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Does the Measure Make the Effect? A Specification-Curve Analysis of Social-Media Use and Mental Health in Indian Adolescents

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Abstract

Concern that social media harms adolescent mental health has produced an unusually contradictory evidence base, in which conclusions can depend as much on analytic choices as on the phenomenon itself. Large specification-curve analyses from high-income countries suggest the association is real but very small; none has been conducted in a lower- and middle-income country (LMIC), and most treat social-media use as a single construct. The present study reports the first specification-curve analysis of social-media use and mental health in a cross-sectional Indian adolescent sample (N = 393, ages 14–17). Eight operationalisations of use were crossed with eight mental-health and wellbeing outcomes and three covariate sets, yielding 192 standardised regression specifications with heteroscedasticity-consistent confidence intervals; joint inference used 5,000 permutations, and the Goldilocks (curvilinear) hypothesis was tested across 64 quadratic models. The median absolute association was small ($|\beta|$ = .095; ~0.9% of variance) and the median sign-aligned effect was statistically indistinguishable from zero (β = .035, p = .141), yet the number of significant harm-aligned associations far exceeded chance (p < .001). Crucially, detectability depended on measurement: attitudinal, intensity, and time-based measures were significant in most of their specifications (e.g., 21/24 and 18/24), whereas follower, friend, and account counts were almost inert (0–4/24). No curvilinear 'Goldilocks' pattern survived correction. Conclusions were invariant to covariate adjustment and replicated in an independent analytic pipeline. Whether social media appears 'associated with' mental health depended materially on which facet of use was measured.

Introduction

Few questions in developmental science are debated as fiercely, or with as little resolution, as whether social media harms adolescent mental health. Public commentary has coalesced around a strong harm narrative, popularised by accounts of an anxiety-ridden, smartphone-bound generation,[1,2] and has begun to shape policy. Yet the empirical literature it invokes is markedly more equivocal than the public conversation implies. The stakes are concretely Indian: mental disorders already account for a substantial share of the disease burden among Indian youth,[3] and with more than 450 million social-media users and one of the world's largest adolescent populations, regulatory choices there, including actively debated minimum-age proposals,[4] will affect tens of millions. Early Indian evidence links heavy use to adolescent distress[5,6] but rests largely on small qualitative or single-instrument studies.

The weight of the quantitative evidence points to associations that are small, inconsistent, and heterogeneous. Umbrella and annual reviews converge: most call the social-media–mental-health association 'weak' or 'inconsistent', a minority 'substantial',[7] and large preregistered cohorts find no population-level change in disorder prevalence despite rising adolescent media use.[8] The most comprehensive meta-review reports typical correlations of only r = .05 to .15, with social support the one beneficial exception;[10] systematic reviews concur that links to depression, anxiety, and distress are weak and inconsistent.[9] Longitudinal and cross-national work agrees, from an eight-year within-person null[11]

to 29- and 72-country analyses finding only small or no associations.[12,13] The most influential single demonstration is methodological: using specification-curve analysis (SCA) across three datasets (combined *n* = 355,358), Orben and Przybylski[14] showed that digital-technology use explained at most about 0.4% of variance in adolescent wellbeing and, more tellingly, that its sign, size, and significance shifted with defensible changes in how variables were defined and modelled. Later work locates age- and sex-specific windows of sensitivity within these small averages[15] without overturning that headline.

Two features of this literature motivate the present study. First, almost all field-defining evidence, including every large SCA, comes from high-income, predominantly Anglophone settings; whether the measurement-contingent, small-effect picture generalises to an LMIC adolescent population, with different platform ecologies, family structures, and norms, is unknown. Second, 'use' is typically collapsed into a single index, most often self-reported time, when it is in fact multidimensional. Reviewers increasingly argue that the quality and type of engagement, not sheer quantity, drive whatever associations exist;[7] a 141-study meta-analysis finds mostly negligible effects except for use-specific links to online support,[16] and theory holds that social media transforms, rather than adds to, adolescent experience,[17] with effects that may benefit some and harm others.[18] If different operationalisations index genuinely different constructs, the answer to 'is social media associated with mental health?' may be an artefact of measurement choice: precisely the analytic-flexibility problem that specification-curve and multiverse methods were designed to expose.[19,20]

The present study therefore brings the specification-curve approach to an Indian adolescent sample to ask not merely *whether* social-media use is associated with mental health, but *how much that conclusion depends on measurement and modelling choices*. We address two questions. First (RQ1), how large and how robust is the use–mental-health association across a bounded multiverse of operationalisations, outcomes, and covariate sets? Second (RQ2), is the association linear or curvilinear: does the 'Goldilocks' hypothesis of an optimal, intermediate level of use[21] hold here? Because the sample (*N* = 393) is small relative to the population-scale exemplars, we frame the contribution as a proof-of-principle demonstration of measurement sensitivity and effect-size realism in an under-represented setting, with honest power accounting, not as a definitive estimate of effect magnitude.

Method

Design and Transparency

This was a cross-sectional, observational study; all estimates are associational and no causal language is used. It follows STROBE guidance for cross-sectional studies and specification-curve reporting conventions.[19] The analysis is a secondary, exploratory (not preregistered) re-analysis of a doctoral dataset, with code and a full numerical audit trail available and a fixed random seed (2025) for reproducibility.

Setting and Participants

Participants were 393 adolescents aged 14–17 years (202 boys, 191 girls) in secondary schools across the Chandigarh tricity (Chandigarh, Mohali, and Panchkula), northern India, surveyed between March and October 2024. Eligible adolescents were aged 14–17, used social media at least three hours per week, and could respond in English or Hindi; those with special needs or significant health conditions were excluded. A stratified design sampled proportionally by city, school, and grade (grades 9–12), with 75 adolescents per grade approached in Chandigarh and 50 per grade in Panchkula and Mohali. Of about 700 approached, 100 declined consent; of the 600 who consented, 110 were ineligible; and of the 490 eligible, 97 failed embedded validity (infrequency) items, yielding a final sample of $N = 393$. Complete responses were required, so there were no missing data (0 cells across the 16 focal variables). Sample size was set a priori with G*Power,[22] indicating about 200 participants for small-to-medium effects ($f^2 = .05$; $r \approx .22$; $d = .35$) at $\alpha = .05$, power = .80; the achieved $N = 393$ exceeded this but, as noted under Effect-Size Realism,[23] remains underpowered for the very small effects a specification curve probes. The study was approved by the relevant institutional ethics committee, with informed consent and adolescent assent (details on the title page).

Measures

Measures were completed as a self-administered structured questionnaire, in English or Hindi, under researcher supervision. *Social-media use.* Eight operationalisations spanned volume, intensity, network size, and attitude. The Social Media Intensity Scale, an eight-item adaptation of the Facebook Intensity Measure,[24] yielded a continuous composite (SMIS intensity) and an attitudinal subscale. Six single items captured daily screen time, daily social-media time, visit frequency (eight ordered categories), and the numbers of accounts, followers, and friends; count and time items were open responses recoded into ordinal bands (codebook definitions). *Mental health and wellbeing.* The Depression, Anxiety and Stress Scales–21 (DASS-21)[25,26] gave depression, anxiety, and stress subscales and a total distress score; wellbeing used the Personal Wellbeing Index–School Children (PWI-SC),[27] self-esteem the Rosenberg Self-Esteem Scale (RSES),[28] social support the Multidimensional Scale of Perceived Social Support (MSPSS),[29] and sleep problems the BEARS screen.[30] *Covariates.* Age, sex, and a composite socioeconomic-status index (Kuppuswamy scale) defined nested adjustment sets.

In-sample internal consistency (Table 2) was excellent for the DASS-21 ($\alpha = .92$, $\omega = .92$), MSPSS ($\alpha = .90$, $\omega = .92$), and PWI-SC ($\alpha = .83$, $\omega = .87$), and acceptable for SMIS intensity ($\alpha = .79$, $\omega = .85$). The RSES total showed only modest reliability ($\alpha = .67$, $\omega = .73$), so self-esteem results are interpreted with caution. The RSES was administered on a reverse-coded Likert scale, verified empirically: the raw item ‘On the whole, I am satisfied with myself’ correlated positively with distress ($r = .37$), whereas valid self-esteem must correlate negatively; the total was therefore oriented so higher scores denote higher self-esteem, correlating as expected with distress ($r = -.49$) and wellbeing ($r = .30$).

