Self-Esteem. * = statistically significant and also meets SESOI (at β = .10). Bonferroni corrections (p = .006) were used for multiple regression models. Standardized regressions reported here are for model 3, including social media hours, even if this step was non-significant. Numbers in parentheses are 95% confidence intervals

<.001). As indicated in Table 2, only social media hours predicted activities. As exploratory follow up analyses, the regression was rerun on individual activities. Social media predicted reduced involvement in reading for fun, going to youth clubs (marginally), going to museums or galleries, participation in sports, other hobbies, meditation or praying, but not going to the cinema or movies, watching live sports, singing in a choir or band, attending religious services, involvement in art, or playing video games. All individual effect sizes were low, even when surpassing the SESOI, suggesting that social media time was associated with a 1–2.25.25% reduction in other activities at most.

Sensitivity testing

All regressions were rerun with listwise and replace with mean options for missing data. These did not substantially change outcomes.

Discussion

The impact of social media on youth mental health continues to be stridently debated in public and among academics. As of yet, no consensus has been reached about social media impact on youth. Some concerns have been raised that analyses which focus on bivariate correlations, including meta-analyses, may inflate confidence in hypotheses linking social media use to youth mental health. By contrast, small correlations may be artifacts of other trait or social issues occurring for youth, such as that more neurotic youth may both use more social media and may experience more mental health issues, without the former causing the latter. The current study tests this in a large sample of UK youth. Consistent with concerns, the evidence here finds that small correlations between social media hours and youth mental health vanish when several controls are employed.

The results presented here are consistent with prior meta-analyses, as discussed in the literature review. Namely, small correlations can be observed between social media use hours and youth self-reported mental health issues. These bivariate correlations typically explain between 1 and 4% of the variance in youth outcomes, with the exception of friendships. However, bivariate correlations may be artifactual. Indeed, the current results are consistent with meta-analyses employing standardized regression coefficients (e.g., Ferguson 2025, Yang & Feng, 2024) which find that these bivariate correlations approximate 0 once a few theoretical controls are employed.

Social media correlations are artefactual

These results support the notion that small correlations in self-reported social media use and mental health are artefactual in nature. The current analyses suggest that trait issues related to emotional regulation and resiliency, arguably opposites of neurotic personality traits, as well as belongingness and, in some cases, school connectedness are key variables of youth success, not social media use. In these cases, it may be that resilient, emotionally regulated youth feel less need to use social media as much. By contrast, neurotic youth who are experiencing more problems may turn to social media to feel better. By focusing on time spent on social media we may be "blaming the messenger" and ignoring more internal/proximal causes of child mental health issues.

There are other possible explanations. For instance, several studies have now pointed out that genetic factors appear to explain the small correlations between social media use and youth mental health (Ayorech et al., 2023; Sametoğlu et al., 2025). Rather than social media use causing mental health issues, common genetic factors are related to both. This is not inconsistent with the model offered above, by which genetic factors may cause trait neuroticism which in turn causes mental health problems. In this model, social media use is a mere byproduct of this chain as youth use social media to feel better.

There are two cautionary notes from this data. First, even the small bivariate correlations are based on self-report data. Issues related to demand characteristics, single-responder bias, and a tendency for more mentally unwell people to overestimate their screen time may have caused false positive results. Further, evidence suggests youth may over-report mental health symptoms as well, leading to false positive results (Scheeringa, 2025). Second, even taken at face value, bivariate correlations are small, and most were within the medium-risk range for false positive results.

Most mental health variables work similarly

One interesting finding is that mental health variables including depression, anxiety, social phobia, quality of life, and mental wellness as well as self-esteem tend to function together in concert. Indeed, intercorrelations between these constructs were very high ranging from a low of $r=-.363$ for social phobia and quality of life, to a high of $r=-.731$ for depression and self-esteem. Depression and anxiety correlated $r=.708$, for instance. These intercorrelations are so high they would be sufficient for construct validity, arguing that these are all elements of the same construct. This is consistent with other results (Mahmoud et al., 2012). Further, this suggests that separating these out into individual micro-variables may result in capitalization on chance, false positives, and potential p-hacking. By contrast, this validates the use of these constructs together such as in meta-analysis (e.g., Ferguson 2025a, b).

Further evidence for their similarity comes from the same control variables predicting them as outcomes and at roughly the same magnitude. The only exception was school connectedness which predicted some variables but not others, and female sex which predicted only anxiety.

In contrast to this, both friendships and particularly activities appear to work independently of these other constructs. Their correlations with other outcomes were far weaker (typically below r =.30), and their pattern of prediction among the covariates differed from the others. Activities, in particular, appeared entirely independent of all other outcomes in its pattern of prediction. Only social media hours predicted activities, unique among the variables. The lack of prediction between social media hours and friendships is consistent with prior research (e.g., Steinsbekk et al., 2024). However, the inverse correlation between social media and other activities is somewhat in contrast to prior research which concluded that smartphones, at least, mainly took time away from television, not other activities (Röhlke, 2025). There are several possibilities for these inconsistencies. One possibility, though less likely, is the mediums of social media and smartphones are different enough to produce different results. Given social media is often used on smartphones, this seems less satisfying an explanation. Perhaps more likely, it may be that Röhlke (2025) examined more general activities such as exercise and hobbies which can be done alone, whereas the current analysis focused more on social activities such as church and clubs. It may be that social media use is more common among those that prefer more solitary activities, from which social media does not distract. The effect size was also small and correlational, so this does not appear to be enough for significant concern as yet.

Implications for meta-analysis

These results appear to confirm one serious concern for meta-analysis. Specifically, the use (or misuse) of bivariate correlations in meta-analyses of correlational and longitudinal studies is likely to artificially inflate effect size estimate, providing unreasonable confidence in hypotheses that, in fact, are poorly supported.

