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Well-Being Associations with Daily Activities on Instagram and Moderation by Beliefs About Social Media Addiction

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Abstract

Debates about the effects of social media on well-being are ongoing, fueled in part by mixed and weak associations found in published research. Researchers have relied heavily on measuring time spent on social media (i.e., “screen time”) and self-reports that fail to capture the detail of young people’s day-to-day activities and interactions on branded apps like Instagram. Further, inconsistent findings may be due in part to beliefs young people hold about social media’s role in harming well-being. This study proposes to obtain direct and objective application data from Instagram to test whether specific activities and interactions (posting, liking, commenting, browsing) are related to daily well-being. We will also test whether these associations are shaped by young people’s pre-existing beliefs about the addictive properties of social media. Young adults aged 18 to 25 who use Instagram daily will be recruited online. Participants will complete 15 daily surveys distributed semi-randomly during a 30-day period during which time Instagram application data will be gathered passively, using Meta’s Instagram account authorization flow enabled as part of Meta and the Center for Open Science’s Instagram Data Access Pilot (Meta

Platforms, Inc., 2025). Multilevel linear models will test confirmatory and exploratory research questions. Results and discussion will follow with the Stage 2 report.

Evaluations

SERVICE	SUMMARY	VERSION	DATE	EVALUATED
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Registration

This Research Plan submission serves as the study registration

Materials

Materials archived with this registration: *https://osf.io/tpe9h*

Data

Data collection has not yet begun.

Code

Sample code is archived with this registration: *https://osf.io/tpe9h*. Final study code is not yet created.

Paper

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Rapid uptake of smartphones since the introduction of the iPhone in 2007 transformed how people of all ages interact with technology in daily life. From a developmental perspective, social media platforms are rich environments in which young people are exposed to opportunities to flourish and to threats against their well-being. However, public reaction to young people's frequent smartphone use has primarily focused on the potential for harm (George & Odgers, 2015; Orben, 2020a). Countering this negative social discourse, reviews and meta-analyses show weak aggregate associations between social media use and well-being in correlational studies (Meier & Reinecke, 2021; Orben, 2020b) and in randomized experiments (Ferguson, 2024; Lemahieu et al., 2024). Lay beliefs that social media causes harm have nonetheless proved resilient to evidence-based counter-claims. The disconnect between evidence and public sentiment about social media is concerning given that governments and school boards have forwarded social media bans (Fardouly, 2025) and lawsuits against platforms such as Instagram and TikTok for allegedly "rewiring" children's brains (Rushowy, 2024) and causing platform addiction (Pierson, 2024).

Evidence of social media harms is weak, even in vulnerable clinical samples (Fassi et al., 2024), in part because of the historical focus on how much time young people spend on social media (i.e., "screen time") without clearly measuring what they are doing when they're online (Kaye et al., 2020). Recognizing the limitations of measuring screen time, researchers are increasingly taking a communication-centered approach (Meier & Reinecke, 2021), and measuring discrete activities and interactions taking place on branded applications (apps) such as Instagram. Activities such as commenting, liking, posting, browsing, and reviewing feedback are linked to well-being (e.g., Cary et al., 2024; Valkenburg et al., 2022) through mechanisms such as social comparison, identity development, and reward sensitivity (Orben et al., 2024). However, this emerging line of research is also limited by a reliance on self-report measures. Direct and objectively recorded data are needed to accurately measure in-app activities and their day-to-day associations with well-being. Young people's pre-existing beliefs about the addictive potential of social media may also color their perceptions of their own well-being in relation to some interactions (e.g., received likes or comments) more than others (e.g., posts created). In this study we link Instagram records of young adults' activities and interactions over 30 days (i.e., posts, likes, comments, and content views) to self-reported well-being, testing whether associations differ between people with varying beliefs about social media addiction.

Social media and well-being from a developmental perspective

Well-being is a psychological construct characterized by the combination of feeling good (e.g., positive emotions, life satisfaction) and functioning well (e.g., purpose in life; Huppert, 2009; Keyes, 2003). Well-being is often studied independent of ill-being and mental illness, where the focus is on the presence of problems such as depression, anxiety, or anger (e.g., Ryff et al., 2006). Scholarly interest in young people's well-being stems from *positive youth development* (Lerner et al., 2005), a strengths-based conceptual framework focused on promoting healthy development rather than merely avoiding problems. Theoretical models of human development contend that people grow and change as a function of reciprocal influences between the individual and their contexts and environmental experiences (e.g., Gottlieb & Halpern, 2002; Lerner & Kauffman, 1985). The ways that young people act and are acted upon in different environments—at home, at school, with friends, online—influence the likelihood that outcomes will be adaptive or maladaptive (Cicchetti & Rogosch, 1996; Gottlieb, 1991).

