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### Authors

Kushlev, Kostadin

Moon, Kibum

Motyl, Matt

et al.



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**Social Technology Use and Life Satisfaction in a Five-Wave Panel Study of U.S. Adults**

Kostadin Kushlev[1*]
Kibum Moon[1]
Matt Motyl[2]
Nathanael J. Fast[3]
Juliana Schroeder[4]

[1] Department of Psychology, Georgetown University, United States
[2] Neely Center, Marshall School of Business, University of Southern California, United States
[3] Marshall School of Business, University of Southern California, United States
[4] Haas School of Business, University of California - Berkeley, United States

* Corresponding Author
kk1199@georgetown.edu

## Abstract

In five waves of panel data (N=1,966 U.S. adults), we examined associations between life satisfaction and self-reported use of ten common social technologies measured every three months on a six-point Likert scale from *not at all* to *multiple times daily*. At this measurement level and time scale, Bayesian and frequentist Random Intercept Cross-Lagged Panel Models showed little credible evidence that *any* social technology use predicts subsequent life satisfaction. In the reverse direction, increases in life satisfaction predicted only modest increases in (video) calling in select demographic groups. In analyses comparing different people, frequency of texting was associated with higher life satisfaction, whereas frequency of YouTube and TikTok use was associated with lower life satisfaction. Despite limited ability to detect within-person change due to temporal stability in responses, the absence of cross-lagged effects is informative: There is scant evidence of a meaningful relationship between social technology use and subsequent life satisfaction.

How is the use of social technologies, such as social media, related to well-being? This question has been hotly debated by researchers and the general public alike, with much dissent. Whereas some researchers suggest that smartphones and social media are detrimental to well-being at least among certain user groups, such as teenage girls[1,2], others have reported that the effects of digital media on adolescent well-being are negligible[3–5] or even positive[6]. In some situations, researchers have reached different conclusions about the relationship between social media use and well-being, even when analyzing the same data[5,7]. Although this debate has centered largely on adolescents, adults make up the largest share of social technology users and spend a substantial and growing part of daily life on these platforms. Their well-being has received far less attention. Examining how everyday social technology use relates to well-being among adults is, therefore, important in its own right, and it offers a broader test of claims often generalized from adolescent samples. In the present research, we focus on exploring the association between social media use and life satisfaction in an adult sample.

One reason for the disagreement about the nature of the relationship between social media and well-being is the lack of theory on relevant controls and mediators, leading to flexible analytical approaches[3]. At least five additional possibilities could account for such divergent findings, all of which we address more carefully in the present study.

First, existing studies either aggregate across different social technology platforms or examine the effects of a single platform. This is problematic because, of course, different technologies feature distinct experiences[8,9]. Although some platforms do share certain characteristics (e.g., recommender algorithms), each new platform typically only achieves widespread adoption to the extent that it offers something unique from existing platforms. Instagram was successful, for example, because it offered image-centric sharing with filters that Facebook lacked; TikTok became popular because it featured short-form videos with algorithmic discovery rather than Instagram's image-focused social graph or YouTube's longer content; and WhatsApp and Snapchat introduced encrypted messaging and ephemeral content that traditional text messaging or email cannot provide. Failing to distinguish between the different platforms could complicate findings. For example, a survey assessing teenagers' "social media use" could produce different conclusions about its relationship to well-being simply depending on whether the survey was administered before or after TikTok's meteoric rise in popularity. Asking whether "social media use" improves well-being is rather like asking whether "eating" is good for

cardiovascular health. Whereas different foods have many shared characteristics, their effects on cardiovascular health vary significantly.

Second, well-being has been measured and operationalized in a range of different ways across the scientific literature on social technology use, including as positive emotions, negative emotions, mood, life satisfaction, domain satisfaction, and symptoms of anxiety and depression[10,11]. Existing studies sometimes use poorly validated measures of these various constructs. Given that different indicators of well-being and their different measurement tools will inevitably vary in their relationship to social technology use, this could create substantial noise when trying to understand the consequences of social technologies.

Third, existing research relies primarily on cross-sectional methods, preventing causal conclusions. For example, it is possible that people use social media more when they struggle with well-being, which would lead to a correlation between usage and poor mental health, but not necessarily indicate that social media harms well-being. In essence, cross-sectional relationships are based on differences between individuals, which may be due to third factors. The few longitudinal studies that do exist suggest that the relationship between social media use and well-being might be bidirectional[12], though no longitudinal studies to our knowledge have explored these associations in adults. Experimental studies have provided some evidence that removing or limiting social media can improve well-being in adults, but are limited by the issues outlined above: either lumping social media together[13] or focusing on a single platform, most commonly Facebook[14–18].

Fourth, the association between social media use and well-being likely depends on a range of demographic variables that have not been adequately explored[11,19,20]. Consider five relevant demographic characteristics: a user's age, gender, race, education level, and income. Regarding age, existing studies almost exclusively focus on adolescents and young adults (i.e., college students). From the higher use of social media in youth[21,22] to the presence of developmentally sensitive periods[23], there are many reasons to believe that social media may have unique effects on younger people that would not necessarily be present in older adults. Regarding gender, different social technology platforms might have different effects on men and women. On Instagram, for example, internal Meta research found that girls experience more negative social comparison, whereas boys experience more bullying (People of the State of California v. Meta Platforms, Inc., No. 4:23-cv-05448, N.D. Cal. 2023). Furthermore, some

studies have found a greater association between social media and well-being in adolescent girls than in boys[7,12,24], but it remains unclear whether this difference persists in adulthood.

Regarding race, some researchers have suggested that social media might be more beneficial for minorities and marginalized communities by allowing people to find representation and social support online[25–27]. In contrast, others have suggested that racial minorities might be at a higher risk of abuse and bullying online[28,29]. Beyond race, a growing literature on digital inequality has begun to document differences by socioeconomic status in the way social technologies are used, people's digital skills, and outcomes of use[30–33]. When it comes to well-being, however, studies typically treat income and education as control variables rather than exploring differences between socioeconomic groups[34].

Fifth, most existing research relies on standard null-hypothesis significance testing, which often yields statistically significant values for trivial associations in large samples. As Ferguson and Heene (2021)[35] argue, statistically significant small effects in large datasets with self-report measures may reflect "crud": Weak associations that arise from random noise or systematic methodological artifacts (e.g., method variance, demand characteristics, single-responder bias, unreliable measures). Because technology use and life satisfaction are typically reported by the same individuals, for example, observed associations may partly reflect single-responder bias, whereby correlations are inflated or distorted by shared response tendencies rather than substantive relationships[35].

In the present research, we address each of these gaps in the existing literature by (1) examining how the use of each social technology platform is uniquely related to well-being, (2) operationalizing well-being with a validated measure of life satisfaction—one of the most well-established and widely used indicators of well-being[36–38], (3) using longitudinal five-wave panel data to explore within-person associations over time, (4) using a large, relatively diverse subsample of adults (N = 1,966) from The Understanding America Study (UAS) to probe differences between demographic groups, and (5) employing a Bayesian approach. Although previous research has tackled some of those gaps[12, 39–40], we aim to address them together in a diverse sample of adults. The UAS (https://uasdata.usc.edu/index.php) features a panel of adults drawn from across the United States via address-based sampling, which mitigates most selection biases associated with opt-in or convenience samples of survey respondents. Table 1 shows details on sample characteristics and demographics.

To leverage our longitudinal data, we use a Bayesian Random-Intercept Cross-Lagged Panel Model (RI-CLPM) framework[41-43]. This model structure (see Extended Data Fig. 1) decomposes variance into between-person and within-person associations. Crucially, the within-person components control for stable trait-level differences and natural autoregressive stability[44], allowing us to isolate the specific cross-lagged associations between an individual's earlier social technology use and their subsequent life satisfaction. While previous research has utilized RI-CLPM to isolate these temporal dynamics in youth[12], no research to our knowledge has applied this approach across platforms in a diverse sample of adults. By focusing on the Bayesian posterior distribution of these isolated within-person parameters, we quantify both the magnitude and uncertainty of the associations, ensuring our conclusions are robust against trivial fluctuations and methodological noise.

## Results

Rather than using traditional, frequentist null-hypothesis significance testing, we opted for a Bayesian parameter estimation framework, which quantifies both the magnitude and uncertainty of the associations. In large samples, frequentist approaches often flag trivial noise as statistically significant[35]. Beyond mitigating this large-sample issue, our Bayesian approach provides a highly descriptive alternative to the rigid, binary decisions inherent in p-value testing. Instead of simply declaring an association significant or non-significant, mapping the posterior distribution transparently illustrates the full shape of the data and its associated uncertainty. This descriptive approach allows readers to visually and mathematically verify exactly where the estimated associations fall. Furthermore, we opted against utilizing Bayes factors to evaluate a point-null hypothesis because they require defining highly specific prior distributions. This is problematic in a research field where there is disagreement about whether the associations between social media use and well-being are negligible and close to null[3] or small and negative[45]. Instead, we evaluate the posterior probability (i.e., the entire range of statistically plausible values for each association) against a region of practical equivalence (ROPE). We specify a Smallest Effect Size of Interest (SESOI) of an absolute value of $|\beta| = 0.10$ to define this ROPE. Following Ferguson and Heene (2021)[35], we consider associations within this range as interpretively negligible. Notably, this threshold accounts for both practical significance and the "crud factor," which encompasses weak associations that arise from systematic methodological artifacts, such as the single-responder bias[35].