Data Analysis Plan

Specification curve (RQ1). A curve was estimated across all combinations of eight use operationalisations, eight outcomes, and three nested covariate sets (unadjusted; age and sex; adding socioeconomic status), giving 192 specifications. Each regressed the standardised outcome on the standardised use measure with heteroscedasticity-consistent (HC3) robust standard errors, sign-aligned so positive values denote use predicting worse mental health (reversed for wellbeing, self-esteem, and support). *Joint inference.* Following Simonsohn et al.,[19] use measures were permuted relative to outcomes and covariates (5,000 permutations) and the curve recomputed under each, comparing the observed median sign-aligned effect, median absolute effect, and share of harm-aligned significant specifications with their nulls. *Functional form (RQ2).* A quadratic term was added to each of the 64 age- and sex-adjusted use × outcome models, corrected by the Benjamini–Hochberg false-discovery rate (FDR).[31] *Power.* Effect-size realism was quantified by the minimum detectable correlation and power at $N = 393$. Analyses used R 4.3.1,[32] with specr[33] cross-checking the curve and every estimate reproduced by Frisch–Waugh–Lovell residualisation (maximum discrepancy $< 1 \times 10^{-8}$). *Robustness.* To test dependence on the pipeline itself, the analysis was independently re-implemented in Python with a different metric (partial correlation), a fourth covariate set (physical activity), and the eighth measure as active versus passive use, giving 256 specifications with permutation and bootstrap inference.

Results

Preliminary Analyses

Descriptive statistics and in-sample reliabilities appear in Tables 1 and 2. The network-count measures (accounts, followers, friends) were positively skewed and floor-heavy, most adolescents selecting the lowest band.

Table 1

Descriptive Statistics for the Eight Use Operationalisations and Eight Outcomes (N = 393)

Variable	*M*	*SD*	Min	Max
Social-media use				
SMIS intensity (composite)	7.30	1.28	4	12
Screen-time/day	3.90	1.77	1	7
Social-media time/day	3.71	1.92	1	7
Visit frequency	5.07	2.03	1	8
No. of accounts	1.38	0.68	1	4
No. of followers	1.93	1.12	1	6
No. of friends	1.56	1.11	1	6
SMIS attitudinal subscale	2.85	0.90	1	5
Mental health and wellbeing				
Depression (DASS)	13.96	10.21	0	42
Anxiety (DASS)	12.37	9.58	0	42
Stress (DASS)	14.33	9.61	0	40
Distress (DASS total)	40.66	26.93	0	120
Wellbeing (PWI)	71.58	17.94	10	100
Self-esteem (RSES)	26.66	4.72	11	39
Social support (MSPSS)	57.15	16.43	12	84
Sleep problems (BEARS)	2.79	1.56	0	6

Note. Screen-time/day and social-media time/day are reported in hours per day; the remaining single-item use measures (visit frequency and the account, follower, and friend counts) are ordinal bands (higher = more use), and SMIS intensity and the SMIS attitudinal subscale are continuous composites. RSES is oriented so that higher = higher self-esteem (the dataset uses a reverse-coded Likert). DASS = Depression Anxiety Stress Scales; PWI = Personal Wellbeing Index–School Children; RSES = Rosenberg Self-Esteem Scale; MSPSS = Multidimensional Scale of Perceived Social Support.

Table 2

In-Sample Internal Consistency of the Multi-Item Scales (N = 393)

Scale	*k* items	Cronbach's *α*	McDonald's *ω*
Social Media Intensity (SMIS)	8	.79	.85
Distress (DASS-21)	21	.92	.92
Wellbeing (PWI-SC)	8	.83	.87
Self-esteem (RSES, corrected)	10	.67	.73
Social support (MSPSS)	12	.90	.92

Note. For SMIS the standardised *α* is reported because its items use heterogeneous response scales. RSES *α* is for the corrected (construct-oriented) total.

Magnitude and Measurement Dependence of the Association (RQ1)

The full curve is displayed in Fig. 1. Across the 192 specifications the median standardised coefficient was small (median sign-aligned β = .035; IQR − .050 to .145) and the median absolute effect was $|\beta|$ = .095, roughly 0.9% of variance. Effects ranged from − .173 to .240; even the largest accounted for only 5.8% of variance. The strongest specification linked the SMIS attitudinal subscale to depression, β = .24, 95% CI [.13, .35], $p < .001$. Of the 192 specifications, 86 (44.8%) were significant at $p < .05$ (HC3 robust standard errors); 117 pointed in the harm direction and 75 in the protective direction.

Whether an association was detectable depended on how use was measured (Fig. 2). Attitudinal-, intensity-, and time-based operationalisations were significant in most of their specifications: the SMIS attitudinal subscale (21/24), screen time per day (18/24), SMIS intensity (18/24), and visit frequency (16/24). Count-based operationalisations were almost inert: friends (4/24), accounts (2/24), and followers (0/24). Significance also varied by outcome, most frequent for depression (16/24), anxiety (15/24), and perceived support (15/24), and least frequent for wellbeing (2/24) and sleep problems (3/24). Of the 86 significant specifications, 69 were harm-aligned and 17 protective, the latter almost entirely the positive use–social-support association.

Figure 1

Specification Curve of the Social-Media-Use–Mental-Health Association Across 192 Specifications

Note

Panel A shows the 192 standardised coefficients (sign-aligned so positive = use predicts worse mental health), sorted by magnitude, with 95% confidence intervals; dark green = $p < .05$ (HC3), grey = non-

significant. Panel B shows which use measure, outcome, and covariate set defined each specification, at the same horizontal rank.

Figure 2

Proportion of Significant Specifications by Use Operationalisation

Note

Each bar is the percentage of that measure's 24 specifications (8 outcomes × 3 covariate sets) significant at $p < .05$ (HC3); counts are annotated. The association is recoverable from attitudinal, intensity, and time-based measures but largely absent from count-based measures.

This concentration mirrors the fact that the eight operationalisations were far from interchangeable. They were only modestly inter-correlated (median $r = .23$; range .10 to .88; Fig. 3), the count-based indices in particular sharing little variance with the time- and attitude-based indices. Social-media use, in this sample, is thus not a unitary construct: whether use is 'associated with' mental health depends on which facet is measured. Conclusions were, by contrast, essentially invariant to adjustment: the number of significant specifications was virtually identical across unadjusted (28/64), demographically adjusted (29/64), and fully adjusted (29/64) sets, indicating that measurement, not confounding, drove the variability.

Figure 3

Inter-Correlations Among the Eight Use Operationalisations

Note

Pearson correlations ($N = 393$). The high SMIS-intensity–attitudinal value reflects shared items by construction. Otherwise modest inter-correlations indicate the operationalisations capture distinct facets of use.

Joint Inference Across the Curve

Joint inference (Table 3) confirmed the curve was not an artefact of analytic flexibility while showing the typical effect was small. The median absolute effect (.095 vs. a permutation-null median of .034), the share of significant specifications (44.8% vs. ~4%), and the share of significant harm-aligned specifications (35.9% vs. ~2%) each far exceeded the null (all $p < .001$). The median sign-aligned effect, however, was small and not distinguishable from zero ($\beta = .035$, $p = .141$), because a protective minority, chiefly the use–social-support association, partly offset the harm-aligned majority. The curve thus

reflects a reliable but small body of associations leaning towards harm (69 of 86 significant effects) without a large or uniform effect.

Table 3

Joint (Permutation) Inference Across the 192 Specifications

Test statistic	Observed	Null median	*p*
Median sign-aligned effect (β)	.035	−.000	.141
Median absolute effect (|β|)	.095	.034	< .001
Share of specifications p < .05	44.8%	~ 4%	< .001
Share significant & harm-aligned	35.9%	~ 2%	< .001

Note. Permutation null = 5,000 reshuffles of the use measures relative to outcomes and covariates;[19] *p* = proportion of permutations with a null statistic at least as extreme as observed. Significance is evaluated with HC3 robust standard errors throughout; the permutation null for the share statistics is essentially invariant to the standard-error choice.

Linear Versus Curvilinear Form: A Test of the Goldilocks Hypothesis (RQ2)

The Goldilocks hypothesis predicts a concave (inverted-U) relationship, with the poorest outcomes at both very low and very high use.[21] The canonical test (wellbeing on screen time) showed no curvilinearity (*b* = − .04, *p* = .534), and none of the eight wellbeing models showed a significant quadratic term (smallest *p* = .070). Of the 64 quadratic tests, 17 were significant uncorrected and only 5 survived FDR correction (Table 4); all five involved the SMIS attitudinal subscale predicting distress or sleep outcomes, and all were convex (accelerating at higher attachment) rather than the concave form Goldilocks predicts. The Goldilocks pattern was therefore not supported.

Table 4

Curvilinear (Quadratic) Tests Surviving FDR Correction, and the Canonical Goldilocks Test

Use measure	Outcome	*b* (quad.)	*p*	*p* (FDR)
SMIS attitudinal subscale	Anxiety (DASS)	.14	< .001	.029
SMIS attitudinal subscale	Distress (DASS total)	.14	< .001	.029
SMIS attitudinal subscale	Stress (DASS)	.13	.003	.042
SMIS attitudinal subscale	Depression (DASS)	.13	.003	.042
SMIS attitudinal subscale	Sleep problems (BEARS)	.14	.003	.044
Screen-time/day	Wellbeing (PWI) [canonical]	−.04	.534	.665

Note. Quadratic coefficient from the standardised model use + use² + age + sex (HC3 SE). Positive *b* = convex (accelerating), opposite to the concave Goldilocks inverted-U. 17/64 tests were significant uncorrected; 5/64 survived FDR correction.

