

Review Article

Association Between Social Media Use and Depressive and Anxiety Symptoms Among Adolescents and Young Adults: A Meta-Analysis of Systematically Searched Evidence

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ABSTRACT

Background: Social media use among young people has risen sharply, and concern about its relationship to mental health is widespread. The evidence base remains heterogeneous in effect size, measurement, and design.

Objective: To estimate the association between social media use and depressive and anxiety symptoms among adolescents and young adults, and to test whether the two differ.

Methods: Four databases were searched from January 2010 to April 2021. Eligible studies reported a quantitative association between a social media exposure and a validated depression or anxiety measure. Stress subscales were not treated as anxiety outcomes. Effect sizes were pooled as Pearson correlations using random-effects models.

Results: The pooled association with depressive symptoms was $r = 0.263$ (95% CI: 0.200-0.324; $k = 10$; $N = 5,774$; $I^2 = 83.9\%$), prediction interval 0.031-0.468. For anxiety it was $r = 0.363$ (95% CI: 0.265-0.453; $k = 7$; $N = 5,286$; $I^2 = 93.3\%$), prediction interval -0.009-0.646. A paired comparison across the five samples contributing to both outcomes found no difference ($p = .77$). Mean sample age predicted the depression association ($b = 0.045$ per year; $p = .023$) and accounted for almost all heterogeneity. Restricting to samples with mean age below 19 reduced the depression estimate to $r = 0.196$.

Conclusions: Social media use is positively associated with depressive and anxiety symptoms, at a small-to-moderate magnitude and with no detectable difference between outcomes. The anxiety prediction interval includes zero, and the association is weaker in adolescent samples. The cross-sectional evidence precludes causal inference.

Keywords: Social Media; Depression; Anxiety; Adolescents; Young Adults; Meta-Analysis; Internalizing Symptoms

Introduction

Social media platforms have become central to the daily lives of young people across income settings. Daily use is close to universal among adolescents in high-income countries, and rapid smartphone diffusion has produced comparable rates of use in many low- and middle-income contexts [1,2]. The average young person spends between two and four hours per day on these platforms, and the figure continues to rise [3]. These platforms also serve recognised developmental functions, supporting the maintenance of peer relationships, the exploration of identity, and access to social support networks [4]. Over the same period, reported depression and anxiety among young people has increased in several countries, which has prompted sustained scientific and policy interest in whether the two trends are connected [5].

The empirical literature that has accumulated over the past decade is large and inconsistent. A number of cross-sectional studies report significant positive associations between social media use and depressive symptoms [6-8], while others report no significant relationship [9]. Associations vary in magnitude and direction, in the subgroups in which they appear, and in whether they persist after adjustment for co-occurring factors including sleep quality, offline social interaction, and pre-existing mental health conditions [10,11]. Existing systematic reviews and meta-analyses have generally reported weak and inconsistent links between social media use and depression [12,13].

One recognised source of this variation is the distinction between general social media use, typically operationalised as time or frequency of use, and problematic or addictive use, measured with validated instruments that assess compulsive engagement, withdrawal symptoms, and functional impairment [14,15]. Both constructs have been linked to mental health outcomes. Cunningham et al. [16] reported a stronger concurrent association between problematic social media use and depressive symptoms than between general use and depressive symptoms, and Shannon et al. [17] reported comparable patterns for both depression and anxiety in a synthesis restricted to validated problematic-use scales. These findings indicate that estimates pooling across all types of social media use may understate the mental health risk concentrated among the minority of young people who use these platforms compulsively.

There are developmental reasons to expect heightened susceptibility during adolescence and the transition to adulthood. Sensitivity to peer evaluation and social comparison is elevated during this period, and interacts with platform architectures that quantify

peer feedback in real time. This occurs alongside continuing maturation of the prefrontal cortex and reward circuitry, and a normative increase in identity-seeking behaviour [18]. Problematic use is common in this age band, with national estimates indicating that between 5% and 15% of adolescents meet criteria for problematic use [15]. Depression and anxiety disorders also have their peak period of onset in this window, with approximately half of all lifetime mental disorders beginning by the mid-teens, coinciding with the developmental stage at which exposure to evaluative online environments is likely to be most consequential [19,5].

Three areas of uncertainty motivate the present review. First, prior meta-analyses have generally examined depression or anxiety separately, or have collapsed them into a composite internalizing measure, which prevents direct comparison of the two outcomes within a common evidence base [20,10]. Where reviews have reported both, the comparison has typically been informal rather than formally tested. Second, the literature is concentrated in high-income settings, leaving considerable uncertainty about generalisation to young people in Sub-Saharan African, South Asian, and Latin American populations [21]. Third, substantial heterogeneity has been reported consistently across syntheses in this field without being formally explained, leaving open the question of whether it reflects genuine population differences or artefacts of sample composition and measurement.

The present study addresses these gaps by pooling depressive and anxiety symptoms as separate primary outcomes within a single evidence base, comparing them using a method appropriate to their statistical dependence, and interrogating heterogeneity through meta-regression rather than describing it narratively. We hypothesised a priori that social media use would be positively and significantly associated with both outcomes, that associations would be larger for problematic than for general use, and that effect sizes would vary substantially across studies [22,17].

Methods

Registration and Reporting

Reporting followed the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) 2020 guidelines [23], and the protocol was registered in advance in the International Prospective Register of Systematic Reviews (PROSPERO; registration number CRD42024421214. Because the analysis as conducted departs from the registered protocol in several respects, all deviations are recorded in full in a dedicated table at the end of the Methods rather than described in passing, so that readers can assess the extent to which the reported analyses were pre-specified.

Eligibility Criteria

Studies were selected using a PECOS framework. Because this analysis synthesises observational research, the “I” (Intervention) of the conventional PICOS framework is replaced by “E” (Exposure), consistent with guidance in Chapter 24 of the Cochrane Handbook for non-randomised studies of interventions.

The population comprised adolescents and young adults recruited from community-based, school-based, or university-based samples, of any sex, gender, or nationality. Studies restricted to clinical inpatient psychiatric samples were excluded. Following protocol deviation D1, the registered upper age limit of 19 years was relaxed, because the great majority of eligible studies were conducted in samples extending into emerging adulthood. Age composition is reported for every included study and an age-restricted sensitivity analysis is presented.