**Cross-Lagged Associations Between Earlier Self-Reported Frequency of Social Technology Use and Later Life Satisfaction**

Figure 1 presents the cross-lagged associations between social media use frequency and later life satisfaction (see Extended Data Table 1 for model fits). Our primary estimates are expressed as posterior medians, standardized using pooled standard deviations across participants and waves for each platform. In practical terms, these posterior medians can be interpreted simply as the standardized regression coefficients, allowing us to evaluate these within-person changes directly against our ROPE. Figure 1 also displays the associated 95% credible intervals (teal regions of the posterior distribution). Unlike a frequentist confidence interval, which describes the percentage of hypothetical repeated intervals that would capture a fixed true parameter, a Bayesian credible interval represents the actual probability that the parameter falls within that specific range, given the observed data. The shaded vertical bands between -0.1 and +0.1 signify our ROPE. Importantly, whereas frequentist reporting often obscures the underlying data by collapsing it into binary significance decisions, plotting the full posterior distributions allows readers to visually evaluate the estimated effect sizes against alternative, more or less conservative ROPE thresholds. To test for the presence of heterogeneity in the associations between demographic subgroups, we used log Bayesian Factors to compare RI-CLPMs with coefficients allowed to vary between subgroups (moderation models) with RI-CLPMs with coefficients constrained not to vary (see Extended Data Table 2 for model comparisons).

As shown in Figure 1 (see also Table 2), we find no credible evidence that the use of any social technology platform is related to subsequent life satisfaction in the full sample. Specifically, the posterior probability that the standardized coefficient falls outside our threshold of $|\beta| = 0.10$ was 0% for all platforms. Across all ten of the subgroups, we also find little credible evidence that earlier social technology use predicts more *or* less subsequent life satisfaction beyond what can be explained by methodological noise. Overall, we find little credible evidence that the use of traditional social media platforms (e.g., Instagram, Facebook), entertainment platforms (YouTube, TikTok), or direct communication platforms (e.g., FaceTime, text messaging) is meaningfully associated with subsequent life satisfaction in adults. Rather, any observed fluctuations fall squarely within the bounds of interpretively negligible noise.

**Cross-Lagged Associations Between Earlier Life Satisfaction and Later Self-Reported Frequency of Social Technology Use**

Next, we examine the reverse temporal path: how earlier life satisfaction is related to subsequent technology use (Fig. 2, Table 2). We find little credible evidence that life satisfaction predicts social technology use across the full sample. Even the strongest omnibus association in the case of FaceTime, $\beta = 0.067$, 95% CrI [0.024, 0.109], showed only a 5.87% posterior probability that the association with earlier life satisfaction exceeded 0.10. Looking within specific subgroups, however, we find that an increase in life satisfaction in some groups subsequently predicted greater frequency of FaceTime use, including in racial or ethnic minorities, $\beta = 0.111$, 95% CrI [0.046, 0.173] (62.03% probability of falling beyond the ±0.10 bounds), people without a Bachelor's degree, $\beta = 0.112$, 95% CrI [0.059, 0.166] (67.07% probability), and people with lower incomes, $\beta = 0.101$, 95% CrI [0.048, 0.154] (51.77% probability). The Bayes Factor comparison, however, favored the no-moderation model (Extended Data Table 2). This pattern provides some evidence that greater life satisfaction may predict more frequent calling behavior (at least on FaceTime) but not more frequent scrolling behavior (e.g., on social media platforms).

**Between-Person Associations Between Life Satisfaction and Self-Reported Frequency of Social Technology Use**

Finally, we examined the between-person associations between social technology use and life satisfaction across all five waves (see Fig. 3, Table 3). To ensure these estimates are on an interpretable standardized metric compatible with our ROPE, we report them as latent random-intercept correlations ($r$) rather than raw latent covariances. This allows us to situate and compare our findings more directly with previous cross-sectional research.

In the full sample, people who reported using texting more frequently were more satisfied with their lives, $r = 0.134$, 95% CrI [0.081, 0.186] (88.50% probability of falling beyond ROPE bounds), as were people who reported using email more frequently, $r = 0.129$, 95% CrI [0.076, 0.183] (86.10%). The association of life satisfaction with WhatsApp use was also positive, $r = 0.091$, 95% CrI [0.038, 0.142], but with only 37.10% probability of exceeding the +0.1 bound. In contrast, people who reported using YouTube more frequently tended to be less satisfied with their lives, $r = -0.107$, 95% CrI [-0.160, -0.050] (59.53%), as did people who reported using TikTok more frequently, $r = -0.106$, 95% CrI [-0.155, -0.056] (59.43%). For all other platforms,

we find little credible evidence of meaningful associations: Snapchat, $r = 0.007$, 95% CrI [-0.041, 0.055] (0%), X (Twitter), $r = -0.005$, 95% CrI [-0.055, 0.046] (0%), FaceTime, $r = 0.038$, 95% CrI [-0.015, 0.090] (1.00%), Instagram, $r = 0.034$, 95% CrI [-0.018, 0.083] (0.47%), and Facebook, $r = 0.065$, 95% CrI [0.016, 0.114] (7.97%). Overall, text-based direct communication platforms tended to show more positive between-person associations with life satisfaction, whereas entertainment-oriented platforms tended to show more negative associations.

Looking at demographic subgroups, we found further evidence for this pattern. For TikTok, negative associations where the posterior probability fell below -0.1 by more than 50% were observed in females, non-Hispanic Whites, and younger adults (see Table 3, Fig. 3). For YouTube, negative associations where the posterior probability fell below -0.1 by more than 50% were observed in females, non-Hispanic Whites, and older adults—as well as people with both higher and lower income and level of education. More broadly, because estimating the models separately by subgroup effectively controls for between-group variance, this pattern suggests that controlling for these demographic factors does not fully explain these negative associations between entertainment-based platforms and life satisfaction.

Among the communication-oriented platforms, the positive association between texting and life satisfaction remained meaningful for most subgroups, with more than 50% of the posterior distribution falling beyond the 0.1 bound. These groups included both males and females, both younger and older adults, non-Hispanic Whites, and people with lower levels of education (Table 3). The consistency of this association indicates that the positive association between texting and life satisfaction cannot be explained away by these demographic factors. For WhatsApp, the association in racial and ethnic minorities showed a definitively meaningful pattern, $r = 0.150$, 95% CrI [0.073, 0.229] (89.17%). WhatsApp also showed potentially meaningful associations for males and younger adults (Table 3). Finally, when broken down by groups, the positive omnibus association between email use and life satisfaction remained potentially meaningful when accounting for gender, age, and race, particularly for both males and females, younger adults, and non-Hispanic Whites. Accounting for education and especially income, however, significantly reduced those associations. This pattern suggests that the positive association between email and life satisfaction may be explained by income. For the rest of the platforms, subgroup-specific analyses generally provided little credible evidence of meaningful associations, with the exception of a potentially meaningful positive association between Facebook use and life satisfaction among females (see Fig. 3, Table 3).

**Robustness checks.** Given the overall pattern of lack of credible evidence, we conducted several robustness checks to see if our results, especially those for the cross-lagged associations, could be explained by analytical decisions or methodological issues.

First, we tested whether the lack of credible evidence for the cross-lagged associations may be due to including both users and non-users for each platform in the main models. Excluding non-users of each platform from the models predicting subsequent life satisfaction from previous social technology use, however, similarly produced a ~0% probability of the estimates falling outside the ROPE bounds (see Extended Fig. 2). Regarding the reverse temporal path, the signal for prior life satisfaction predicting subsequent FaceTime use was stronger among active users, $\beta = 0.112$, 95% CrI [0.037, 0.188], with 63.10% of the posterior distribution falling outside the threshold. Furthermore, the notable subgroup associations where life satisfaction predicted subsequent FaceTime use remained consistent (see Extended Data Table 3). The omnibus between-person associations of life satisfaction with YouTube, Texting, and Email also remained consistent, but there was little credible evidence for an association with TikTok among users. Overall, restricting the sample strictly to active users did not meaningfully alter the primary conclusions regarding the lack of credible evidence for the cross-lagged associations.