Effect-Size Realism and Statistical Power

The magnitudes here are consistent with, though somewhat larger than, the benchmark SCA estimate of Orben and Przybylski,[14] who found digital-technology use explained at most about 0.4% of variance in adolescent wellbeing. At $N = 393$ the design had 80% power to detect $r = .14$ (about 2% of variance) but only 24% power for an Orben-sized $r \approx .06$; detecting the latter at 80% power would require roughly $N = 1{,}975$. The analysis is therefore a proof-of-principle demonstration of measurement sensitivity, not a definitive estimate of magnitude.

Robustness Across an Independent Analytic Pipeline

The conclusions did not depend on the analytic pipeline. An independent Python re-implementation (partial correlations, four covariate sets adding physical activity, an alternative eighth measure, a different inference engine; 256 specifications) reproduced every result: the typical effect was again small (median $|r| = .078$), the median sign-aligned effect indistinguishable from zero ($p = .476$), yet harm-aligned significant specifications far exceeded chance (76 vs. ~4 expected; $p = .002$; Figure S1). Significance again concentrated in intensity-, screen-time-, and frequency-based measures and was near-absent for network counts, and no quadratic term survived FDR correction in the 56 tests (Figure S2). That two different pipelines converge is the strongest evidence the conclusions are not artefacts of analytic choice.

Discussion

This study asked how large and how robust the social-media–mental-health association is in an Indian adolescent sample, and whether its form is linear or curvilinear. The association is small, predominantly linear, reliably non-zero in aggregate yet indistinguishable from zero in typical magnitude, and, most importantly, contingent on how use is operationalised. To our knowledge this is the first specification-curve analysis of the question in a lower- and middle-income country, showing that the measurement-dependence documented in large Western datasets is not peculiar to them but reproduces in an LMIC adolescent population.

The headline finding is that the measure makes the effect. Significant associations were recoverable almost entirely from attitudinal, intensity, and time-based operationalisations and essentially absent from network counts, though all eight were defensible indices of ‘use’. This is not a psychometric nuisance to average away; it follows from the operationalisations being only modestly inter-correlated and therefore indexing different constructs. The result gives empirical, LMIC-based support to the argument that quality and type of engagement, not sheer quantity, carry whatever signal exists, and that ‘overall use’ measures aggregate heterogeneous exposures.[7] It concretises the analytic-flexibility concern behind specification-curve and multiverse methods:[19,20] from identical data, a researcher choosing screen time or an attachment subscale would report harm, one choosing follower counts nothing.

At the same time, our joint inference guards against the opposite over-reading. Although harm-aligned associations far exceeded chance, so the curve is not merely noise, the median effect was trivial and statistically indistinguishable from zero. This aligns with Orben and Przybylski[14] and later large-scale nulls:[11,13] the association is real but tiny, fractions of a percent of variance. It tempers the strong-harm narrative[1,2] without endorsing a dismissive 'no effect' reading; the harm-aligned signal is reliable, concentrated in particular measures and outcomes (depression, anxiety), and consistent with developmental sensitivity documented elsewhere.[15] The honest summary is that effects are small, real, and heterogeneous, compatible with meaningful harm for some adolescents even where the average is negligible.[8]

The functional-form analysis adds a boundary condition. The Goldilocks hypothesis[21] was not supported: the canonical wellbeing–screen-time quadratic was null, and the only curvilinear terms to survive correction were convex (accelerating distress at higher emotional attachment), the opposite of the predicted shape. In this sample the relationship is adequately described as weak and linear; any non-linearity is an acceleration of risk at high attachment, not a penalty for abstinence. Replicating Przybylski and Weinstein's curvilinear test as a null in an LMIC context is itself informative, suggesting their inflection-point result may not generalise across settings or operationalisations.

One protective result deserves comment. More intensive use was associated with more perceived social support, the principal source of the protective specifications, counter to a simple displacement account. This fits the stimulation hypothesis and the original rationale of the Facebook Intensity Measure, developed to capture social-capital benefits of networked communication;[24] meta-analytic evidence likewise identifies perceived support as the one outcome reliably, and more strongly, linked to greater use.[10,16] In an LMIC context, where social media may extend rather than replace kin and peer networks, intensive engagement appears to co-occur with, not erode, support, cautioning against treating all 'high use' as uniformly harmful.

Implications

For research, the central implication is methodological: social-media harm should be reported across a specification curve, or at least across multiple operationalisations, because a single-measure estimate is uninformative about robustness, and primary studies should preregister their operationalisations. For clinical practice, the concentration of signal in attitudinal and time measures, and its absence in network counts, suggests screening is better directed at how central and emotionally absorbing social media is to an adolescent than at follower or account counts. For policy, the finding cautions the current wave of age-limit and screen-time legislation: rules indexed to crude exposure metrics may target the facets carrying the least signal, and small average effects do not, alone, license strong population-level restrictions.

Limitations

Several limitations bound these conclusions. The design is cross-sectional, so no causal or temporal claims are warranted and reverse causation (distress shaping use) remains plausible. Use was self-reported and several measures were coarse, single-item ordinal bands, which attenuates associations and is the likeliest reason the network counts were inert; common-method variance may inflate within-survey associations. The self-esteem scale fell below the conventional reliability threshold (α = .67), so those findings are tentative. Most importantly, the sample (N = 393) is small relative to the population-scale exemplars and underpowered for Orben-sized effects, so the contribution is a proof-of-principle demonstration, not a precise magnitude estimate. Finally, participants were urban adolescents from a single tricity region, limiting generalisation to rural or out-of-school youth.

Conclusion

In an Indian adolescent sample, whether social media appeared ‘associated with’ mental health depended on which facet of use was measured: attitudinal and intensity measures revealed a small, reliable, harm-leaning signal, while network counts revealed nothing, from the same adolescents and data. The effect is real but small, linear rather than Goldilocks-shaped, and robust to covariates and to the analytic pipeline itself. The first lesson of an LMIC specification curve matches that of the large Western ones, now shown to travel: measurement is not a detail but a determinant of the conclusion.

Declarations

Assent was obtained from all participants and written informed consent for participation and for publication of anonymised findings was obtained from their parents or legal guardians. No individual-level data are presented; all results are reported in anonymised, aggregated form."

Supplementary Material

The two supplementary figures below summarise the independent robustness pipeline (256 specifications; partial-correlation metric; four covariate sets). They are provided as online supplementary material.