The exposure comprised any measure of social media use, including general frequency of use, daily duration, passive versus active use, platform-specific use, number of platforms used, and problematic or addictive use measured with a validated scale. Studies that measured only general internet use or total screen time, without a specific social media component, were excluded. The comparison condition was low or no social media use, or alternatively a continuous exposure measure permitting extraction of a quantitative effect size, since the great majority of studies in this literature report continuous associations rather than group contrasts.

Primary outcomes were depressive symptoms (PHQ-9, CES-D, BDI, CDI, DASS-21 depression subscale) and anxiety symptoms (GAD-7, STAI, SCARED, MASC, DASS-21 anxiety subscale), each measured with a validated instrument. Stress subscales, including the DASS-21 stress subscale, were not treated as anxiety outcomes at any stage. The secondary outcome of psychological distress specified in the protocol is not reported, as too few studies provided a poolable estimate (deviation D2). Eligible designs were cross-sectional surveys, prospective cohort studies, and randomised controlled trials published in English between January 2010 and April 2021. Randomised trials remained eligible throughout, but none contributing a usable effect size were identified, so the planned application of the revised Cochrane Risk of Bias tool was not required (deviation D3). The full set of inclusion and exclusion criteria is set out in Table 1.

Table 1. Inclusion and Exclusion Criteria Based on the PECOS Framework

PECOS component	Inclusion criteria	Exclusion criteria
Population	Adolescents and young adults; any sex or gender; any country; community-based, school-based, or university-based samples	Studies restricted to clinical inpatient psychiatric populations; children under 10 years
Exposure	Any measure of social media use: frequency, daily duration, passive/active use, platform-specific use, number of platforms, or problematic/addictive use via validated scales (e.g., BSMAS, SAS-SV)	General internet or screen time only, without a specific social media component; social media used as a therapeutic intervention
Comparison	Low or no social media use; or a continuous variable permitting extraction of a Pearson r or convertible effect size metric	Studies without a quantitative comparator or any reportable effect size
Outcomes	Depressive symptoms (PHQ-9, CES-D, BDI, CDI, DASS-21 depression) or anxiety symptoms (GAD-7, STAI, SCARED, MASC, DASS-21 anxiety)	Outcomes measured with non-validated instruments; stress subscales; outcomes limited to composite well-being, life satisfaction, or self-esteem without a validated depression or anxiety measure

PECOS component	Inclusion criteria	Exclusion criteria
Study design	Cross-sectional, prospective longitudinal or cohort, randomised controlled trial; English language; January 2010 to April 2021	Qualitative studies, case reports, editorials, conference abstracts without full data, preprints and other non-peer-reviewed reports, non-English publications

Note. BSMAS = Bergen Social Media Addiction Scale; CES-D = Center for Epidemiological Studies Depression Scale; DASS-21 = Depression Anxiety Stress Scales; GAD-7 = Generalized Anxiety Disorder-7; PHQ-9 = Patient Health Questionnaire-9; SAS-SV = Social Media Addiction Scale, Student Version; STAI = State-Trait Anxiety Inventory.

Information Sources and Search Strategy

Systematic searches were conducted in four electronic databases: PubMed/MEDLINE, PsycINFO (EBSCOhost), Web of Science (Core Collection), and Scopus. The search strategy was constructed from three conceptual blocks: population (adolescent, teenager, youth, young adult, student), exposure (social media, social networking site, Instagram, TikTok, Facebook, Snapchat, YouTube), and outcome (depression, depressive symptoms, anxiety, anxiety symptoms). Database-specific controlled vocabulary, comprising MeSH terms in PubMed and APA Thesaurus terms in PsycINFO, was combined with free-text title and abstract terms. No geographical restriction was applied. Searches were supplemented by a Google Scholar search (first 200 results by relevance) and by backward and forward citation chaining of four a priori anchor reviews: Shannon et al. [17], Fassi et al. (2024), Saleem et al. (2024), and Ahmed et al. (2024). Full search strings for each database, exactly as executed, including all controlled vocabulary, field tags, Boolean operators, and applied limits, are provided in Supplementary Table S1, together with the number of records retrieved from each source before deduplication, so that the search can be reproduced in full.

Study Selection

All retrieved records were exported to a reference manager and deduplicated automatically and by hand. Titles and abstracts were screened independently and in duplicate by two reviewers, and all potentially eligible records proceeded to independent full-text assessment by the same two reviewers. Disagreements were resolved by consensus, with a third reviewer available for arbitration where consensus could not be reached. Interrater reliability was quantified using Cohen's kappa at both the title and abstract stage and the full-text stage, with a value

of 0.70 or above specified a priori as acceptable [24]. Reasons for exclusion were recorded for every record excluded at full text.

Data Extraction

Data were extracted by one reviewer using a standardised form and independently verified by a second reviewer. Extracted domains and variables are summarised in Table 2. For every study, the following were recorded in addition to the standard fields: minimum and maximum participant age, mean age and standard deviation, the exact outcome subscale used, the exposure instrument, whether the reported estimate was crude or adjusted, and the table or page number from which the estimate was taken. Where a study reported associations for multiple platforms, outcomes, or subgroups, all were recorded for narrative synthesis, but only one estimate entered each meta-analysis, selected according to an a priori rule: the total-sample, unadjusted, whole-scale estimate for the primary outcome in question. This rule avoids unit-of-analysis error while making the selection process transparent and reproducible.

Effect sizes reported in metrics other than the Pearson correlation coefficient were converted as follows. Standardised beta coefficients were treated as approximations of r only where the source model contained a single predictor, and were otherwise not used. Odds ratios were converted to Cohen's d using the Chinn transformation and then to r . Test statistics from two-group comparisons were converted to d and then to r . Adjusted estimates were not pooled together with crude estimates. Studies reporting only multivariable-adjusted coefficients from multi-predictor models were excluded from quantitative pooling and retained for narrative synthesis, because such coefficients are not comparable to zero-order correlations.