Second, we tested whether allowing the within-person model parameters to vary by wave rather than constraining them across time would produce different conclusions (see Extended Fig. 3). Examining the coefficients for the association between technology use and later life satisfaction wave by wave (e.g., Wave 1 to Wave 2) showed little credible evidence of meaningful associations, with a maximum of 37.6% of the posterior probability of any coefficient falling outside the ±0.10 bounds. For the reverse temporal path, wave-by-wave analyses again indicated a possibly meaningful relationship for earlier life satisfaction predicting later FaceTime use, with the associations varying between $\beta = 0.036$, 95% CrI [-0.063, 0.142] (12.10% probability of falling beyond the ±0.10 bounds) for Waves 2 to 3, and $\beta = 0.162$, 95% CrI [0.067, 0.262] (89.60% probability) for Waves 4 to 5. The association between earlier life satisfaction and later TikTok use also showed some potentially meaningful, albeit fluctuating patterns: Wave 2 life satisfaction predicted higher self-reported TikTok use at Wave 3, $\beta = 0.150$, 95% CrI [0.072, 0.231] (89.93% probability), but Wave 4 life satisfaction predicted lower TikTok use at Wave 5, $\beta = -0.130$, 95% CrI [-0.214, -0.045] (76.17% probability). Altogether, while unconstraining the models revealed minor temporal fluctuations, the association between

earlier technology use and later life satisfaction remained consistently small and showed little credible evidence of meaningful effects.

Third, we tested whether the use of frequentist RI-CLPMs would produce different conclusions. Because we conducted 110 statistical tests for each type of association, we adjusted p-values using the Benjamini-Hochberg false discovery rate procedure: adjusted p-value = p*(m / k), where p is the unadjusted p-value, m = 110 represents the total number of tests, and k is the rank order of each test based on its p-value, with 1 corresponding to the smallest p-value. Final adjusted p-values were then made monotonic using the standard Benjamini-Hochberg step-up correction. For the most conservative case—the smallest p-value within a 110-test family—the Benjamini-Hochberg critical threshold was 0.00045; with $N$ = 1,966, this corresponds to an approximate detectable effect of $r$ = 0.079 for statistical significance and $r$ = 0.098 for 80% power under a two-sided single-parameter test approximation (for details, see Extended Table 4).

These frequentist models with FDR corrections indicated an identical pattern of lack of credible evidence for earlier social technology use predicting subsequent life satisfaction. For the associations between earlier life satisfaction and later FaceTime use, the frequentist models concluded that all of the subgroup associations for FaceTime deemed potentially meaningful by the Bayesian analyses reached statistical significance (lower education, $p_{adj}$ = 0.0055; lower income, $p_{adj}$ = 0.0055; racial ethnic minority, $p_{adj}$ = 0.0257). For all other platforms, pathways, and subgroups in the cross-lagged analyses, the frequentist analyses yielded the same conclusions of lack of credible evidence as the Bayesian models. The frequentist analyses also showed a generally consistent pattern with the between-person associations flagged as potentially meaningful by the Bayesian models. The frequentist models were, however, slightly less conservative, additionally indicating positive associations of life satisfaction with WhatsApp and Facebook—both omnibus and within specific subgroups (Extended Table 4). Overall, the frequentist checks confirm the conclusion of the Bayesian models, with both statistical paradigms overwhelmingly pointing to a lack of credible evidence of meaningful cross-lagged associations.

Finally, we examined whether there are potential restrictions-of-range issues that could limit the size of the associations. Life satisfaction displayed a typical, negatively skewed distribution with full range. The three-item measure also showed acceptable reliability (see Methods). As shown in Fig. 4 (Panel a), the frequency distributions of use for each platform also represented all possible values on the scale, indicating no obvious restrictions-of-range issues.

The distribution of change in use between adjacent waves also showed a full range of possible values, indicating that some participants reported substantial changes in use between waves, including shifts from using a platform multiple times a day to not using it at all, and vice versa. That said, most adjacent-wave changes were concentrated around zero (Fig. 4, Panel b), indicating substantial within-person stability in self-reported use across waves—even after excluding nonusers. This pattern suggests that the null RI-CLPM findings may partly reflect limited within-person variability in self-reported platform use over time. Thus, the lack of credible evidence of cross-lagged associations should only be interpreted as evidence against robust prospective associations between self-reported social technology use and life satisfaction at the time scale (i.e., an average of 3 months between waves) and level of measurement examined in the present study.

## Discussion

Among a diverse sample of U.S. adults, we found little credible evidence that frequency of use of social media, texting, email, or video calling are prospectively related to later life satisfaction. We also found little credible evidence that earlier life satisfaction predicts later social technology use, except for FaceTime use. People across various subgroups who had higher life satisfaction were subsequently more likely to engage in calling behavior. Life satisfaction may thus predict more frequent (video) calling but not more or less (social media) scrolling. This is consistent with evidence that happier people are more likely to invest time in relationships and social interactions[46,47].

At the between-person level (i.e., not time-lagged analyses), we also found little evidence that use of traditional social media like Instagram or Snapchat is associated with life satisfaction overall and within specific demographic subgroups. This pattern is consistent with recent meta-analytic evidence focusing on adolescents[3]. However, we did find some notable patterns in the between-person analyses, whereby people who used text messaging and email more reported higher life satisfaction, whereas those who used TikTok and YouTube more reported lower life satisfaction. Though the association of life satisfaction with email seemed to be explained by differences in income, the rest of the associations remained robust when controlling for demographic factors and remained largely consistent within demographic subgroups. At the person-level, then, direct communication technologies tended to show more positive associations

with life satisfaction, whereas video-based, entertainment-oriented platforms tended to show more negative associations.

Unlike most other research in this area, we examined social technologies beyond traditional social media, such as email, text messaging, and video calling. It is notable, therefore, that we found little credible evidence that any social technology predicted higher subsequent life satisfaction, either across or within any specific demographic group. Despite their promise and potential to bring convenience, unlimited entertainment, and social connection to users, we find scant evidence that social technologies are making people more satisfied with their lives, at least at the measurement and time scales employed in the current study. Likewise, despite popular claims that these social technologies harm users, we find little credible evidence that social technologies are making people less satisfied with their lives.

Our sample consisted of adults, and results may differ for adolescents. Adolescence is a period of heightened social sensitivity and ongoing neural and emotional development, so the same patterns of use may relate to well-being differently, and perhaps more strongly, among younger users. Our null findings should not be read as evidence that social technologies are inconsequential for adolescents.

Null associations, more generally, should, of course, be interpreted with caution. The most parsimonious interpretation is that, at the timescale and level of measurement examined here, frequency of social technology use simply does not meaningfully shape subsequent life satisfaction in adults. This reading is consistent with recent meta-analytic evidence[3,48] and should be treated as a substantive finding of little credible evidence in its own right rather than merely an absence of one. That said, several alternative explanations could account for the predominantly null pattern across our analyses, and we consider each in turn.

One possibility concerns our measurement of platform use. Because we assessed each technology separately, small negative associations specific to individual platforms might compound when use is aggregated. Our platform-specific measures may therefore have yielded weaker associations than the general "social media use" measures common in prior research. A related concern is the precision of the measure itself. Like the vast majority of existing studies, we relied on a single-item frequency scale with limited response categories, the highest being "multiple times a day." Although we found no obvious restriction of range within the observed data, the measure is range-restricted by design: it cannot distinguish a person who uses a platform a few times a day from one who uses it hundreds of times a day. Our null findings may

thus reflect a lack of evidence for an association among moderate users rather than among heavy users, consistent with research showing that the relationship between use and well-being is curvilinear and concentrated at high levels of use[49].

A second possibility is that we found a lack of credible evidence because we chose a SESOI that is too conservative/high. However, frequentist analyses pointed to largely the same pattern. Still, some have argued that cross-lagged effects should have a different threshold than cross-sectional effects[50], whereby r = 0.03 is a small effect, r = 0.07 is a medium effect, and r = 0.12 is a large effect (vs. 0.1, 0.2, and 0.5 in cross-sectional studies). Notably, however, we did not find meaningful associations for most of our cross-sectional analyses as well as our cross-lagged analyses. Still, a benefit of our descriptive Bayesian approach is that others could easily re-interpret our findings after applying less conservative SESOIs.

A third possibility is that our measure of social technology use captured only overall use rather than how, when, and for what purpose people use these platforms. Our approach, therefore, does not allow us to establish whether the null associations we observed are due to a lack of any associations with a given platform or due to the presence of both positive and negative effects of different types of use that cancel each other out. Of course, these considerations do not mean that using more precise, nuanced, and objective measures of technology use would show stronger relationships. Still, using such precise measures is a necessary step to advance the literature, which is plagued by the same crude measurement issue as the current work.