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Figures

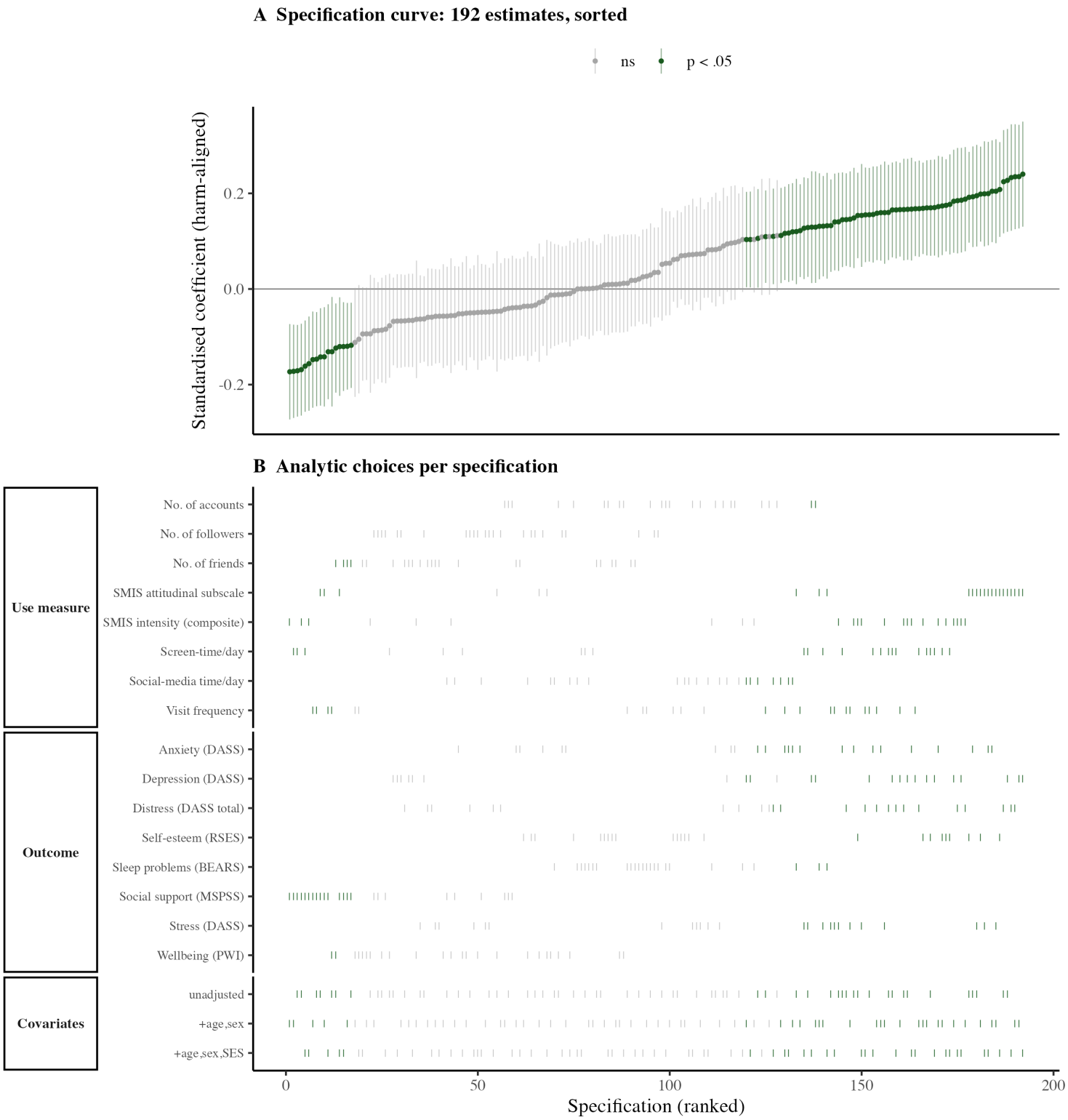


Figure 1

Specification Curve of the Social-Media-Use–Mental-Health Association Across 192 Specifications

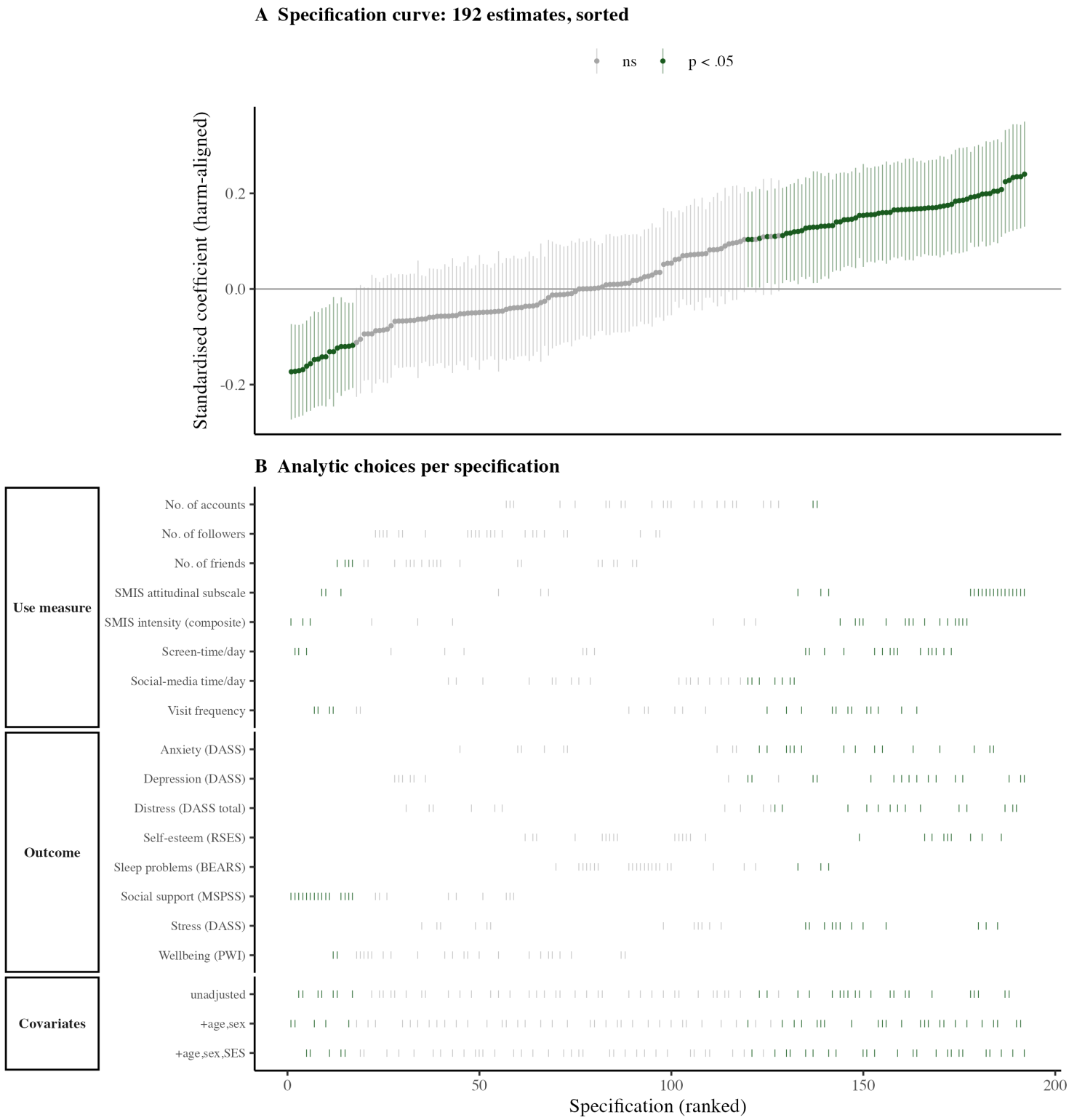


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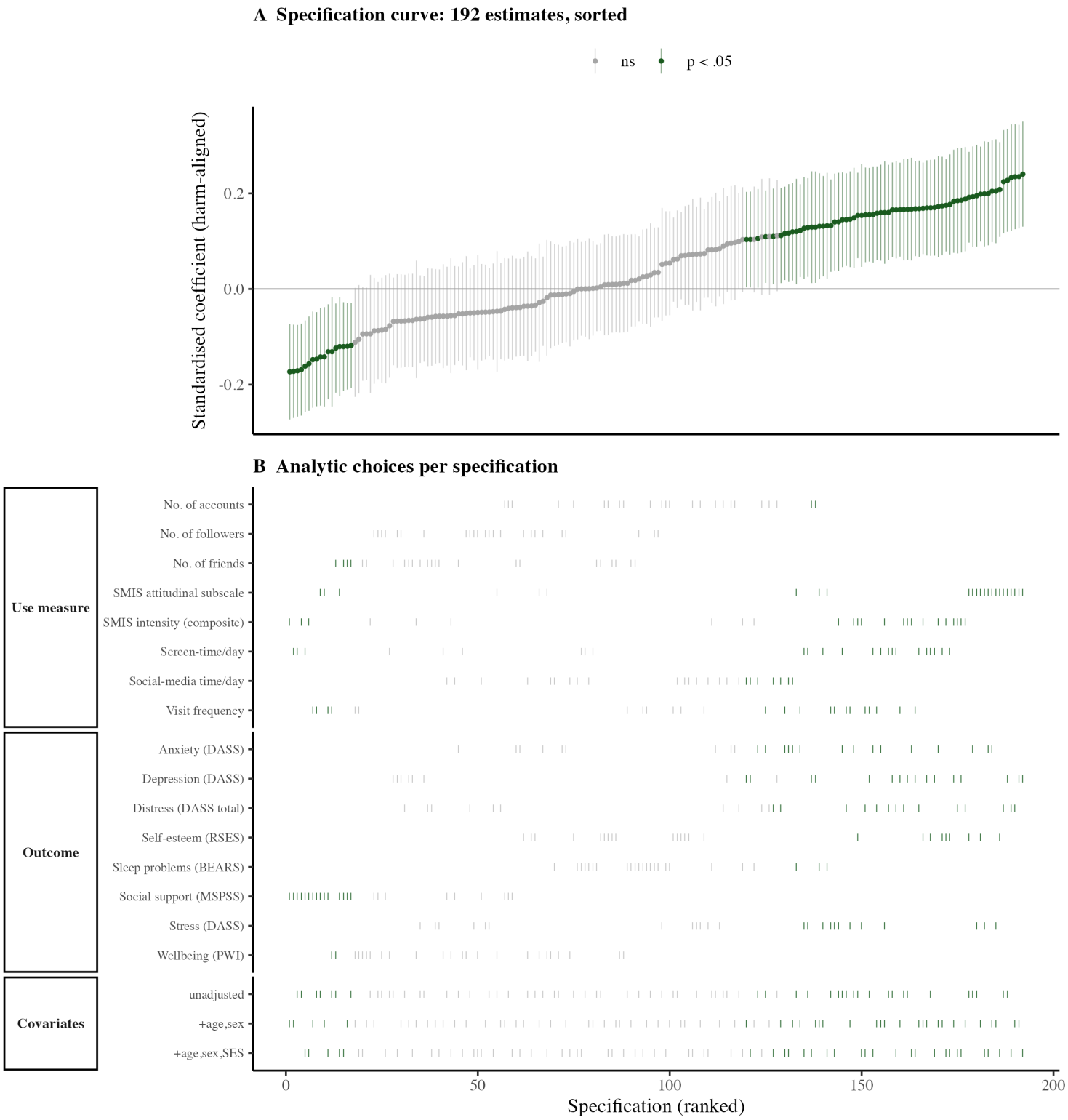