Most research on social media and youth well-being has focused conceptually on the part of the developmental system in which the branded app (e.g., Instagram, TikTok) acts on the individual to cause harm. A central argument against social media is that apps are intentionally built to keep users engaged on the platform through an infrastructure that delivers social feedback rewards (Maza et al., 2023). Concerns are most acute for young people in their early teens through mid-twenties, because most mental illness has its onset during this period (Beck et al., 2024; Kessler et al., 2007), risk-taking is at its peak (Duell et al., 2018), and the capacity for impulse control is still maturing (Somerville et al., 2010). Likes, followers, and notifications prompting further app engagement offer the kind of feedback that is acutely impactful in adolescence and the transition to adulthood, periods of heightened sensitivity to social evaluation (Flannery et al., 2024; Somerville, 2013). Computational models of thousands of social media posts show that online behavior follows patterns consistent with principles of reward learning (Lindstrom et al., 2021; Turner et al., 2024). Neuroimaging studies show that negative impacts in areas of the brain involved in reward processing are linked to heavy and potentially problematic levels of social media use (Flannery et al., 2024; Achterberg et al., 2022). Social media is clearly rewarding, but young people also have agency and behave online in ways that influence their own experiences, and research shows that online problems tend to reflect offline vulnerabilities (Odgers & Jensen, 2020).

Evidence linking social media to well-being

Evidence across systematic reviews and meta-analyses is mixed as to whether (and when) time spent on social media use is positively, negatively, or neutrally associated with well-being (Hancock et al., 2022; Odgers & Jensen, 2020; Orben, 2020b; Valkenburg, 2022). In addition to the limitations of relying on screen time to measure social media use (Kaye et al., 2020), studies have tended to

use “well-being” as a catchall term for an assortment of well- and ill-being constructs, including mental illness. Meta-analytic inconsistency may be due to the imprecision that comes from aggregating over many distinct measures (Valkenburg, 2022). If it is the case that social media use causes harm, even observational studies should find that people using social media heavily are doing worse over time. However, longitudinal studies testing this question show social media screen time to be unrelated to later ill-being (Bradley & Howard, 2023; Coyne et al., 2020; Heffer et al., 2019; Sewall et al., 2022; Tang et al., 2021). One large study did find that heavier social media use predicted later reductions in life satisfaction, but only at specific ages (Orben et al., 2022). In controlled experiments, some studies that randomly assigned participants to reduce or abstain from using social media (vs. continue as usual) showed improvements in well-being over time (e.g., Brailovskaia et al., 2020; Hunt et al., 2018; Tromholt, 2016), but others show no effects (e.g., Przybylski et al., 2021). In meta-analyses of such experiments, aggregate effects are weak or not distinguishable from zero (Ferguson, 2024; Lemahieu et al., 2024).

The scholarly consensus is that researchers need to move away from screen time and focus on measuring young people’s activities on social media. Indeed, using social media comprises many distinct behaviors: scrolling through posts, pausing to read or view, liking and sharing posts, commenting, creating posts, reviewing notifications, and responding to others’ comments. The *multidimensional model of social media use* (Yang et al., 2021) categorizes these behaviors into directed, interactive activities (liking, commenting, direct messaging), broadcasting activities (posting or sharing content to a broad audience), and browsing (scrolling through feeds of one’s own or others’ content). Interaction and broadcasting may be further broken down by specifying whether communiqués are sent versus received, and the relationships between the parties involved in communications (see Meier & Reinecke’s communication-centered approach*;* 2021). Arguing for a new social media research agenda, Meier and colleagues (2024) described testing associations between specific activities on social media and well- or ill-being as a priority route for progressing knowledge in this area.