Finally, a fundamental limitation of our research—and indeed all contemporary studies of social technology use—is that we can only examine patterns of usage within a world already saturated with these technologies. Our findings reveal how variations in platform use relate to life satisfaction among people who inhabit a society where social technologies are omnipresent, shaping not only individual behaviors but also social norms, expectations, and the very means by which we communicate and relate to one another. The question of whether users or society as a whole would be better or worse off without social technologies—or in a world where they exist but are not dominant—remains unanswerable. To truly assess the net impact of social technologies on human well-being would require a counterfactual world where these platforms either never emerged or failed to achieve widespread adoption. Such a comparison is, of course, impossible. That said, recent work in economics does suggest that a great deal of the value that people place on using social media platforms comes from their wide adoption in society; in other

words, people show a preference for not using social media but only in a counterfactual world where most people are not users[51]. Still, our method cannot address the more fundamental question of whether the existence of this ecosystem itself enhances or diminishes human flourishing.

It is also worth noting that, despite the increasing popularity and many strengths of the RI-CLPM, experimental designs remain the strongest way to provide clear causal evidence. Notably, however, the same "counterfactual world" problem applies to even this gold standard method of establishing causality. Even if social media is truly contributing to poor mental health in adolescents, for example, individually assigning adolescents to use or not to use social media might not produce changes in well-being: the benefits might be offset by the costs of exclusion from social interactions and activities with one's peers. Group and population-level interventions (e.g., smartphone bans in schools or blocking social media in all under the age of 16) provide a possible solution to this issue. This underscores the urgent need and critical value for science and society of funding rigorous natural experiments to study the effects of new phone and social media laws.

In sum, the present research speaks to the ongoing question of how social technologies affect human well-being. Across ten of the most commonly used social technologies, we found little credible evidence for associations between use at one time point and life satisfaction at subsequent time points. Notably, this lack of credible evidence does not rule out the possibilities that technology use may influence well-being over other time intervals, for heavy users, in specific contexts, or when assessed using more sensitive and fine-grained measures. Given that public policy debates are being shaped by past research on the relationship between using social technology and well-being, more research with precise measures and rigorous methodologies is urgently needed.

## Method

All research procedures complied with relevant ethical regulations for research with human participants. The Understanding America Study (UAS) is approved by the University of Southern California (USC) Institutional Review Board (protocol UP-14-00148), and all participants provided informed consent. Participants were compensated in accordance with UAS guidelines (for details, see https://uasdata.usc.edu). The present study was not preregistered. No statistical methods were used to pre-determine sample sizes, but our sample size is larger than those reported in many previous publications[3,4].

### Participants

The Understanding America Study (UAS) is an Internet panel of adults drawn from across the United States. People are invited to participate using random, address-based sampling, which reduces selection biases associated with convenience samples and opt-in samples (for details, see https://uasdata.usc.edu/page/Recruitment). Once in the UAS panel, participants complete core surveys every two years, which collect information on their employment, health, cognition, personality, and more. Subsamples of participants are invited to complete additional study-specific measures several times a month.

For this study, N = 3,066 adults were subsampled from the full UAS panel (N ~ 12,000) to complete the social technology measures. The goal of this subsampling was to obtain a diverse cross-section of the U.S. population while oversampling younger participants with the aim of having about 25% of participants in the 18–29 age group, 25% in the 30–39 group, and 50% in the 40+ group. People in the 18–29 age group were born in 1993 or later. N =1,966 responded to the first survey (Wave 1, UAS-514), of which N = 1,928 (62.88%) completed the full survey. Participants who responded to the first wave were invited to complete four additional surveys (Wave 2, 3, 4, & 5; UAS-515–518). The average number of days between two subsequent responses of the same participant was M = 89 days (SD = 43.6). Two participants did not answer any of the social technology use questions, the life satisfaction questions, or both during any of the five waves. See Table 1 for details on demographics, attrition, and missing data. Survey weights were not applied to the analytical models.

Starting at Wave 4, the UAS invited additional participants to complete the social technology substudy, resulting in N = 1,472 additional participants. At Wave 5, N = 211 additional participants completed the survey. To keep the number of waves consistent between participants, we did not include those additional participants in our analyses. In sum, we only included participants who completed Wave 1 and were invited to complete all subsequent waves, N = 1,966.

**Measures**

During each wave, we measured life satisfaction with a validated three-item scale from the Comprehensive Inventory of Thriving[38]: *I am satisfied with my life; In most ways, my life is close to my ideal; My life is going well.* These items are some of the most widely used and well-established items for measuring life satisfaction[36,37]. Participants were instructed to rate how strongly they agree or disagree with each statement on a scale from *1–Strongly Disagree* to *7–Strongly Agree* (for descriptives, see Table 1). We computed life satisfaction scores for any participant who answered at least one of the three questions. Using the *mlr* function from the *psych* package in R, we conducted a multilevel generalizability analysis to assess the reliability of the life satisfaction measure across five time points with three items. The reliability capturing stable individual differences was excellent at RkRn = .94. The reliability capturing within-person change over time was acceptable, Rcn = .47, indicating that 47% of the observed within-person fluctuations in the life satisfaction measure represent reliable fluctuations rather than random measurement error[52]. While .47 may be considered modest by conventional standards for between-person reliability, it has been suggested that more lenient criteria may be appropriate in within-person contexts, particularly when scales include fewer items[53].

During each wave, participants were also asked to indicate whether or not they had used one of the following social platforms in the past 28 days: *Facebook, Twitter/X, Instagram, TikTok, Snapchat, YouTube, Reddit, WhatsApp, Mastodon, LinkedIn, Pinterest, Twitch, Nextdoor,* and *Discord*. Participants were also asked whether or not they had used services in the following categories: *Email, Text Messaging, FaceTime, Dating Apps*, and *Online Gaming*. If participants indicated that they have used each of those platforms, they were further asked to indicate how frequently they had used it, from the following options: *Multiple times per day; About once a day; A few times per week; About once a week; Less than once a week; I did not use*. We coded the responses so that 5 indicated the highest frequency and 0 indicated not using a given platform at all, including participants who had not selected a given platform on the initial question. If a participant indicated they used a platform in the dichotomous question but did not answer the follow-up frequency question, we coded their response as missing (n = 110). In the present report, we focus on the ten most used platforms/categories as reported in the UAS sample. Consequently, our analyses excluded the following platforms and categories: *Mastodon, Reddit, LinkedIn, Pinterest, Twitch, Nextdoor, Discord, Dating Apps*, and *Online Gaming.*

Participants indicated their gender as either female or male. We also coded the rest of the demographic factors into binary variables, as follows. Participants were asked to report their highest level of education, which we classified as either 'Associate College Degree or Less' or 'Bachelor's degree or Higher.' Race and ethnicity were categorized into "Non-Hispanic White" and "Racial and Ethnic Minority," which included participants identifying as Hispanic or Latino, as well as any of the following races: Black/African American, Asian, American Indian/Alaskan Native, and Native Hawaiian/Pacific Islander. Participants who selected more than one race, including White, were also classified as a minority. Household income was categorized into "lower income" (less than $75,000) and "higher income" ($75,000 or more), with the division based on an approximate median split of income levels within the sample ($60,000 to $74,999) and the 2023 U.S. median household income ($80,610) as reported by the U.S. Census Bureau (see Table 1).

**Statistical Analyses**

We used random intercept cross-lagged panel models (RI-CLPM)[43] using a Bayesian framework[54]. These models, based on structural equation modeling (SEM), effectively account for stable, trait-like characteristics by including random intercepts. These random intercepts partial out between-person variance so that the lagged relationships in the model pertain only to

within-person dynamics[43]. The Bayesian SEM framework further allowed characterizing uncertainty by comparing the range of posterior distributions and a region of practical equivalence (ROPE) around the null value[55,56].

To test the cross-lagged relationships between social media use and life satisfaction, we ran a separate Bayesian RI-CLPM for each platform. We adopted the model structures and R code from Mulder and Hamaker[42]. Random intercepts were included as latent variables to capture between-person means, such as the average frequency of self-reported social technology use and life satisfaction across all waves for each participant.

To ensure our within-person estimates are readily interpretable as standard effect sizes while preserving natural temporal fluctuations in variance, we standardized all observed variables using grand pooled standard deviations calculated across all participants and waves prior to model estimation. Consequently, the resulting cross-lagged estimates can be interpreted as standardized regression coefficients (β). Specifically, a given β indicates the predicted change in an outcome—measured in total pooled standard deviations—associated with a one-standard-deviation shift from a participant's own baseline average on the predictor. Crucially, this grand-mean standardization approach establishes a universal, stable metric, allowing us to evaluate the cross-lagged within-person fluctuations against our ROPE and conceptually compare their magnitude to the between-person associations.