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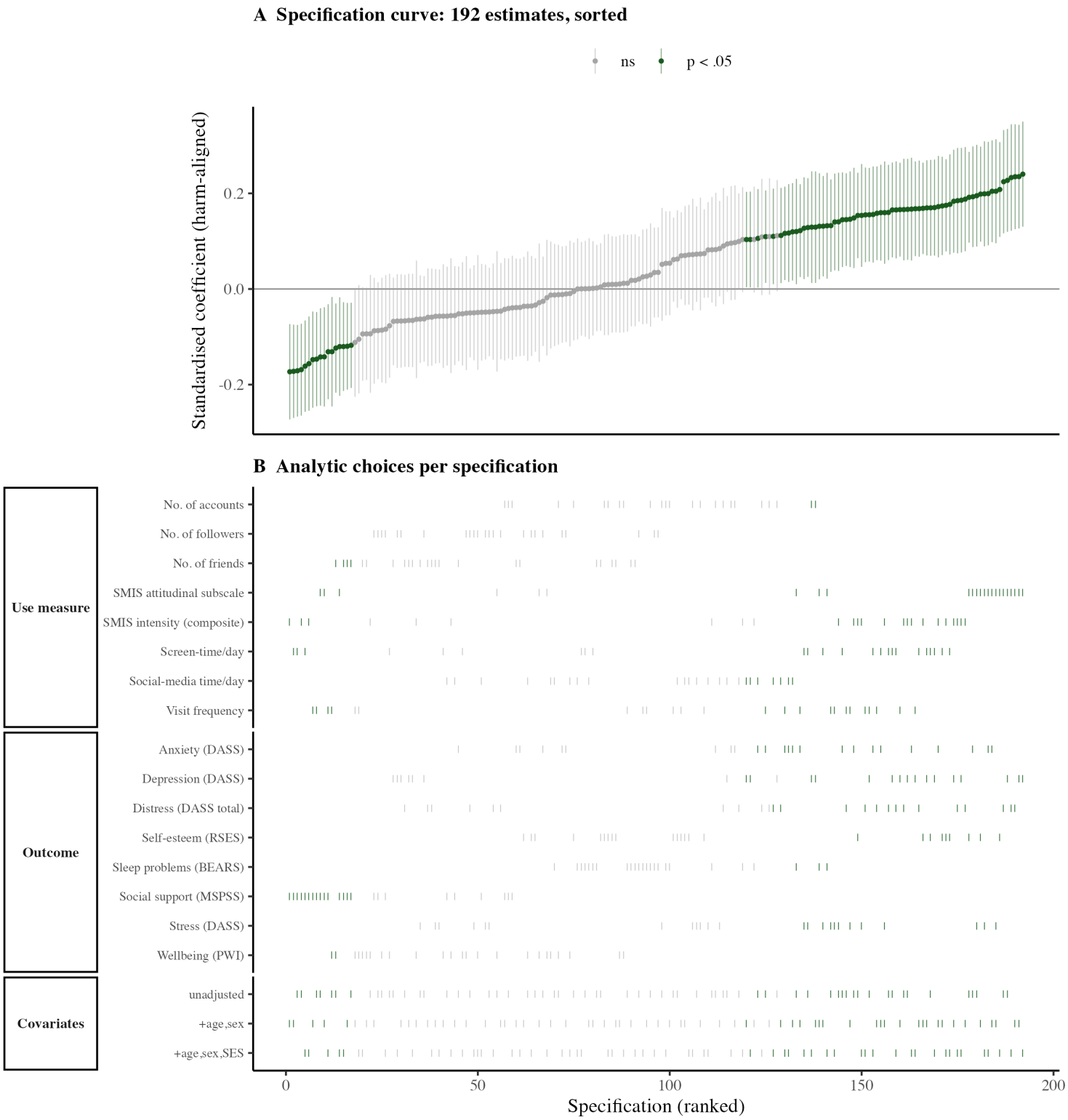


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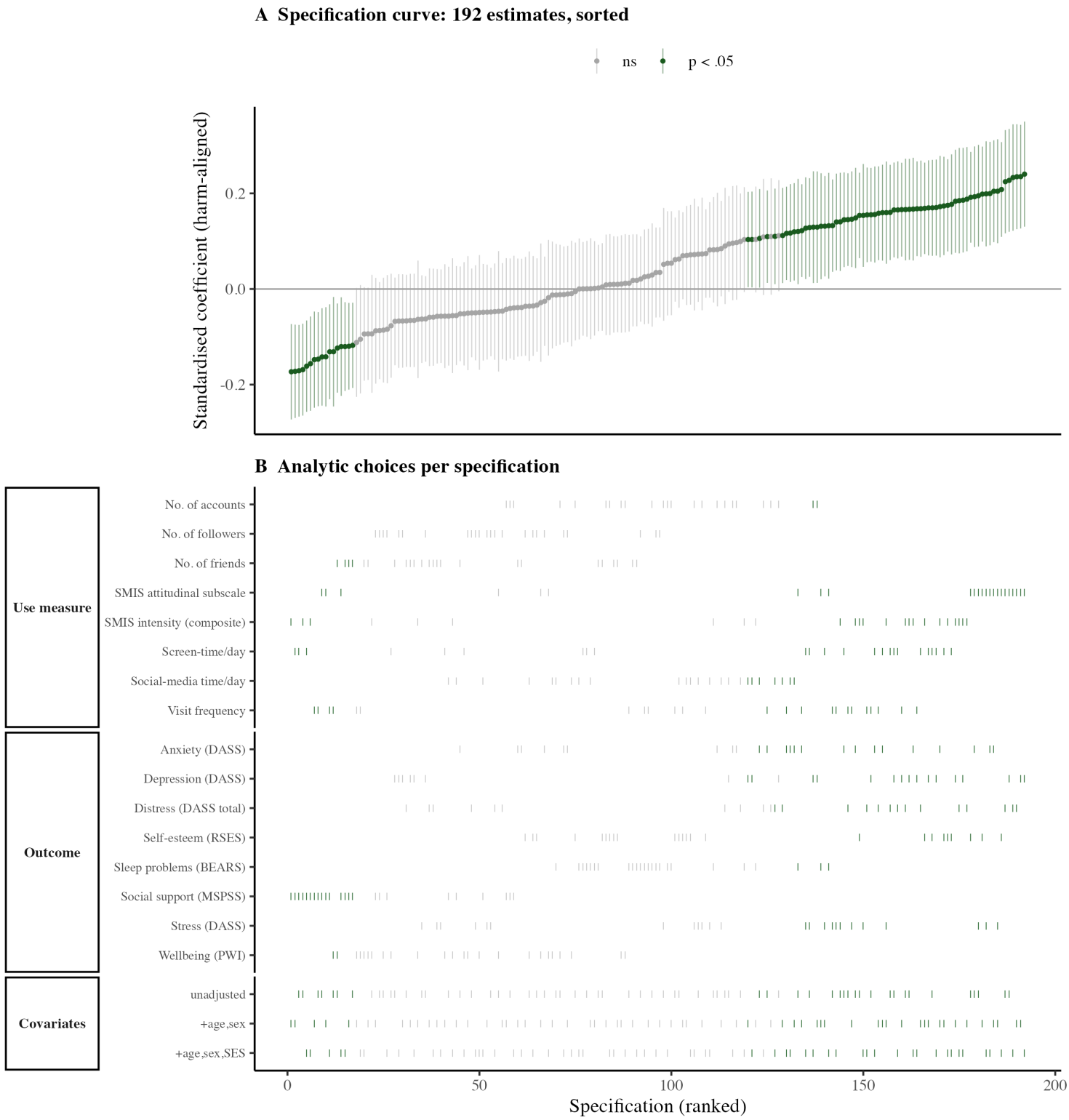


Figure 2

Specification Curve of the Social-Media-Use–Mental-Health Association Across 192 Specifications