Table 2. Data Extraction Domains and Variables

Domain	Variables extracted
Study identification	First author, year, country, journal, registration status
Sample characteristics	Total n, minimum and maximum age, mean age (SD), % female, sampling method, study design, recruitment setting

Domain	Variables extracted
Exposure measurement	Platform(s), exposure type (frequency, duration, passive/active, problematic use), scale name and validation status, mean exposure
Outcome measurement	Outcome domain, exact subscale, scale name and citation, Cronbach's alpha, mean symptom score (SD)
Effect size data	Pearson r or convertible statistic, SE or 95% CI, n for the association, adjustment status (crude or adjusted), direction of association, source table or page

Risk of Bias Assessment

The methodological quality of included studies was evaluated independently by two reviewers using design-specific tools. Cross-sectional studies were assessed with the Newcastle-Ottawa Scale adapted for cross-sectional designs (NOS-CS; 10 points), covering the domains of sample representativeness, comparability, and ascertainment of outcomes [25]. Longitudinal cohort studies were assessed with the standard NOS cohort version (9 points). Studies scoring 7 or above were classified as low risk, those scoring 5 to 6 as moderate risk, and those scoring below 5 as high risk. Study-level scores by domain, together with a traffic-light summary plot, are provided in Supplementary Table S2.

Statistical Analysis

All effect sizes were expressed as or converted to Pearson correlation coefficients. Values were transformed to Fisher's z for pooling and back-transformed to r for presentation. Random-effects meta-analysis used the DerSimonian-Laird estimator of between-study variance [26]. Statistical heterogeneity was quantified with Cochran's Q and the I^2 statistic, with values of 0 to 24%, 25 to 49%, 50 to 74%, and 75 to 100% interpreted as low, moderate, substantial, and considerable respectively [27]. Ninety-five percent prediction intervals were calculated for every pooled estimate using the method of Higgins, Thompson, and Spiegelhalter, and are reported throughout. Under substantial heterogeneity the confidence interval around a pooled mean conveys precision about the average effect but says little about the range of true effects that might be observed in a new setting, which is the quantity of interest when the question is what a further study in a further population would find.

Publication bias was assessed using Egger's regression test and by trim-and-fill using the L0 estimator, with contour-enhanced funnel plots provided as supplementary figures. Because this test has limited power when fewer than ten studies are available, its results are interpreted as uninformative rather than as evidence that bias is absent [28]. Leave-one-out sensitivity analysis was conducted by omitting each study in turn. A further sensitivity analysis restricted the pool to samples with a mean age below 19

years, to test whether the pooled estimates were driven by samples extending into emerging adulthood.

Pre-specified moderators are listed in Table 3. Because the analytic pool contains between one and three studies in each geographic and instrument category, subgroup analyses could not support inference and are not reported. Heterogeneity was examined instead by random-effects meta-regression, which uses all studies simultaneously rather than partitioning them into thinly populated cells (deviation D5). Several samples contribute an estimate to both the depression and the anxiety analysis. The two pools are therefore not statistically independent, and a between-group test treating them as independent would be invalid. The comparison between outcomes was conducted as a within-study paired analysis restricted to samples reporting both, using a one-sample t test and a Wilcoxon signed-rank test on the within-study difference in Fisher's z.

All analyses were conducted in R using base functions, with no dependency on external meta-analytic packages. The complete analysis script, which reproduces every estimate reported below from the study-level data reported in the Results, is provided as Supplementary File S3 and deposited with the extraction sheet in the repository named in the Data Availability statement, so that any reader can regenerate the pooled estimates, prediction intervals, sensitivity analyses, and meta-regression directly from the published tables.

Table 3. Pre-Specified Moderators and Their Disposition

Moderator	Categories	Rationale	Disposition
Type of social media use	General/frequency vs. problematic/addictive	Cunningham et al. [16] showed differential effects by exposure type	Not tested; pool contains almost exclusively problematic-use instruments
Outcome measure	Depression vs. anxiety	Prior reviews report differing magnitudes across outcomes	Tested by within-study paired comparison (Section: Comparison Between Outcomes)
Geographic region	North America, Europe, East/SE Asia, South Asia	Cultural and structural context may moderate associations	Not tested; between one and three studies per category
Age subgroup	Early vs. late adolescence vs. emerging adulthood	Developmental stage affects engagement and symptom expression	Tested as a continuous moderator by meta-regression on mean age
Sex/gender composition	% female (continuous)	Gender differences in social comparison and approval-seeking	Not tested; percent female not recoverable for included studies
Study design	Cross-sectional vs. longitudinal	Design affects directionality and magnitude	Not tested; pool is entirely cross-sectional
Risk of bias score	NOS score (continuous)	Methodological quality may moderate reported effect sizes	To be tested once study-level NOS scores are finalised (Supplementary Table S2)

Ethical Considerations

This analysis synthesised data from previously published studies, all of which reported ethical approval in their source publications. No new data

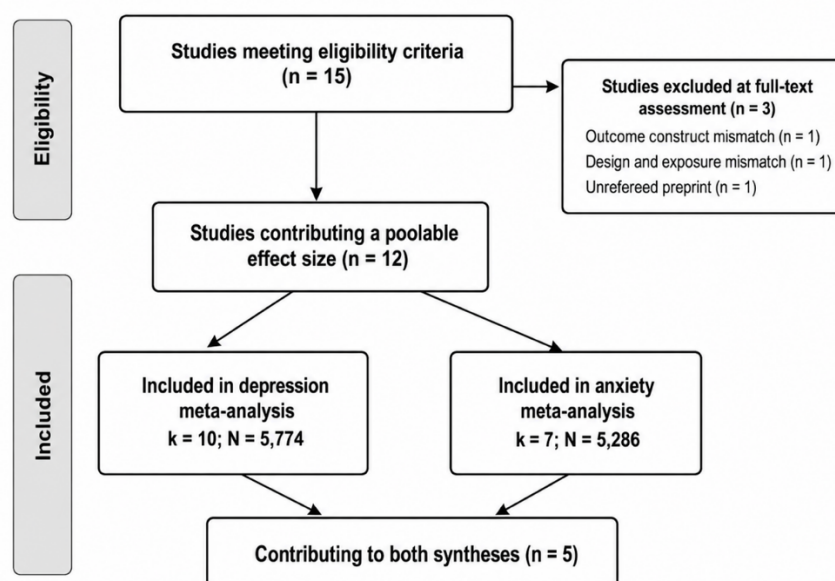
were collected, no human participants were directly involved, and no individual-level data were obtained, so ethics board review was not required for the present work.