For the between-person associations, we report the latent random-intercept (RI) correlations (r) rather than the raw latent covariances estimated by the RI-CLPM. Although our observed variables were standardized to a variance of 1, the RI-CLPM partitions this total variance into within- and between-person components, meaning the latent random-intercept variances are not necessarily equal to 1. As a result, the raw latent covariance remains scale-dependent and cannot be meaningfully evaluated against a standard effect-size threshold. By converting these covariances into latent correlations by dividing each posterior covariance draw by the product of the corresponding latent RI standard deviations, we ensure mathematical compatibility with our ROPE framework, align the scale of interpretation with our within-person estimates, and make our between-person estimates directly comparable to the extensive existing literature relying on cross-sectional correlations.

Models were estimated in *blavaan 0.5-8* using the Stan backend[54] with three Markov chains, 1,000 burn-in iterations, and 1,000 retained posterior draws per chain. Because we did not have strong prior expectations about the size of the platform-specific cross-lagged

associations, we retained *blavaan*'s default weakly informative prior distributions rather than imposing more informative parameter-specific priors. Specifically, default priors were normal for intercepts, factor loadings, and regression parameters, gamma priors on standard deviations, beta priors on correlations, and Wishart priors for covariance-matrix parameters, where applicable. For full details on the model specification and *blavaan* code, see the Code Availability section.

Instead of relying on maximum likelihood estimation and Full Information Maximum Likelihood (FIML), we summarized results using posterior medians, 95% credible intervals, and the posterior probability that the absolute value of an association exceeded 0.10[55,56]. Model fit was evaluated using Bayesian fit indices including, BRMSEA, BGammaHat, adjusted BGammaHat, and BMc[42,57]. All models showed good to excellent fits. Additional details are available in the Extended Data Table 1. We constrained the random intercepts for both social platform use and life satisfaction to remain stable over time; thus, they vary between, but not within, participants. Latent within-person deviation variables were used to capture the within-person fluctuations over time. For parsimony in our main analyses, we constrained all bidirectional and autocorrelated associations to remain static across waves. Thus, we treated the within-person relationships between variables as consistent over time. Details on models where the coefficients were allowed to vary by wave are reported in the Extended Data. Further information on model specifications is provided in Extended Data Table 5.

In the moderation analyses, we conducted planned Bayesian model comparisons for each platform-by-moderator combination (see extended data Table 2 for details). For each combination, we tested moderation separately for three targeted associations: the cross-lagged association between earlier self-reported frequency of social technology use and later life satisfaction, the cross-lagged association between earlier life satisfaction and later self-reported frequency of social technology use, and the between-person association. For each targeted association, we contrasted otherwise identical models in which that association was either constrained to be equal across groups or allowed to differ across groups. Log Bayes factors compared the no-moderation model against the corresponding moderation model for each targeted association; positive values favored the no-moderation model, whereas negative values favored the moderation model. We classified model support only when the absolute log Bayes factor was at least 1; smaller absolute values were treated as inconclusive. Notably, these model

comparisons are not equivalent to standard moderation analyses, providing a more conservative test of moderation.

Despite its strengths, the RI-CLPM has been criticized for not accounting for wave-specific state components, including measurement errors or reliable wave-specific variances, potentially leading to spurious findings[58]. The Stable Trait Autoregressive Trait and State (STARTS) model has been proposed as an alternative to address these issues by incorporating wave-specific state components[59]. However, while preferable in theory, the STARTS model is often difficult to estimate, as it requires a large number of waves (10 or more in practice) and is prone to estimation issues, such as convergence failures or negative variance estimates[59–61]. We also encountered these issues during our attempts to apply the STARTS model. Thus, we opted to use the RI-CLPM as a more practical and reliable approach for our analyses. Notably, when the STARTS models did converge, we did not observe any reversal of associations from negative to positive compared to the RI-CLPMs. According to Sorjonen and colleagues (2023)[58], the presence of reversal of associations between the two types of models can be seen as evidence that the RI-CLPM is producing spurious relationships; hence, we found no such evidence of spurious relationships in our data. But the observed relationships and their causal nature should still be interpreted with caution.

**Data Availability**

Data are available to researchers by application through the UAS data portal (https://uasdata.usc.edu/). Access is governed by the UAS data-use agreement, and the authors are not permitted to redistribute the data.

**Code Availability**

Analysis code is available on the Open Science Framework.[62]

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**Author Contribution Statement**

All authors have made substantial contributions to the work. Each author contributed to the conceptualization of the project. K.K. and K.M. drafted the manuscript, and all authors revised the manuscript. K.M. conducted the formal statistical analyses, including statistical coding and data cleaning. M.M., N.J.F., and J.S. contributed to the development of survey materials, including survey items. None of the authors was directly involved in the collection of the data; the data was collected by the Understanding America Study. All authors have approved the submitted version and agree to be personally accountable for their own contributions while helping ensure the integrity of the work as a whole.

**Competing Interests Statement**

The authors declare no competing interests.

## Tables

**Table 1**| Sample demographics and descriptive information

| | Wave 1 | Wave 2 | Wave 3 | Wave 4 | Wave 5 |
|---|---|---|---|---|---|
| Wave Start | Mar 2023 | Aug 2023 | Sep 2023 | Nov 2023 | Feb 2024 |
| Wave End[1] | May 2023 | Sep 2023 | Oct 2023 | Feb 2024 | May 2024 |
| **N** | 1,966 | 1,619 | 1,600 | 1,512 | 1,500 |
| **Response Rate**[2] | 100% | 82.35% | 81.38% | 76.91% | 76.30% |
| **Age**[3] | | | | | |
| Mean (SD) in years | 40 (15) | 41 (15) | 41 (15) | 41 (15) | 41 (15) |
| Number with Age Under 30 Years | 539 (27%) | 401 (25%) | 399 (25%) | 374 (25%) | 368 (25%) |
| Range | 18, 99 | 18, 89 | 18, 89 | 18, 89 | 18, 89 |
| Missing (n) | 3 | 2 | 2 | 2 | 3 |
| **Gender** | | | | | |
| Female | 1,259 (64%) | 1,023 (63%) | 1,001 (63%) | 941 (62%) | 943 (63%) |
| Male | 705 (36%) | 594 (37%) | 598 (37%) | 570 (38%) | 555 (37%) |
| Missing (n) | 2 | 2 | 1 | 1 | 2 |
| **Education** | | | | | |
| Assoc. College Degree or less | 1,051 (53%) | 851 (53%) | 850 (53%) | 779 (52%) | 778 (52%) |
| Bachelor's Degree or Higher | 915 (47%) | 768 (47%) | 750 (47%) | 733 (48%) | 722 (48%) |
| **Race/Ethnicity**[4] | | | | | |
| Non-Hispanic White | 1,263 (64%) | 1,068 (66%) | 1,047 (66%) | 1,000 (66%) | 989 (66%) |
| Racial and Ethnic Minority | 696 (36%) | 545 (34%) | 548 (34%) | 506 (34%) | 507 (34%) |
| Missing (n) | 7 | 6 | 5 | 6 | 4 |
| **Household Income**[5] | | | | | |
| Income >= $75,000 | 892 (46%) | 758 (47%) | 736 (46%) | 715 (47%) | 712 (48%) |
| Income < $75,000 | 1,067 (54%) | 855 (53%) | 858 (54%) | 791 (53%) | 781 (52%) |
| Missing (n) | 7 | 6 | 6 | 6 | 7 |
| **Life Satisfaction**[6] | | | | | |
| Mean (SD) | 4.98 (1.38) | 4.99 (1.42) | 4.94 (1.43) | 4.95 (1.40) | 5.04 (1.42) |
| Range | 1–7 | 1–7 | 1–7 | 1–7 | 1–7 |
| Missing (n) | 39 | 35 | 28 | 42 | 38 |

1. Participants completed the surveys in the same order from Wave 1 to 5; each participant had at least 15 days between surveys. 2. We examined differences in response rates by observable variables; older people, females, those with an associate's college degree or less, those from lower-income households, and those from racial and ethnic minority backgrounds were less likely to respond (see SOM for details). 3. Demographic variables represent reports at the time of Wave 1; people in the young adult group were born in 1993 or later. 4. Race and ethnicity were categorized into "Non-Hispanic White" and "Racial and Ethnic Minority," which included participants identifying as Hispanic or Latino, as well as any of the following races: Black/African American, Asian, American Indian/Alaskan Native, and Native Hawaiian/Pacific Islander. 5. Income was categorized based on being above or below $75,000 because it was the closest category we measured to the median household income in the United States ($80,610 according to the U.S. Census Bureau, 2024). 6. Life satisfaction was assessed with a validated three-item measure *(I am satisfied with my life; In most ways, my life is close to my ideal; My life is going well*) on a scale from *1–Strongly Disagree* to *7–Strongly Agree*; life satisfaction items were administered later in the survey, possibly resulting in more missing data.