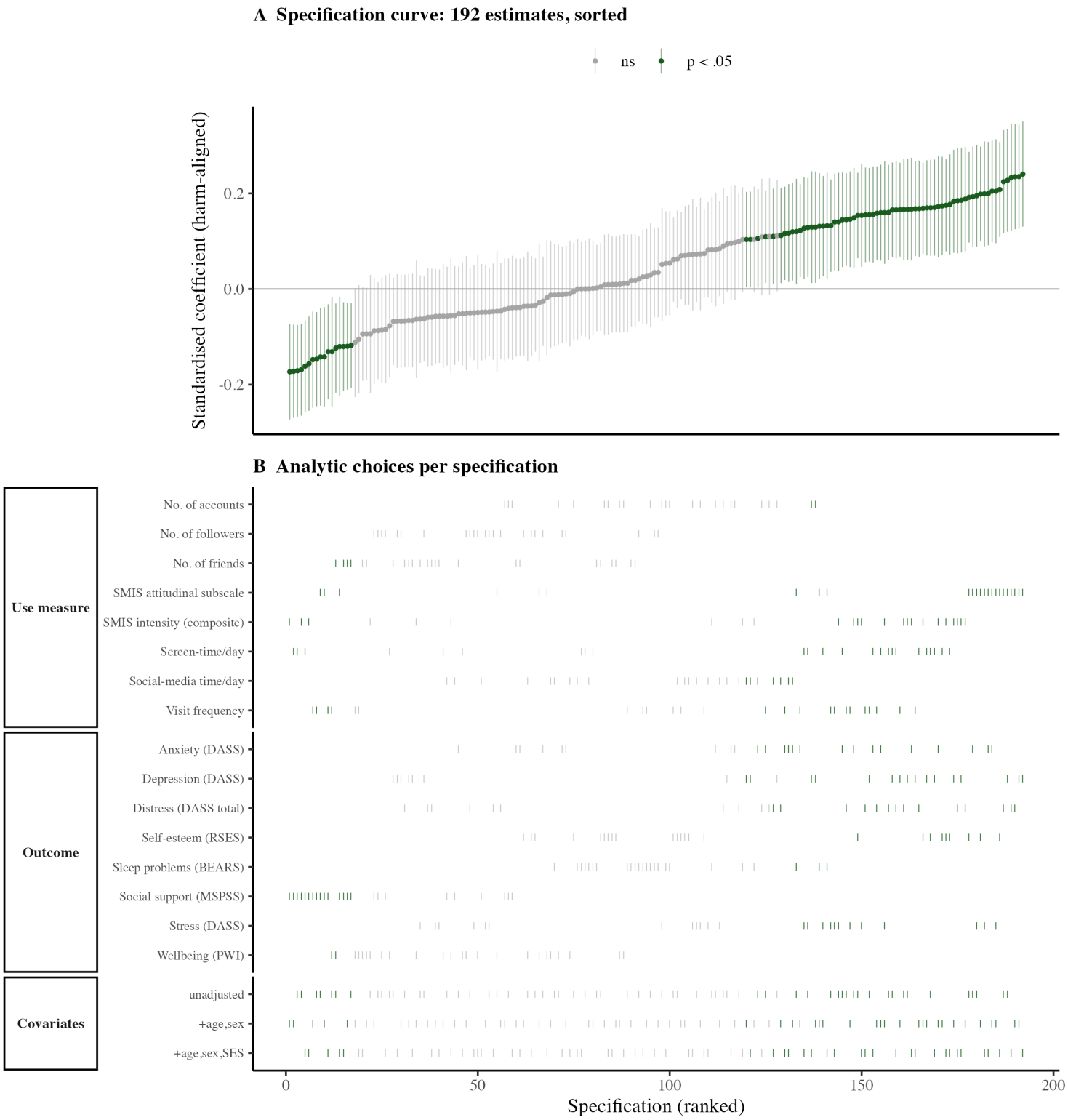


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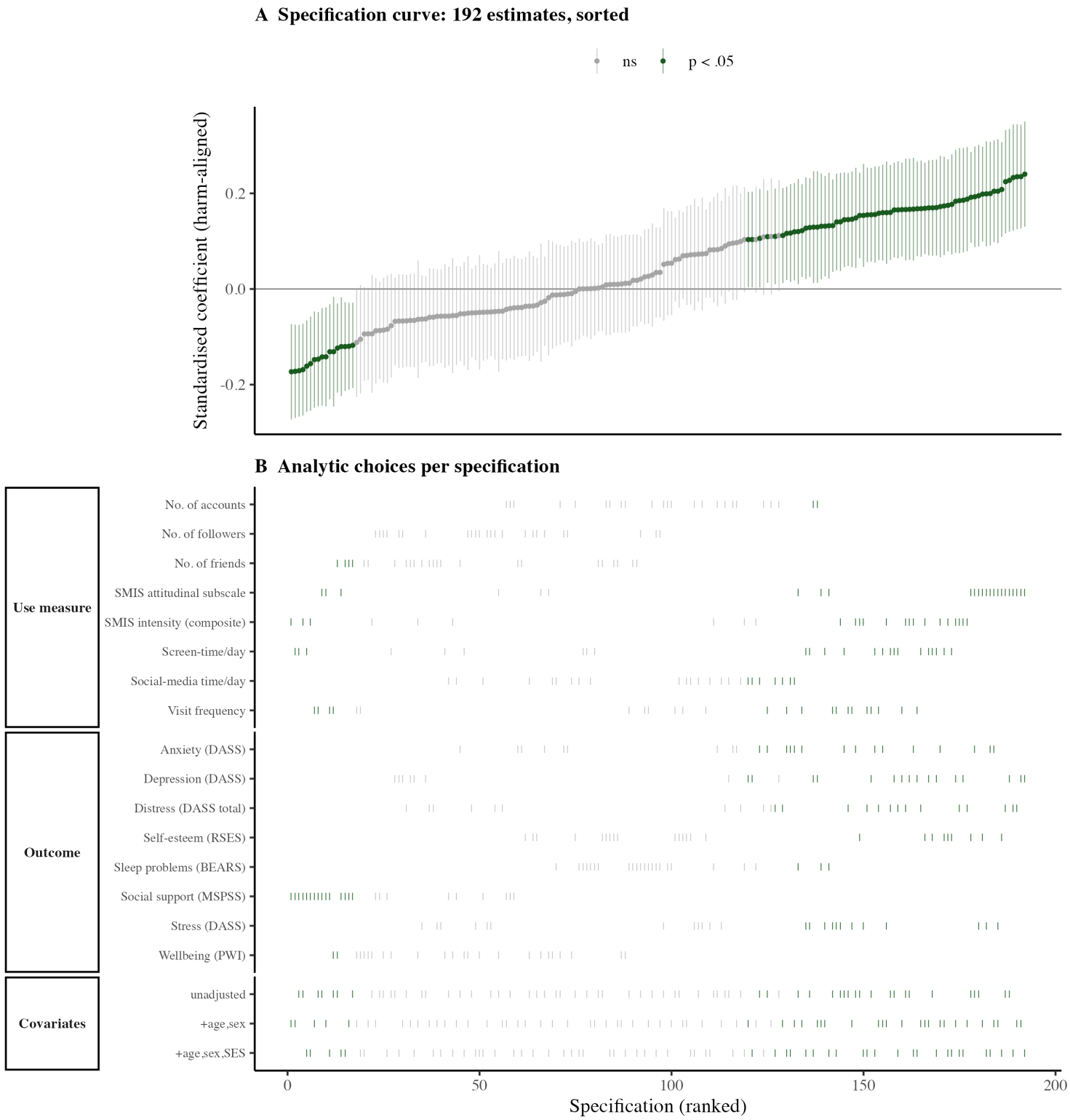