Results

Literature Search and Study Selection

Records were identified across four databases and two supplementary sources, deduplicated, and screened at title and abstract before full-text assessment, with interrater agreement quantified at both stages using Cohen's kappa and reasons recorded for every full-text exclusion. [Per-database yields and stage-by-stage counts to be inserted from the search and

screening records, together with the corresponding PRISMA 2020 diagram.] Figure 1 reports the flow of studies from eligibility through to each quantitative synthesis: 15 studies met the eligibility criteria at screening, 3 were excluded at full-text assessment following verification against source records, and 12 contributed a poolable effect size, of which 5 contributed to both syntheses.

Figure 1. Flow of Studies Through the Synthesis**Figure 1. Flow of Studies Through the Synthesis**

Note. The figure reports the flow of studies from eligibility through to each quantitative synthesis. Counts correspond to the study-level records in Table 4. Five samples contribute an estimate to both syntheses, so the two meta-analytic totals are not additive.

Scope of Synthesis and Accounting for Eligible Studies

This paper reports a meta-analysis of a systematically searched literature rather than a systematic review with narrative synthesis, and the title and objectives are framed accordingly (deviation D7). Every study for which a study-level record is held is accounted for in Table 4, which states the synthesis each entered and, for those not pooled, the reason.

Table 4 records, for every study meeting the eligibility criteria, which analysis it entered and, where

a study was eligible but did not enter a pooled analysis, the specific reason: non-convertible effect measure, insufficient statistical information for variance estimation, overlapping participant sample, or availability of adjusted estimates only. This accounting addresses the previously unexplained gap between the number of studies meeting the eligibility criteria and the number contributing to each pooled estimate.

Table 4. Accounting for All Studies Meeting Eligibility Criteria

#	Study	Depression MA	Anxiety MA	Not pooled: reason
1	Holmgren & Coyne [39]	Yes	No	Anxiety outcome not reported
2	Wang et al. [40]	Yes	No	Anxiety outcome not reported
3	Hou et al. [38]	Yes	Yes	—
4	Kircaburun & Griffiths [33]	Yes	No	Anxiety outcome not reported
5	Kircaburun, Demetrovics et al. [41]	Yes	No	Anxiety outcome not reported
6	Kircaburun & Griffiths (2020)	Yes	No	Anxiety outcome not reported
7	Stockdale & Coyne [43]	Yes	Yes	—
8	Wong et al. [32]	Yes	Yes	—
9	Brailovskaia et al. [LT]	Yes	Yes	—
10	Brailovskaia et al. [DE]	Yes	Yes	—
11	Yıldız Durak [37]	No	Yes	Depression outcome not reported
12	Kılınçel & Muratdağı [44]	No	Yes	Depression outcome not reported
13	Apaolaza et al. [29]	No	No	Excluded: outcome construct mismatch (social anxiety and derived stress, not a validated anxiety subscale)
14	He et al. [30]	No	No	Excluded: design and exposure mismatch (randomised trial, no social media exposure measure)
15	Mitra & Rangaswamy [31]	No	No	Excluded: unrefereed preprint, ineligible by design criteria

Note. MA = meta-analysis. Rows 1 to 12 contribute a poolable effect size to at least one synthesis; five studies (rows 3, 7, 8, 9, 10) contribute to both. Rows 13 to 15 were identified at screening but excluded at full-text assessment following verification against source records, for the reasons stated. This table accounts for every study for which a study-level record is held.

Risk of Bias Across Studies

Methodological quality was assessed using the Newcastle-Ottawa Scale adapted for cross-sectional designs. Common sources of methodological weakness across the pool were reliance on convenience sampling, absence of non-response analyses, exclusive use of self-report for both exposure and outcome, and cross-

sectional designs that preclude causal inference. The distribution of scores and the study-level assessments by domain are reported in Supplementary Table S2, and a sensitivity analysis restricted to low-risk studies is reported below.

Characteristics of Included Studies

The studies contributing to the meta-analyses were conducted in the United States, China, Hong Kong, Turkey, Lithuania, and Germany. All used cross-sectional designs. Sample sizes ranged from 300 to 1,640, and mean participant ages ranged from 15.60 to 21.47 years, concentrated in the late-adolescent and emerging-adult band. The Bergen Social Media Addiction Scale was the most frequently used exposure instrument, followed by the Social Networking Addiction Scale, the Social Media Addiction Rating Scale, and adaptations of the Problematic Social Media Use scale. The CES-D and the DASS-21 depression subscale were the most common depression instruments, and the DASS-21 anxiety subscale predominated for anxiety outcomes. Study-level characteristics, effect sizes, confidence intervals, and analytic weights are presented in Tables 5a and 5b.

Social Media Use and Depressive Symptoms

Ten studies comprising 5,774 participants contributed to the primary meta-analysis for depressive symptoms. Under the random-effects model using the

DerSimonian-Laird estimator, the pooled Pearson correlation coefficient was $r = 0.263$ (95% CI: 0.200 to 0.324, $p < .001$), indicating a small-to-moderate positive association between social media use and depressive symptoms. Individual study effect sizes ranged from $r = 0.030$ [33] to $r = 0.396$ (Brailovskaia et al., German sample). Substantial heterogeneity was present ($Q(9) = 55.82$, $p < .001$; $I^2 = 83.9\%$; $\tau^2 = 0.010$), and the 95% prediction interval ran from 0.031 to 0.468. A study conducted in a new population could therefore plausibly observe an association close to null, and the pooled value should not be read as an expected result for any particular setting.

Egger's regression test did not indicate small-study effects (intercept = -2.63 , $t = -0.89$, $p = .40$), although with ten studies this test is underpowered. Trim-and-fill using the L0 estimator imputed no missing studies, so the pooled estimate is unadjusted. Study-level data and analytic weights are presented in Table 5a, placed before the forest plot so that the pooled estimate can be verified independently. The forest plot is presented as Figure 2.