**Table 2 |** Summary of cross-lagged associations: Median posteriors standardized using pooled standard deviations across participants and waves for each platform and associated posterior probability that the coefficients exceed the bounds of the region of practical equivalence.

| Platform | Omnibus | Gender | | Age | | Education | | Household Income | | Race and Ethnicity | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Male | Female | 18–29 | 30 or older | Assoc. Degree or Lower | Bachelor Degree or Higher | Income < 75k | Income ≥ 75k | Non-Hispanic White | Racial or Ethnic Minority |
| Associations between earlier self-reported frequency of social technology use and later life satisfaction | | | | | | | | | | | |
| FaceTime | 0.020 (0%) | 0.025 (0.10%) | 0.017 (0%) | 0.013 (0%) | 0.023 (0%) | 0.018 (0%) | 0.024 (0%) | 0.016 (0%) | 0.023 (0%) | 0.036 (0%) | 0.002 (0%) |
| Texting | 0.003 (0%) | -0.002 (0%) | 0.007 (0%) | 0.007 (0%) | 0.002 (0%) | 0.015 (0%) | -0.008 (0%) | 0.008 (0%) | 0.002 (0%) | -0.004 (0%) | 0.016 (0%) |
| WhatsApp | -0.002 (0%) | -0.022 (0.10%) | 0.009 (0%) | 0.019 (0.40%) | -0.011 (0%) | 0.024 (0.10%) | -0.017 (0%) | 0.008 (0%) | -0.009 (0%) | 0.014 (0.03%) | -0.019 (0.03%) |
| Email | -0.012 (0%) | -0.010 (0%) | -0.013 (0%) | -0.040 (0.30%) | -0.003 (0%) | -0.015 (0%) | 0.003 (0%) | -0.013 (0%) | -0.003 (0%) | -0.006 (0%) | -0.018 (0%) |
| Facebook | -0.013 (0%) | 0.004 (0%) | -0.025 (0%) | -0.069 (14.20%) | 0.014 (0%) | -0.032 (0%) | 0.014 (0%) | -0.015 (0.03%) | -0.012 (0.03%) | -0.027 (0.03%) | 0.009 (0.03%) |
| Instagram | -0.016 (0%) | -0.018 (0.37%) | -0.014 (0%) | -0.067 (13.43%) | 0.012 (0%) | -0.046 (0.87%) | 0.029 (0.23%) | -0.049 (1.03%) | 0.031 (0.30%) | 0.023 (0.03%) | -0.062 (7.83%) |
| Snapchat | 0.003 (0%) | -0.005 (0.27%) | 0.009 (0.03%) | -0.011 (0.13%) | 0.018 (0%) | -0.011 (0%) | 0.027 (0.67%) | -0.016 (0%) | 0.033 (0.77%) | 0.025 (0.17%) | -0.021 (0.03%) |
| X (Twitter) | -0.031 (0%) | -0.050 (0.70%) | -0.011 (0%) | -0.019 (0.07%) | -0.034 (0.03%) | -0.050 (1.23%) | -0.016 (0%) | -0.044 (0.43%) | -0.013 (0%) | -0.008 (0%) | -0.060 (4.07%) |
| TikTok | 0.019 (0%) | 0.029 (0.97%) | 0.015 (0%) | 0.036 (0.77%) | 0.011 (0%) | 0.013 (0%) | 0.023 (0.13%) | 0.003 (0%) | 0.032 (0.43%) | 0.042 (0.23%) | -0.009 (0%) |
| YouTube | -0.012 (0%) | -0.001 (0%) | -0.016 (0%) | -0.022 (0.03%) | -0.007 (0%) | -0.024 (0%) | 0.004 (0%) | -0.028 (0%) | 0.014 (0%) | -0.014 (0%) | -0.008 (0%) |
| Associations between earlier life satisfaction and later self-reported frequency of social technology use | | | | | | | | | | | |
| FaceTime | 0.067 (5.87%) | 0.039 (3.47%) | 0.081 (24.83%) | 0.059 (15.50%) | 0.072 (13.47%) | 0.112 (67.07%) | -0.000 (0.33%) | 0.101 (51.77%) | -0.006 (0.17%) | 0.041 (2.07%) | 0.111 (62.03%) |
| Texting | 0.051 (3.00%) | 0.012 (2.43%) | 0.073 (19.77%) | 0.053 (18.40%) | 0.052 (5.37%) | 0.090 (37.70%) | 0.001 (1.23%) | 0.068 (16.53%) | 0.038 (6.87%) | 0.046 (5.47%) | 0.064 (18.60%) |
| WhatsApp | 0.012 (0%) | 0.028 (0.37%) | 0.004 (0%) | -0.004 (0.13%) | 0.019 (0%) | 0.011 (0.03%) | 0.024 (0.17%) | -0.010 (0%) | 0.065 (10.60%) | 0.006 (0%) | 0.020 (0.07%) |
| Email | -0.003 (0.03%) | 0.009 (1.67%) | -0.011 (0.27%) | -0.090 (42.27%) | 0.027 (0.80%) | 0.006 (0.37%) | -0.004 (1.23%) | -0.012 (0.40%) | 0.016 (2.30%) | 0.003 (0.23%) | -0.005 (1.00%) |
| Facebook | 0.006 (0%) | 0.019 (0.10%) | -0.001 (0%) | 0.043 (4.57%) | -0.005 (0%) | 0.051 (1.13%) | -0.064 (8.27%) | 0.018 (0.03%) | -0.020 (0.13%) | 0.006 (0%) | 0.009 (0.03%) |
| Instagram | 0.013 (0%) | -0.017 (0.03%) | 0.030 (0.03%) | 0.064 (13.30%) | -0.001 (0%) | 0.029 (0%) | -0.009 (0%) | 0.023 (0%) | -0.014 (0.03%) | 0.002 (0%) | 0.032 (0.23%) |
| Snapchat | 0.000 (0%) | -0.016 (0.07%) | 0.013 (0%) | 0.020 (0.53%) | -0.001 (0%) | -0.012 (0%) | 0.014 (0.07%) | -0.013 (0%) | 0.020 (0.07%) | -0.012 (0%) | 0.018 (0.07%) |
| X (Twitter) | 0.011 (0%) | 0.005 (0.03%) | 0.014 (0%) | -0.042 (4.00%) | 0.031 (0.07%) | 0.000 (0%) | 0.035 (1.17%) | 0.019 (0%) | 0.004 (0.07%) | -0.007 (0%) | 0.040 (1.10%) |
| TikTok | -0.003 (0%) | 0.024 (0.43%) | -0.017 (0%) | 0.004 (0.50%) | -0.003 (0%) | -0.025 (0.07%) | 0.024 (0.20%) | -0.031 (0.23%) | 0.039 (1.30%) | -0.011 (0%) | 0.014 (0.20%) |
| YouTube | -0.014 (0%) | -0.005 (0.60%) | -0.016 (0.17%) | -0.069 (24.60%) | 0.007 (0.03%) | -0.010 (0.20%) | -0.017 (1.10%) | -0.027 (0.67%) | 0.012 (0.70%) | -0.004 (0.03%) | -0.028 (2.87%) |

We standardized life satisfaction and the frequency of social technology use prior to model estimation using pooled standard deviations across participants and waves for each platform. RI-CLPM coefficients were therefore expressed in pooled standard deviation-scaled effect sizes, representing the expected change in life satisfaction (in pooled standard deviation units) associated with one pooled standard deviation from an individual's average level of social technology use frequency at the prior wave. Racial minority status includes all other races and ethnicities except non-Hispanic Whites.

**Table 3 |** Summary of between-person associations between self-reported frequency of social technology use and life satisfaction across platforms: Median posteriors of latent random-intercept correlations and associated posterior probability that the coefficients exceed the bounds of the region of practical equivalence.