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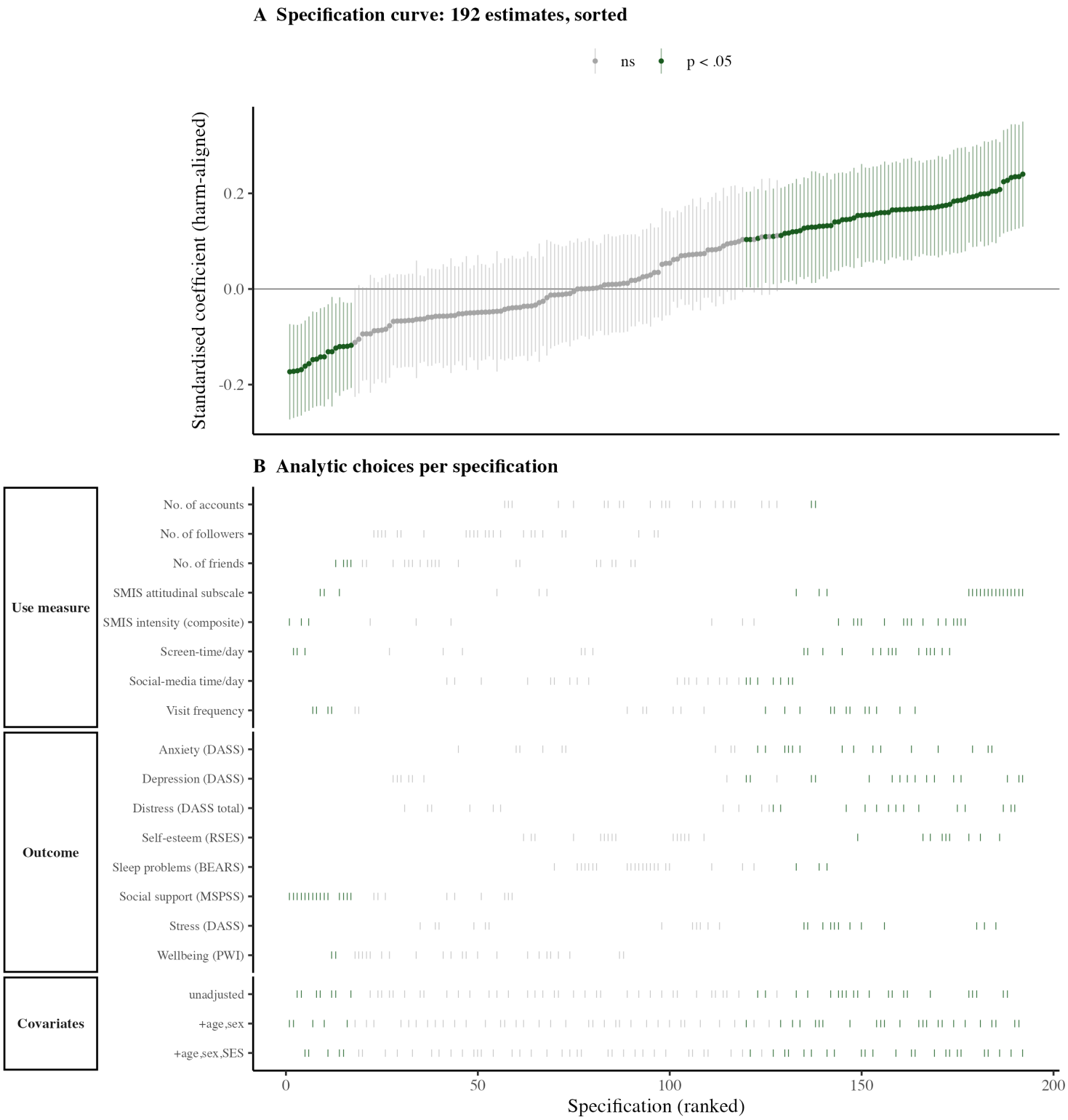


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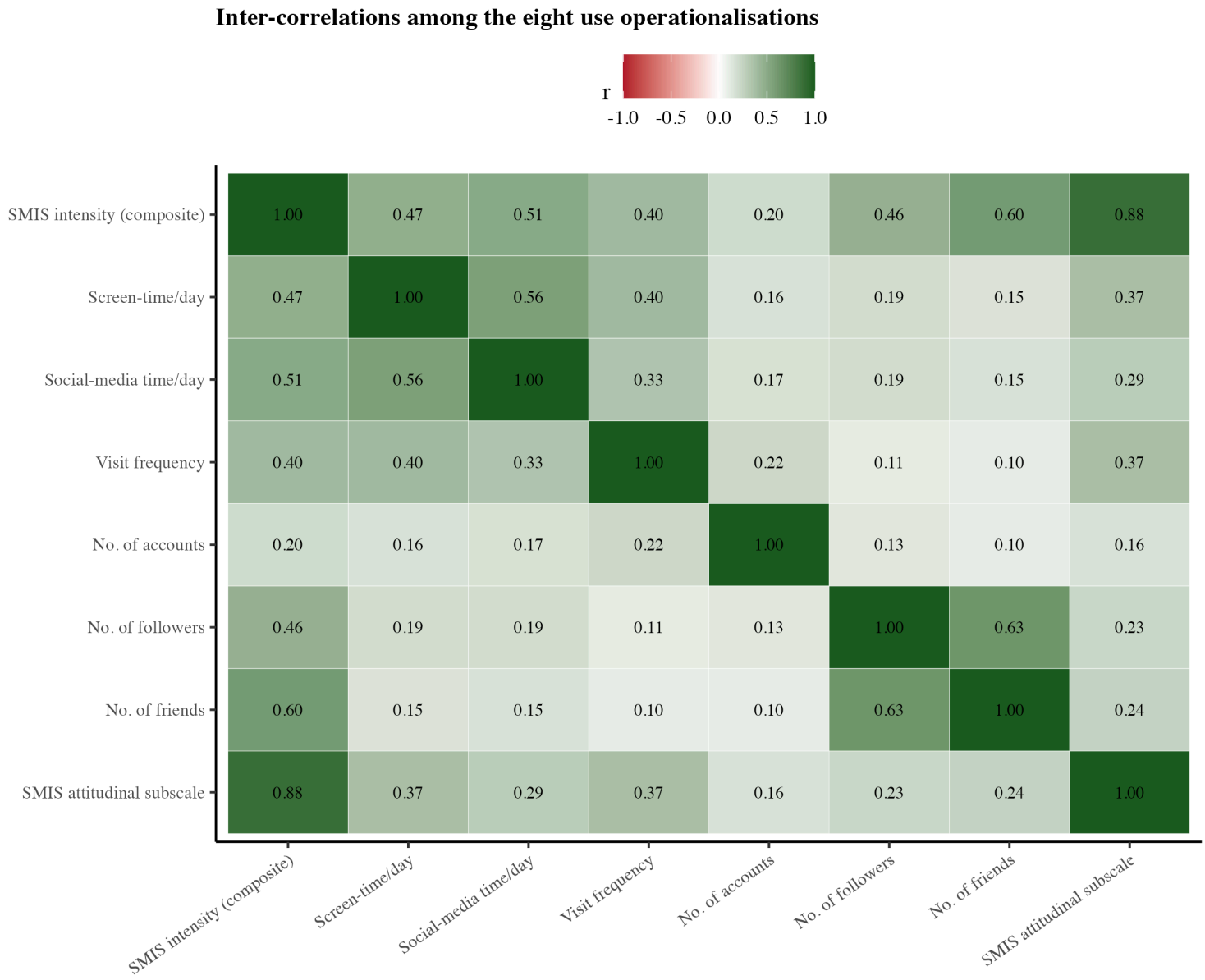