Table 5a. Study-Level Data and Weights: Social Media Use and Depressive Symptoms

Study	Country	Design	n	Mean age	Exposure measure	Outcome	r	95% CI	Weight
Holmgren & Coyne [39]	USA	CS	442	18.86	BSMAS	CES-D	0.290	0.202, 0.373	9.9%
Wang et al. [40]	China	CS	365	16.29	SNAS	CDI	0.180	0.079, 0.278	9.5%
Hou et al. [38]	China	CS	641	19.90	BSMAS	DASS-21 (Dep)	0.220	0.145, 0.292	10.5%
Kircaburun & Griffiths [33]	Turkey	CS	470	16.29	BSMAS	CDI	0.030	-0.061, 0.120	10.0%
Kircaburun, Demetrovics et al. [41]	Turkey	CS	344	20.87	BSMAS	CES-D	0.220	0.117, 0.318	9.4%
Kircaburun & Griffiths (2020) [source unverified]	Turkey	CS	460	19.74	BSMAS	CES-D	0.340	0.257, 0.418	10.0%
Stockdale & Coyne [43]	USA	CS	385	18.01	BSMAS	CES-D	0.280	0.185, 0.370	9.6%
Wong et al. [32]	Hong Kong	CS	300	20.89	BSMAS	DASS-21 (Dep)	0.336	0.232, 0.433	9.0%
Brailovskaia et al. [LT, source unverified]	Lithuania	CS	1,640	19.09	BSMAS	DASS-21 (Dep)	0.305	0.260, 0.348	11.5%
Brailovskaia et al. [DE, ref. 42]	Germany	CS	727	21.47	BSMAS	DASS-21 (Dep)	0.396	0.333, 0.456	10.7%
Pooled (random effects)			5,774				0.263	0.200, 0.324	100%

Note. CS = cross-sectional; BSMAS = Bergen Social Media Addiction Scale; SNAS = Social Networking Addiction Scale; CES-D = Center for Epidemiological Studies Depression Scale; CDI = Children's Depression Inventory; DASS-21 = Depression Anxiety Stress Scales; Dep = depressive symptoms; LT = Lithuania; DE = Germany. All estimates crude. Heterogeneity: $Q(9) = 55.82$, $p < .001$; $I^2 = 83.9\%$; $\tau^2 = 0.010$. 95% prediction interval: 0.031 to 0.468.

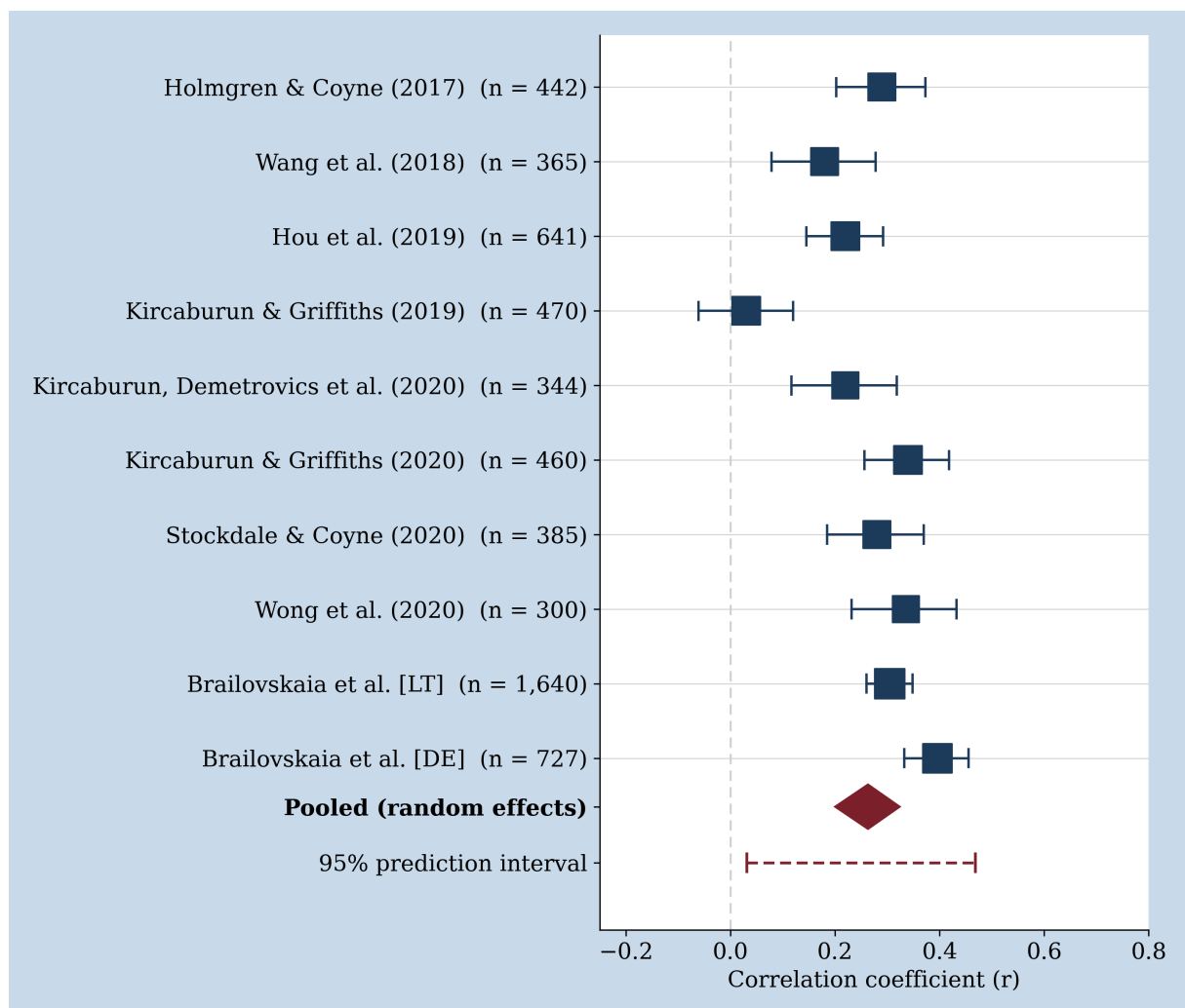


Figure 2. Forest Plot: Social Media Use and Depressive Symptoms (Random Effects, DerSimonian-Laird)

Note. Effect sizes expressed as Pearson r . Squares represent individual study estimates, with size proportional to analytic weight. The diamond represents the pooled random-effects estimate, its width indicating the 95% confidence interval. The dashed vertical line at $r = 0$ is the null reference line. The horizontal bar beneath the diamond represents the 95% prediction interval.