| Platform | Omnibus | Gender | | Age | | Education | | Household Income | | Race and Ethnicity | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | Male | Female | 18–29 | 30 or older | Associate Degree or Lower | Bachelor Degree or Higher | Income < 75k | Income ≥ 75k | Non-Hispanic White | Racial or Ethnic Minority |
| FaceTime | 0.038 (1.00%) | 0.073 (27.00%) | 0.041 (3.00%) | 0.033 (9.87%) | 0.057 (8.67%) | 0.025 (1.93%) | 0.054 (12.37%) | 0.043 (5.57%) | 0.028 (3.93%) | 0.059 (9.77%) | 0.059 (17.10%) |
| Texting | 0.134 (88.50%) | 0.153 (87.97%) | 0.136 (83.40%) | 0.121 (63.93%) | 0.137 (86.33%) | 0.122 (72.47%) | 0.059 (17.10%) | 0.083 (31.70%) | 0.040 (9.27%) | 0.130 (79.77%) | 0.099 (48.87%) |
| WhatsApp | 0.091 (37.10%) | 0.105 (54.03%) | 0.086 (32.93%) | 0.126 (71.50%) | 0.081 (26.33%) | 0.038 (3.63%) | 0.070 (20.00%) | 0.073 (19.97%) | 0.060 (15.70%) | 0.095 (43.77%) | 0.150 (89.17%) |
| Email | 0.129 (86.10%) | 0.116 (63.73%) | 0.140 (85.33%) | 0.190 (95.43%) | 0.094 (42.13%) | 0.053 (9.90%) | 0.025 (5.87%) | 0.041 (6.43%) | -0.076 (30.47%) | 0.130 (80.63%) | 0.085 (38.60%) |
| Facebook | 0.065 (7.97%) | 0.024 (3.47%) | 0.106 (57.50%) | 0.061 (20.87%) | 0.056 (6.63%) | 0.077 (24.97%) | 0.064 (17.07%) | 0.077 (24.90%) | 0.072 (22.73%) | 0.024 (0.63%) | 0.099 (48.97%) |
| Instagram | 0.034 (0.47%) | 0.000 (2.00%) | 0.065 (13.77%) | 0.009 (4.40%) | 0.069 (14.03%) | 0.023 (1.37%) | -0.055 (9.43%) | 0.009 (0.47%) | -0.035 (4.23%) | 0.045 (3.50%) | 0.056 (14.77%) |
| Snapchat | 0.007 (0%) | 0.060 (15.93%) | -0.010 (0.23%) | 0.042 (10.87%) | 0.032 (1.30%) | 0.032 (2.23%) | 0.005 (1.13%) | 0.039 (3.67%) | 0.011 (1.17%) | -0.015 (0.27%) | 0.046 (9.40%) |
| X (Twitter) | -0.005 (0%) | -0.006 (1.37%) | -0.020 (0.77%) | 0.026 (6.87%) | -0.009 (0.23%) | -0.010 (0.87%) | -0.073 (23.67%) | -0.010 (0.40%) | -0.045 (8.00%) | 0.012 (0.13%) | -0.027 (5.23%) |
| TikTok | -0.106 (59.43%) | -0.060 (16.53%) | -0.117 (71.13%) | -0.112 (59.27%) | -0.082 (27.87%) | -0.068 (16.20%) | -0.097 (46.50%) | -0.055 (8.53%) | -0.075 (26.40%) | -0.105 (55.37%) | -0.083 (34.70%) |
| YouTube | -0.107 (59.53%) | -0.072 (26.57%) | -0.142 (88.83%) | -0.073 (30.93%) | -0.110 (63.03%) | -0.128 (77.03%) | -0.127 (75.93%) | -0.106 (56.20%) | -0.150 (89.23%) | -0.127 (79.47%) | -0.024 (5.50%) |

## Figure Legends

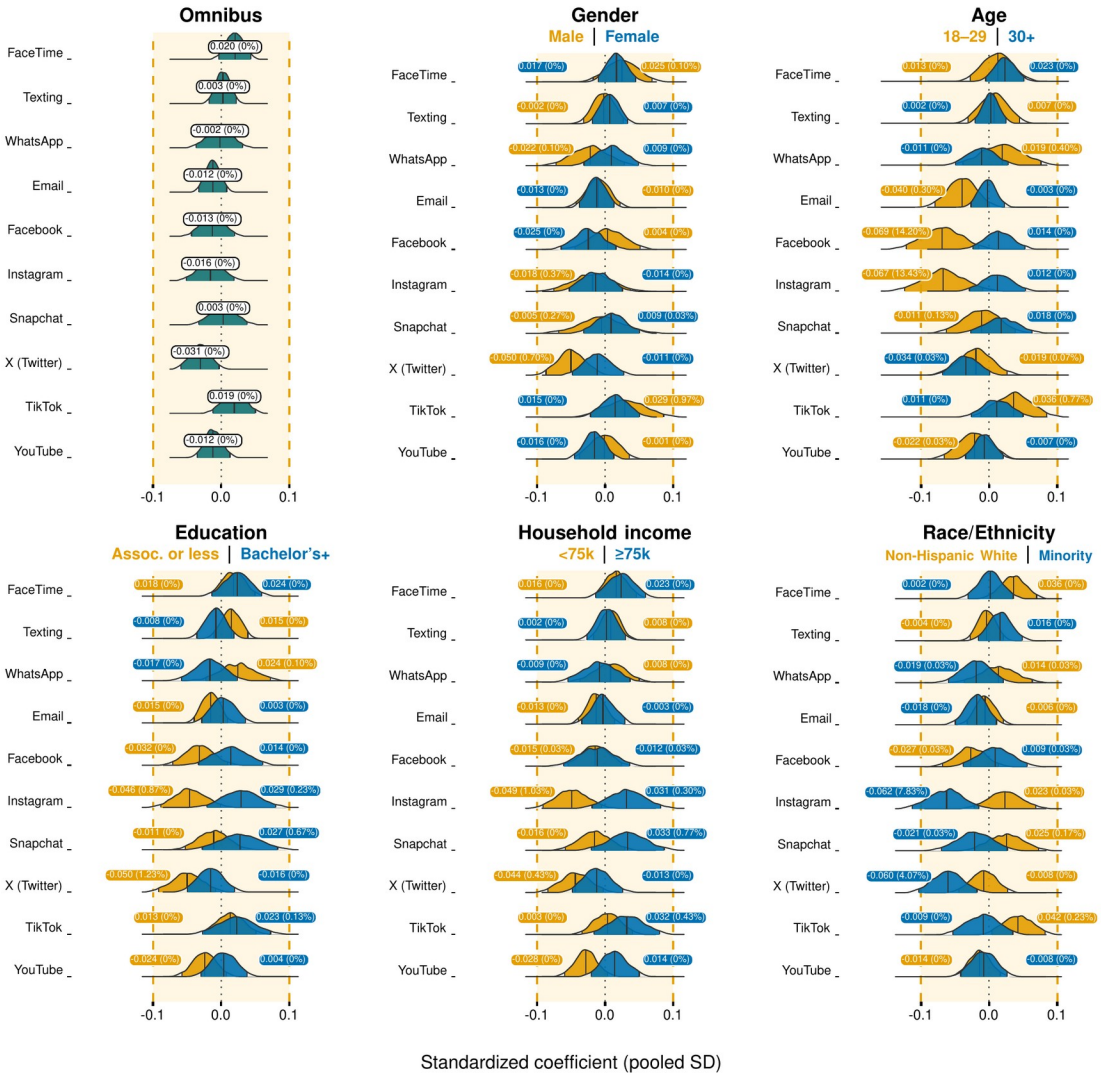


**Fig. 1** | Posterior distributions of cross-lagged associations between earlier self-reported frequency of social technology use and subsequent life satisfaction. Ridgelines represent posterior densities from Bayesian random-intercept cross-lagged panel models (RI-CLPMs) estimated separately for each platform. Shaded areas under the posterior distribution curves indicate the 95% credible interval. The figure displays these associations for the full sample alongside associations broken down by demographic subgroups. Life satisfaction and the frequency of social technology were scaled using pooled standard deviations across participants and waves to yield the standardized regression coefficients ($\beta$). The shaded vertical bands mark the region of practical equivalence, ROPE (-0.10 to +0.10), representing associations considered negligible. To improve readability, posterior density curves are plotted from the 0.5th to 99.5th percentiles of the posterior draws. Numeric labels report the posterior median of the standardized coefficient, with the percentage of the posterior distribution falling outside the ROPE bounds shown in parentheses. Bayesian model comparisons using log Bayes factors favored no group difference in the cross-lagged association from social technology use to life satisfaction across all platforms. Default blavaan weakly informative priors were used as follows: normal for intercepts, factor loadings, and regression parameters; gamma priors on standard deviations; beta priors on correlations, and Wishart priors for covariance-matrix parameters, where applicable.