Figure 3

Inter-Correlations Among the Eight Use Operationalisations

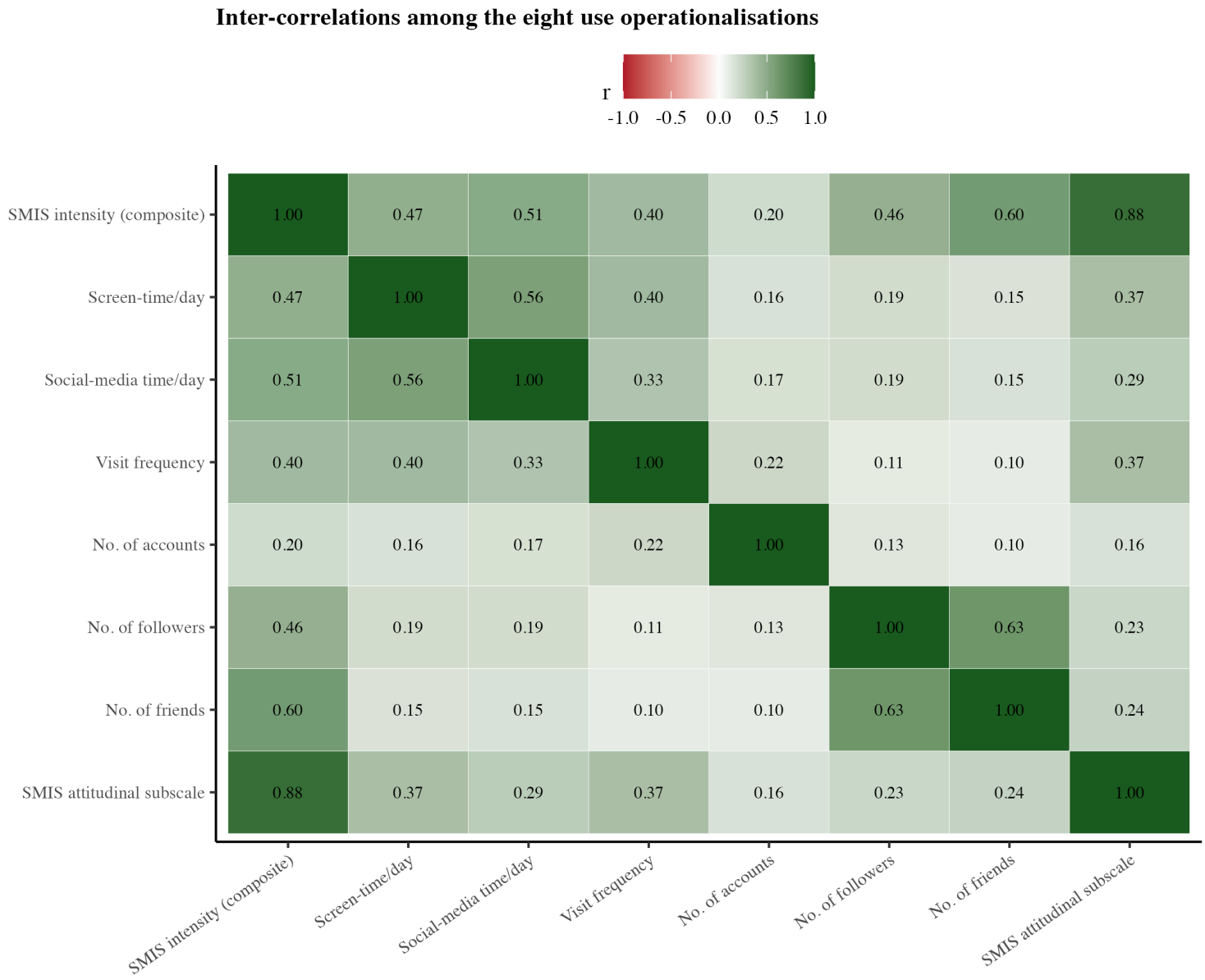


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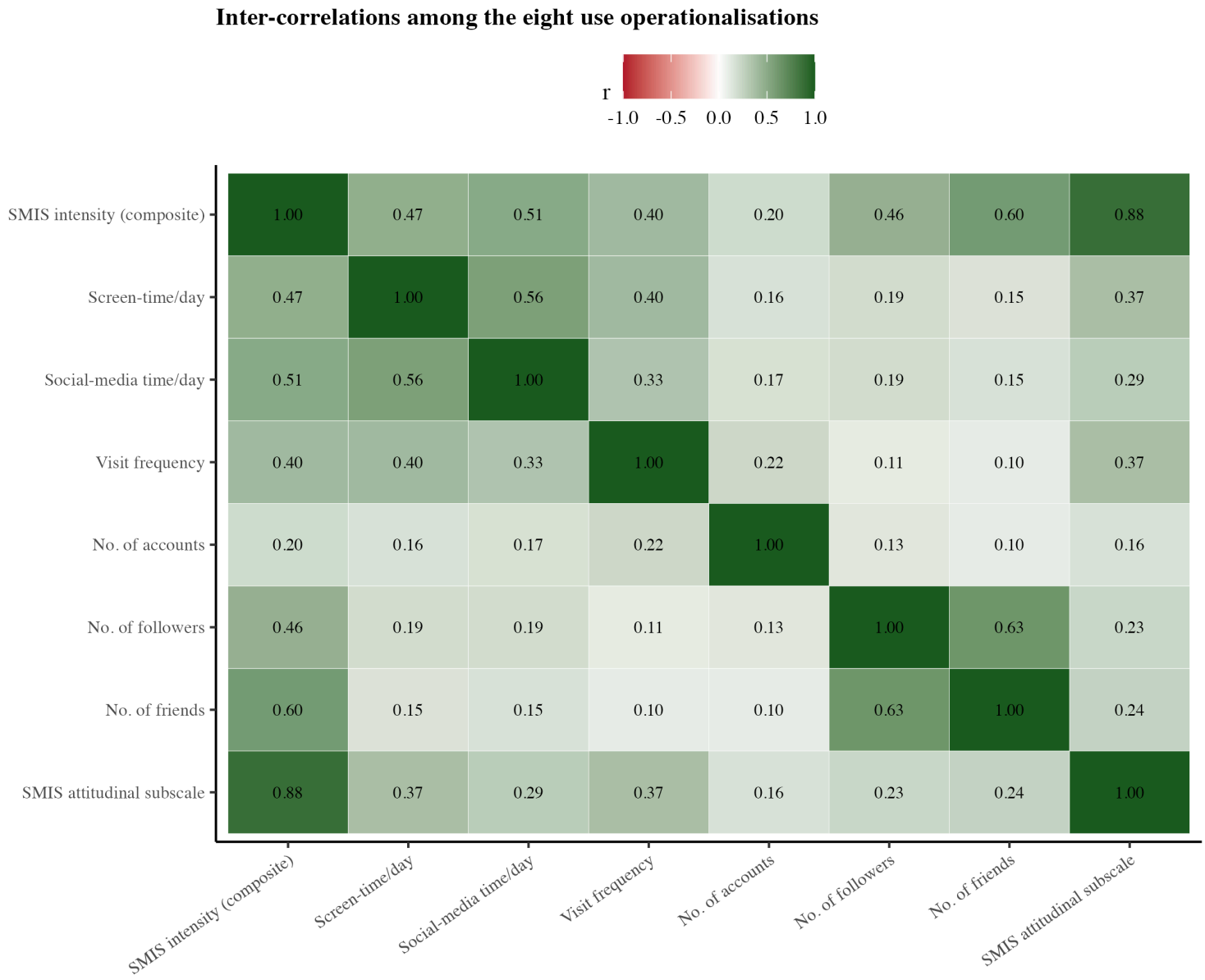


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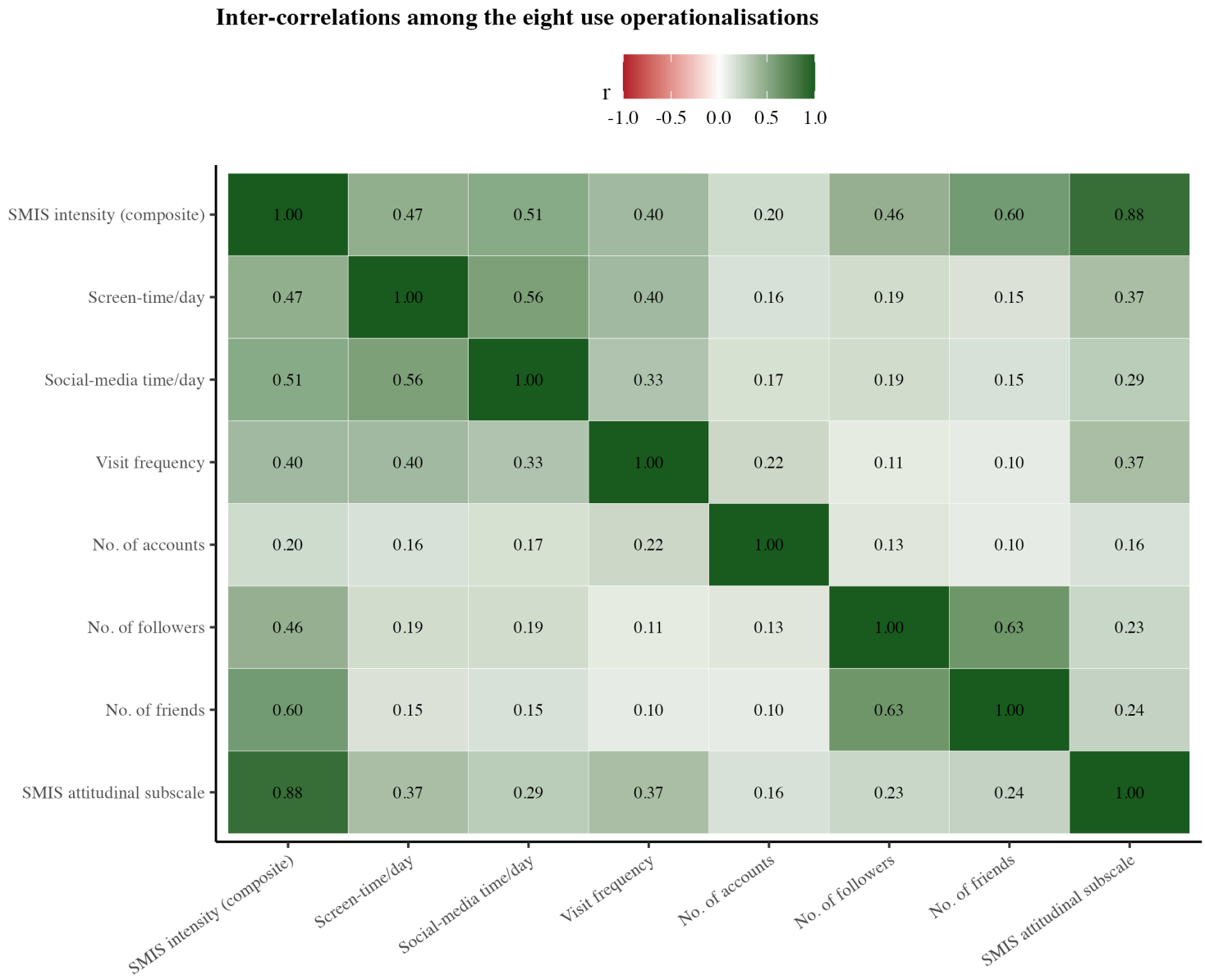


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Supplementary Files

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