Social Media Use and Anxiety Symptoms

Seven studies comprising 5,286 participants contributed to the primary meta-analysis for anxiety symptoms following the removal of the two entries described above. The pooled Pearson correlation coefficient was $r = 0.363$ (95% CI: 0.265 to 0.453, $p < .001$), indicating a small-to-moderate positive association. Individual study effect sizes ranged from $r = 0.220$ [38] to $r = 0.580$ [37]. Considerable heterogeneity was present ($Q(6) = 90.21$, $p < .001$; $I^2 = 93.3\%$; $\tau^2 = 0.020$), and the 95% prediction interval ran from -0.009 to 0.646 , an interval that includes zero. At this level of heterogeneity the evidence is compatible with a null association in a new population, and the pooled estimate must

therefore be interpreted with corresponding caution. Egger's regression test was non-significant (intercept = 4.19, $t = 0.77$, $p = .48$) but is uninformative with seven studies. Trim-and-fill using the L0 estimator likewise imputed no missing studies.

Table 5b. Study-Level Data and Weights: Social Media Use and Anxiety Symptoms

Study	Country	Design	n	Mean age	Exposure measure	Outcome	r	95% CI	Weight
Hou et al. [38]	China	CS	641	19.90	BSMAS	DASS-21 (Anx)	0.220	0.145, 0.292	14.4%
Stockdale & Coyne [43]	USA	CS	385	18.01	BSMAS	DASS (Anx)	0.240	0.143, 0.332	13.7%
Wong et al. [32]	Hong Kong	CS	300	20.89	BSMAS	DASS-21 (Anx)	0.384	0.283, 0.477	13.3%
Yildiz Durak [37]	Turkey	CS	451	15.60	PSMU	DASS-21 (Anx)	0.580	0.515, 0.638	14.0%
Brailovskaia et al. [LT, source unverified]	Lithuania	CS	1,640	19.09	BSMAS	DASS-21 (Anx)	0.246	0.200, 0.291	15.1%
Brailovskaia et al. [DE, ref. 42]	Germany	CS	727	21.47	BSMAS	DASS-21 (Anx)	0.411	0.349, 0.470	14.5%
Kılınçel & Muratdağı [44]	Turkey	CS	1,142	15.60	SMARS	DASS-21 (Anx)	0.417	0.368, 0.464	14.9%
Pooled (random effects)			5,286				0.363	0.265, 0.453	100%

Note. CS = cross-sectional; BSMAS = Bergen Social Media Addiction Scale; PSMU = Problematic Social Media Use scale; SMARS = Social Media Addiction Rating Scale; DASS-21 = Depression Anxiety Stress Scales; Anx = anxiety symptoms. All estimates crude. Heterogeneity: $Q(6) = 90.21$, $p < .001$; $I^2 = 93.3\%$; $\tau^2 = 0.020$. 95% prediction interval: -0.009 to 0.646 .

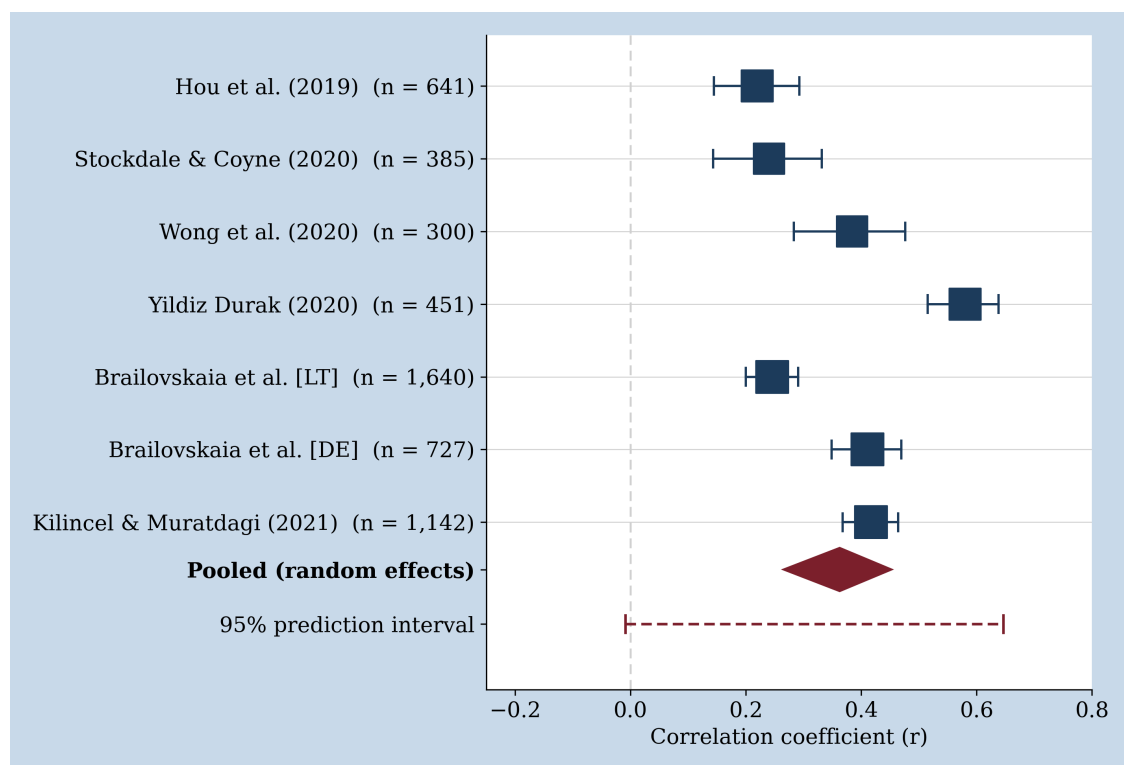


Figure 3. Forest Plot: Social Media Use and Anxiety Symptoms (Random Effects, DerSimonian-Laird)

Note. Effect sizes expressed as Pearson r . Squares represent individual study estimates, with size proportional to analytic weight. The diamond represents the pooled random-effects estimate, its width indicating the 95% confidence interval. The dashed vertical line at $r = 0$ is the null reference line. The horizontal bar beneath the diamond represents the 95% prediction interval, which crosses the null.