Use of bivariate correlations in meta-analysis appears to be the product of a misimpression that such correlations are homogeneous and, as such, more comparable. This makes sense as different regression models may use different control variables, introducing heterogeneity among standardized regression coefficients. However, for disparate correlational and longitudinal studies, this assumption has largely proven to be false. In fact, for a variety of reasons, bivariate correlations have higher heterogeneity than do standardized regression coefficients, with considerable evidence suggesting that, in addition to being theoretically superior, standardized regression coefficients are also statistically superior to bivariate correlations for meta-analysis (e.g. Furuya-Kanamori & Doi, 2016; Pratt et al., 2010; Savage & Yance, 2008).

To the extent that different regression models use different control variables, this can be addressed through moderator analysis in meta-analysis. In effect, meta-analysists can examine the impact of different control variables on effect sizes via moderator analysis. Given that heterogeneity statistics tend to be high for most social science metas, more emphasis on moderator analysis and less emphasis on pooled effect sizes is more useful anyway.

As such, the recommendation for meta-analysis is to cease the use of bivariate correlations. Instead, standardized regression coefficients should be analyzed instead. Bivariate correlations should never be interpreted as indicating population effect sizes, nor relied upon for hypothesis support. Further, meta-analysis should employ a SESOI as recommended here and elsewhere (Ferguson & Heene, 2021) in order to avoid false positive conclusions from statistical noise.

Implications for policy

The current study provides no evidence for the belief that social media use, at least in terms of raw hours, is predictive of youth mental health. Given various biases in self-report data (single-responder bias, demand characteristics, overreporting of social media hours and mental health severity, etc.), the effect sizes from this study are more likely to be overestimates of population effect sizes than underestimates. Yet, even taken at face value, they provide no evidence in support of government policies seeking to restrict access to social media in order to promote mental health. Although rigorous testing of policies such as Australia's national under-16 social media ban have yet to be undertaken, this may explain why adjacent policies such as school cellphone bans appear to be failing (e.g., Goodyear et al., 2025)[4].

It is recommended that policy makers hold off on further attempts to restrict social media for youth, given lack

[4] Advocates for such policies, particularly in news media, have sometimes highlighted unpublished studies of such bans as evidence for their effectiveness. However, a close look at these studies reveals that they, too, typically have near zero effect sizes (rs of 0.02 or so), that provide no actual evidence for the utility of such policies.

of evidence this is a useful policy. Evidence we have now suggests that restricting social media time does not improve mental health (Ferguson 2025a, b; Lemahieu, 2025)[5]. Further, restricting youth access to information and socialization may actually backfire, causing more problems than they help. In the United States, such policies have generally been found unconstitutional, though free speech issues are not a barrier for other countries such as Australia. As such, it may be worth holding off for a rigorous (as opposed to politically perfunctory) evaluation of Australia's policy before considering any others.

At present, in the United States, according to Centers for Disease Control data, youth mental health has been improving, despite no change in youth access to social media. Hopefully that pattern will hold. In other countries, increased social media use was never associated with a rise in significant youth mental health problems (Ferguson 2025a, b). As such, many of the policies currently attempted cross-nationally may have more to do with moral panic than good policy or sound science.

Limitations

Some of the limitations of this dataset have been noted earlier. The data are cross-sectional and correlational. Longitudinal data would be welcome, and it appears likely that BrainWaves Project will, in future years, provide such data. Various vagaries of self-report data may distort effect sizes, though this is more likely to be inflationary than truncating. Not all potential control variables, such as family stress, were available in the dataset. Likewise, genetic data were not available. The current study also focuses on social media hours. This does not provide insight into *how* different youth use social media, which may provide more nuanced understanding.

Future directions

The current sample is cross-sectional and correlational in nature. It is possible that longitudinal follow-ups may provide more data regarding the long-term associations between social media use and youth wellness outcomes with other factors controlled. For instance, it is possible that although social media does not cause mental health issues, there could be a longer-term feedback effect. Longitudinal analyses may help to elucidate this issue, though once again, it is critical that theoretically relevant control variables be included in all future analyses.

[5] Though, it is worth noting, the effectiveness of research designs to test this hypothesis are also quite limited.

Conclusions

Increasingly, it is evident that an overreliance on bivariate correlations may be causing confusion and misinformation in this research realm. The current analyses suggest that small correlations between social media use and most outcomes vanish with only a few child trait control variables. Ultimately, data from these analyses provide no evidence for the level of alarm regarding social media, nor claims of dramatic transformation in children's lives should social media exposure be reduced (e.g., Haidt, 2024). Instead, these results suggest that such dramatic claims are more akin to moral panics over other media and technology ranging from the radio to video games. These moral panics were, likewise, often abetted by hyped but weak social science. Only with greater rigor and critical thinking will we have a fuller understanding of the nuances of youth social media use.

Author contributions Christopher J. Ferguson is solely responsible for writing and data analysis on the current manuscript. He is responsible for the scientific integrity of this manuscript. All data were provided via BrainWaves Project 20016 who did all survey design and data collection and initial calculation of main variables. DPUK (Dementias Platform UK) provided data/sample/ participant access for this project.

Data availability All data are owned by BrainWaves Project. Data can be obtained from DPUK via application.

Declarations

Ethical standards As data were archival, this study was exempt from local Stetson University IRB. Original IRB approval was obtained by BrainWaves at Oxford University.

Accordance statement/informed consent All research described within passed local IRB and was designed to comport with federal standards for human participants research included proper informed consent. All consent issues were handled by BrainWaves Project.

Conflict of interest The author once consulted with the free speech group Foundationfor Individual Rights in Education on a social media case. He has no connections of any kind tosocial media companies.

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