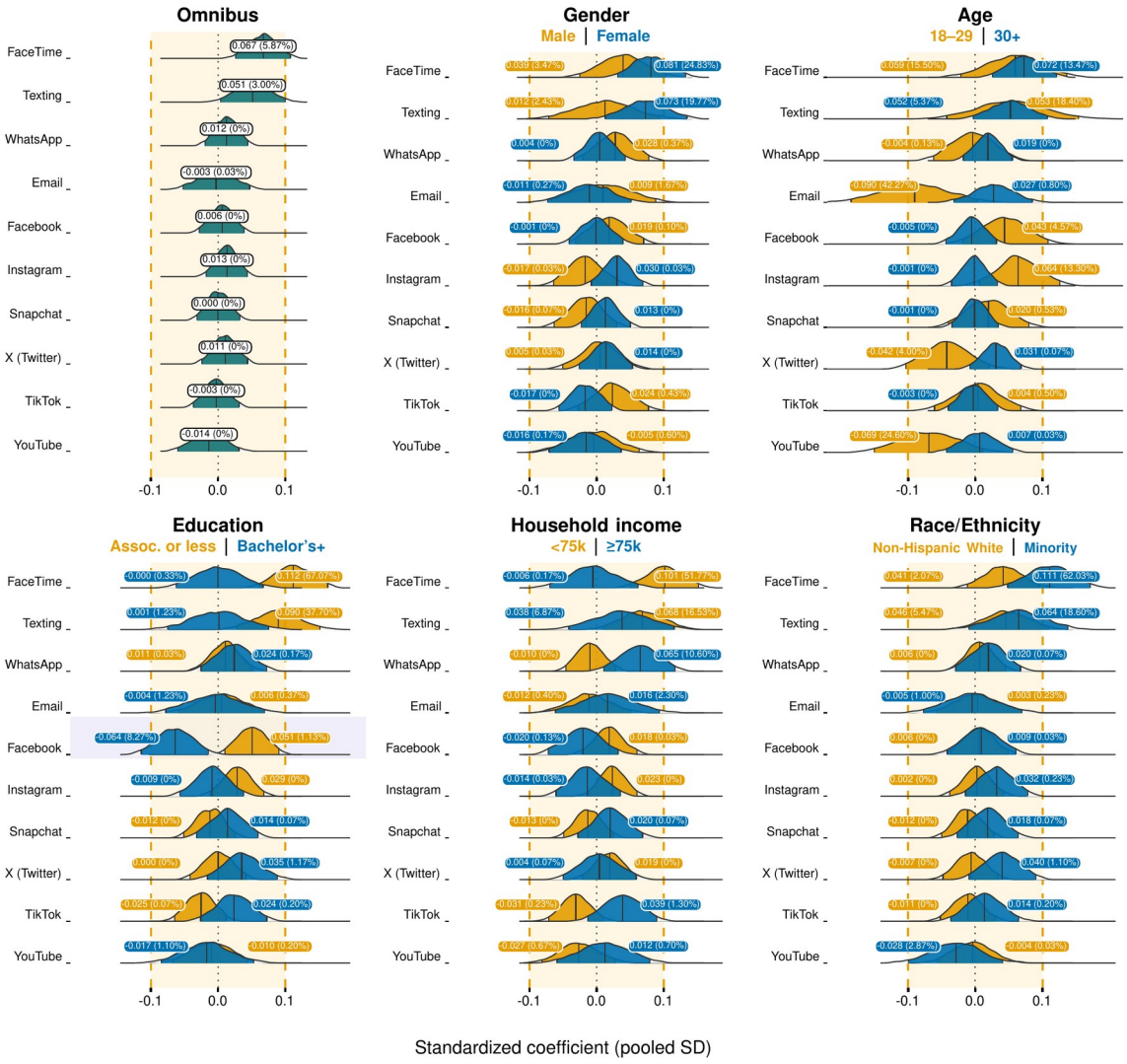


**Fig. 2** | Posterior distributions of cross-lagged associations between earlier life satisfaction and subsequent self-reported frequency of social technology use. Ridgelines represent posterior densities from Bayesian random-intercept cross-lagged panel models (RI-CLPMs) estimated separately for each platform. Shaded areas under the posterior distribution curves indicate the 95% credible interval. The figure displays these associations for the full sample alongside associations broken down by demographic subgroups. Life satisfaction and the frequency of social technology were scaled using pooled standard deviations across participants and waves to yield the standardized regression coefficients ($\beta$). The shaded vertical bands mark the region of practical equivalence, ROPE (-0.10 to +0.10), representing associations considered negligible. To improve readability, posterior density curves are plotted from the 0.5th to 99.5th percentiles of the posterior draws. Numeric labels report the posterior median of the standardized coefficient, with the percentage of the posterior distribution falling outside the ROPE bounds shown in parentheses. Lavender row shading indicates inconclusive Bayesian model comparisons using log Bayes factors; unshaded rows indicate evidence favoring no group difference in the cross-lagged association from life satisfaction to social technology use. Default blavaan weakly informative priors were used as follows: normal for intercepts, factor loadings, and regression parameters; gamma priors on standard deviations; beta priors on correlations, and Wishart priors for covariance-matrix parameters, where applicable.

**Fig. 3 | Posterior distributions of between-person associations between life satisfaction and self-reported social technology use.** Ridgelines represent posterior densities of latent random-intercept correlations (*r*) derived from Bayesian random-intercept cross-lagged panel models (RI-CLPMs) estimated separately for each platform. Shaded areas under the posterior distribution curves indicate the 95% credible interval. The figure displays these associations for the full sample alongside associations broken down by demographic subgroups. Between-person latent covariances were converted into latent correlations, placing the estimates on an interpretable standardized effect-size metric. The shaded vertical bands mark the region of practical equivalence, ROPE (-0.10 to +0.10), representing associations considered negligible. To improve readability, posterior density curves are plotted from the 0.5th to 99.5th percentiles of the posterior draws. Numeric labels report the posterior median of the latent correlation, with the percentage of the posterior distribution falling outside the ROPE bounds shown in parentheses. Lavender row shading indicates inconclusive Bayesian model comparisons using log Bayes factors; unshaded rows indicate evidence favoring no group difference in the between-person association. Default blavaan weakly informative priors were used as follows: normal for intercepts, factor loadings, and regression parameters; gamma priors on standard deviations; beta priors on correlations, and Wishart priors for covariance-matrix parameters, where applicable.

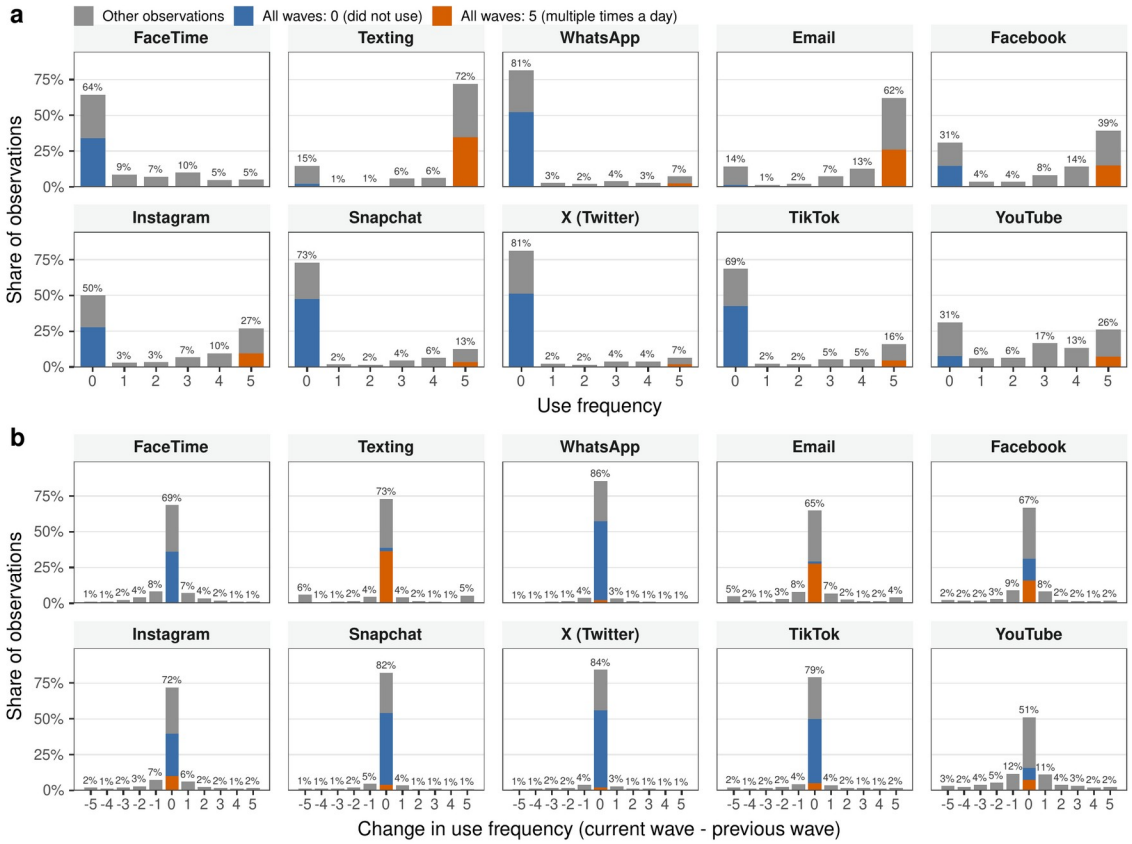


**Fig. 4.** Frequency distributions of social technology use for raw variables (Panel a) and change between adjacent waves (Panel b) in the full sample. Distributions include the full sample, including nonusers. In Panel b, the blue portion of the no-change bar indicates the percentage of non-users of a given platform; that is, those who reported not using a given platform at all in any of the five waves. The orange portion of the no-change bar indicates the percentage of people who reported using a given platform the maximum on the scale ("5 – multiple times a day") across all five waves. The grey portion shows all other cases of no change between adjacent waves.

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