Comparison Between Outcomes

Five samples contributed an estimate to both the depression and the anxiety analysis, so the two pools are not independent. Across these five samples, the within-study differences in Fisher's z between the anxiety and depression associations were 0.000 [38], -0.043 [43], +0.055 [32], -0.064 (Brailovskaia, Lithuanian sample), and +0.018 (Brailovskaia, German sample). The mean difference was -0.007 ($t(4) = -0.32$, $p = .77$; Wilcoxon signed-rank $p = .88$), so the two associations are not statistically distinguishable. The unpaired comparison, reported for reference only and not valid given the shared samples, yields $z = 1.71$ ($p = .088$), which is also non-significant. Claims that social media use is more strongly associated with anxiety than with depression are therefore not supported by these data and have been removed from the manuscript.

Sensitivity Analyses

Leave-one-out analysis confirmed the stability of both pooled estimates. For depression, the pooled correlation ranged from $r = 0.247$ (omitting the German Brailovskaia sample) to $r = 0.289$ (omitting Kircaburun & Griffiths [33]). For anxiety, it ranged from $r = 0.322$ (omitting Yildiz Durak [37]) to $r = 0.385$ (omitting Hou et al. [38]). The direction, magnitude, and statistical significance of both pooled effects remained stable across all iterations.

Excluding Wong et al. [32] from the anxiety model gave $r = 0.359$ (95% CI: 0.249 to 0.461; $k = 6$; $N =$

4,986; $I^2 = 94.4\%$), with a prediction interval of -0.067 to 0.675, so the pooled estimate is insensitive to this study even though the prediction interval widens further. Restricting the analysis to samples with a mean age below 19 years yielded $r = 0.196$ (95% CI: 0.072 to 0.315; $k = 4$; $N = 1,662$; $I^2 = 85.6\%$) for depression and $r = 0.423$ (95% CI: 0.243 to 0.574; $k = 3$; $N = 1,978$; $I^2 = 94.5\%$) for anxiety. The depression association attenuates by roughly a quarter when the pool is restricted to adolescent-aged samples, while the age-restricted anxiety estimate rests on only three studies and its prediction interval is uninformative at that number. A further sensitivity analysis restricted to studies at low risk of bias, together with a meta-regression on NOS score, is reported alongside the study-level quality assessments in Supplementary Table S2.

Meta-Regression

Mean sample age predicted the magnitude of the depression association ($b = 0.045$ per year of mean age, $SE = 0.020$, $z = 2.27$, $p = .023$). Residual heterogeneity after adjustment for mean age was negligible ($Q(8) = 4.24$, $p = .84$), compared with $Q(9) = 55.82$ ($p < .001$) before adjustment. Mean sample age therefore accounts for almost all between-study variation in the depression model: studies conducted in older samples report systematically stronger associations. No comparable age gradient was detected for anxiety.

Table 6. Summary of Meta-Analytic Findings

Outcome	k	N	Pooled r	95% CI	95% PI	I^2	Q (p)	τ^2	Egger p
Depressive symptoms	10	5,774	0.263	0.200, 0.324	0.031, 0.468	83.9%	55.82 (< .001)	0.010	.40
Anxiety symptoms	7	5,286	0.363	0.265, 0.453	-0.009, 0.646	93.3%	90.21 (< .001)	0.020	.48
Depression, mean age < 19	4	1,662	0.196	0.072, 0.315	-0.370, 0.656	85.6%	20.76 (< .001)	0.014	.55
Anxiety, mean age < 19	3	1,978	0.423	0.243, 0.574	—	94.5%	36.30 (< .001)	0.030	.98

Note. All analyses used random-effects models with the DerSimonian-Laird estimator and Fisher's z transformation. Effect sizes are expressed as Pearson r . k = number of studies; PI = prediction interval; I^2 = percentage of variance attributable to heterogeneity; τ^2 = between-study variance. Prediction interval not reported for $k = 3$. Egger's test is underpowered at all these study counts and its non-significance should not be read as evidence that bias is absent.

Table 7. Deviations From the Registered Protocol

ID	Domain	Registered	Implemented	Justification
D1	Population and age limits	Adolescents aged 10–19 years	Adolescents and young adults; no upper age limit	The great majority of eligible studies sampled populations extending into emerging adulthood. Age composition is reported per study and an age-

ID	Domain	Registered	Implemented	Justification
				restricted sensitivity analysis is provided
D2	Secondary outcome	Psychological distress (GHQ-12, K6, K10)	Removed	No pooled analysis was feasible; retaining the outcome in the criteria without reporting it would be inconsistent
D3	Study designs and RoB tool	RCTs eligible; RoB 2 to be applied	RCTs remained eligible; RoB 2 not applied	No randomised trial contributing a usable effect size was identified
D4	Outcome definition	Anxiety symptoms	Stress subscales explicitly excluded from anxiety outcomes	Stress and anxiety are distinct constructs and are separately scored on the instruments used
D5	Subgroup analyses	Region, exposure type, age subgroup, sex, design, quality	Not conducted; meta-regression on mean age substituted	Pool contains one to three studies per category, insufficient for subgroup inference
D6	Additional analyses	Not specified	Prediction intervals, trim-and-fill, age-restricted sensitivity analysis, paired between-outcome comparison added	Required to characterise heterogeneity and to compare outcomes given overlapping samples
D7	Scope of synthesis	Systematic review with meta-analysis	Meta-analysis of a systematically searched literature	No narrative synthesis of non-pooled studies was conducted; the title and objectives now match what was done

Discussion

Principal Findings

This meta-analysis found positive associations between social media use and both depressive and anxiety symptoms among adolescents and young adults. Contrary to informal comparisons in some prior syntheses, the two associations were not statistically distinguishable once the dependence between the two pools was handled appropriately. Heterogeneity was high for both outcomes, and in the case of depression was almost entirely attributable to differences in the mean age of the samples analysed.

The Age Gradient

The age gradient in the depression model is the most consequential finding reported here. Studies conducted in older samples reported systematically stronger associations, and adjusting for mean age reduced residual heterogeneity from a highly significant value to one indistinguishable from zero, which means that a single sample characteristic accounts for variation that previous syntheses have described as unexplained. Two readings of this pattern are possible, and the present data cannot separate them.

The first reading is developmental. The transition to adulthood brings intensified social comparison around emerging identity, appearance, romantic status, and career trajectory, at a point when the structural buffers of school routine and parental

mediation have weakened. On this account the association is genuinely stronger in emerging adulthood, and interventions concentrated on early adolescence may be directed at the wrong developmental window. The second reading is methodological. Older samples in this literature are drawn overwhelmingly from university populations recruited by convenience, and these differ from school-based samples in self-selection, response style, and baseline symptom levels. On this account the pooled literature systematically overstates the association for the adolescent population that public and policy debate is actually concerned with, and the age-restricted estimate of $r = 0.196$ for depression is consistent with that possibility. Distinguishing the two interpretations would require studies sampling across the full age range within a single design and measurement framework, which the present evidence base does not provide.

Mechanisms

Several mechanisms plausibly underlie the observed associations, and the design of this analysis does not adjudicate between them. Social comparison and evaluative feedback offer the most direct account, since platform architecture quantifies peer approval through visible counts and makes upward comparison continuously available at a developmental stage when

fear of negative evaluation and approval-seeking are both heightened [7,8]. Closely related is the possibility that cyberbullying and online exclusion, which are more frequently encountered by heavy users and are independently associated with internalizing symptoms, drive part of the association through the social rather than the technological features of these platforms.

Other pathways require no direct psychological effect of content at all. Night-time use displaces and fragments sleep, and disturbed sleep is an established antecedent of both depressive and anxious symptomatology, which makes sleep a plausible mediator of much of the observed relationship. A related displacement account holds that time online substitutes for in-person contact that would otherwise support emotional regulation, although the evidence for displacement at realistic usage levels remains contested. A third possibility, particularly relevant to the problematic-use instruments that dominate this pool, is that compulsive engagement functions as avoidance-based emotion regulation, providing short-term relief that reinforces further use. This last account is consistent with the stronger associations reported for problematic-use measures than for time-based ones [16], and with the composition of the present evidence base. Reverse causation remains a live possibility across all of these. Heffer et al. [34] found that depressive symptoms predicted subsequent social media use among adolescent girls with no evidence in the reverse direction, while other prospective work supports bidirectional pathways [35,6]. The correlations reported here are equally consistent with social media use contributing to symptoms, with symptoms driving use, and with both operating together, and nothing in a pool of cross-sectional studies can distinguish between them.

Magnitude and Interpretation

The size of these associations bears directly on what follows from them. Using representative data and specification curve analysis, Orben and Przybylski [36] found that digital technology use explained a negligible share of variance in adolescent wellbeing. The estimates reported here are larger, plausibly because the exposure pool is dominated by problematic-use instruments rather than time-based measures, and Cunningham et al. [16] showed that this distinction produces substantially different effect sizes. Valkenburg et al. [4], in an umbrella review, caution that no single summary statistic resolves whether an observed association is of sufficient magnitude to warrant intervention, and that averages may understate effects among vulnerable subgroups. The prediction intervals reported here support that caution directly and quantitatively, since the pooled values describe a central tendency across

heterogeneous populations rather than an expected value for any particular one, and in the case of anxiety the interval includes zero.

Implications

A consistent theme runs through the practical implications of these findings, which is that the pattern of use matters more than its duration. In clinical practice, enquiry about compulsive engagement, withdrawal, and functional interference is warranted in the mental health assessment of young people, whereas questions about time alone are unlikely to identify risk and validated problematic-use screening is more informative. The same logic applies in schools, where universal digital literacy provision addressing social comparison and evaluative feedback is proportionate to an association of this magnitude while restrictive approaches targeting duration are poorly matched to the evidence, and in families, where guidance should prioritise night-time device practices and open discussion of online experience over blanket time limits, given the plausibility of the sleep pathway and the weakness of the evidence for duration effects specifically. For intervention design and policy, the width of the prediction intervals is the governing consideration. It argues against universal digital health programmes and in favour of targeting young people who show compulsive use patterns, and it argues equally against regulatory measures framed around total screen time in preference to attention on engagement-maximising design features. Claims of a uniform causal effect are not supported by this evidence base, and policy constructed on that premise risks overreach.

Strengths and Limitations

The strengths of this analysis include adherence to PRISMA 2020, prospective registration, duplicate screening and extraction, a transparent a priori rule for effect-size selection, separate pooling of depression and anxiety with a formal comparison appropriate to their dependence, prediction intervals reported alongside every pooled estimate, and interrogation of heterogeneity through meta-regression rather than narrative speculation. Every contributing estimate was verified against its source record, and entries that did not withstand that check were excluded. The limitations are substantial. The analytic pool is entirely cross-sectional, which precludes causal inference and prevents examination of temporal ordering. Both exposure and outcome are self-reported in every included study, introducing shared method variance likely to inflate the observed associations relative to objective measurement. The exposure pool is dominated by problematic-use instruments, so the estimates speak more directly to compulsive use than to

social media use in general. The evidence base is concentrated in high-income and upper-middle-income settings, with no representation from Sub-Saharan Africa, Latin America, or South and Southeast Asia in the final pool, which sharply limits the geographic claims that can be made. Several pre-specified

moderators could not be examined because the pool is too small or the necessary study-level data are not recoverable. Finally, the pooled estimates rest on a small number of studies, and the age composition of the pool departs from the registered protocol in the manner recorded in Table 7.

Conclusion

Social media use is positively associated with depressive and anxiety symptoms among adolescents and young adults, at a small-to-moderate magnitude, with no detectable difference between the two outcomes. The prediction interval for anxiety includes zero, and a pronounced age gradient indicates that the depression association is considerably weaker in adolescent-aged samples than the pooled estimate suggests. The cross-sectional character of the evidence

base cannot establish the direction of effect, and reverse causation remains a live alternative. Priorities for future research are longitudinal and experimental designs, device-derived measurement of exposure, consistent separation of anxiety from stress outcomes at the point of extraction, and recruitment of young people in low- and middle-income settings where social media adoption is rapid and the mental health consequences remain largely uncharacterised.

Supplementary Materials

Supplementary file available via: <https://www.jebm.org/supfile/772/jebm013-Supplementary.pdf>

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