Activities on social media correlate with well-being

Diary and experience sampling studies offer correlational support for the idea that activities on social media might trigger reactions to the presence or absence of social rewards (e.g., Colasante et al., 2024; Karsay et al., 2023). The multidimensional model of social media use attributes some improvements in well-being to the perceived social support that accompanies interactive and broadcasting activities (receiving and sending likes and comments; posting content; Yang et al., 2021). In contrast, lack of sufficient perceived social support—too few likes and comments on one’s own posts—may have the opposite effect. For example, an experiment using a sham social media platform found that positive and negative affect were both modestly worse for young adults who were randomly assigned to receive a low number of likes after posting a selfie (Rosenthal-von der Putten et al., 2019). In another experiment, adolescents reported slightly worse mood after receiving few likes for their posts (and better mood after receiving many; Pinho et al., 2024). An

experience sampling study of 18-year-olds found that broadcasting—posting original content on social media—predicted increased positive affect within the same hour, though liking and commenting on others' posts predicted less positive affect (G. Ferguson et al., 2024). Unlike broadcasting and interactive activities, effects linked to browsing are more mixed, with several studies finding negative links to well-being (Karsay et al., 2023; Roberts & David, 2023; Verduyn et al., 2015) and others finding a mix of positive *and* negative feelings after browsing (e.g., amusement, envy, inspiration; Valkenburg et al., 2022; Weinstein, 2018). Asking people about their browsing experiences may be especially unreliable because viewing content on social media occurs continuously. Opportunities for harm (e.g., negative social comparisons) and for enrichment (e.g., gaining information or inspiration; see Yang et al., 2021) may occur nearly simultaneously.

Methodological challenges. Most existing studies measure activities on social media by self-report, a task made difficult by the burden of recalling which (and how many) communiqués took place over a discrete span of time, and the near impossibility of accounting for content browsed throughout the day. People are known to have poor accuracy in recalling time spent on social media (Parry et al., 2021), and some research teams have instead attempted to link well-being to the logged records of activity downloaded by participants from their social media apps. Obtaining and aggregating donated data downloads from participants' own social media accounts poses numerous technical difficulties (large file sizes; complex data processing; file format changes by the branded app; van Driel et al., 2022), but results from the few studies that have attempted it so far are consistent with findings from studies relying on self-report. One cross-sectional study that linked extracted Facebook data to self-reports found that receiving likes was associated (weakly) with more happiness and self-esteem, but posting new content was unrelated to either (Marengo et al., 2021). Another study that extracted public Twitter data found that in people with (vs. without) a self-reported depression diagnosis, likes were more strongly reinforcing (i.e., led to more next-day posting; Mirea et al., 2024). One study recorded participants' phone screens in-person while they used Instagram for 10 minutes. Receiving more likes, comments, and direct messages during that time predicted greater post-test life satisfaction ratings, as did browsing other people's content. Posting content and sending messages, however, were unrelated to life satisfaction (Cary et al., 2024).

Advantages of provider-supplied application data. Downloaded data and screen recordings are incomplete records of young people's activities on a social media branded app. In the present study, we will access the comprehensive daily records of participants' activities on Instagram, including instances of posts, likes and comments (sent and received), and content views. Linking these data to survey reports of well-being presents an opportunity to test well-being associations with greater precision. For example, evidence to date suggests that people feel worse when they receive fewer likes and comments on their posted content and better when they receive more (e.g., Pinho et al., 2024; Rosenthal-von der Putten et al., 2019). We presume that this effect is a function of content creation rate—i.e., receiving few likes after sharing several posts of original content likely feels worse than receiving few likes on a day a person hasn't posted at all. With application

data we can explore this previously untested question. Similarly, broadcasting and interactive messages (outgoing posts, likes, comments) are sometimes positively related to well-being, but perhaps these more active behaviors happen less often when young people are browsing social media to manage boredom. Detailed daily records from Instagram allow us to condition on content views, an activity not readily quantified by self-report. Mixed evidence about well-being links to browsing may also be clarified by accounting for whether a person's social media use on a given day has also included active or interactive behaviors. In this study we test these and other hypotheses by combining well-being diary data with official records of participants' Instagram activities over a one-month period.

Moderating role of pre-existing beliefs about social media addiction

The modest-sized effects and mixed evidence linking social media screen time to well-being are inconsistent with the alarmist tone of narratives in the popular press, some of which likely influence people's beliefs about social media harms (Lanette et al., 2018; Størup & Lieberoth, 2023). The studies that more consistently find links to poorer well- and ill-being including depression, anxiety, stress, loneliness, and lower life satisfaction are the ones focused on *problematic* or addiction-like social media use (e.g., preoccupation with social media when not in use; relying on social media as an affect regulation tool; Andreassen et al., 2016; Boer et al., 2020; Brailovskaia & Margraf, 2020; Chao et al., 2023; Marengo et al., 2022). Results of these studies may reinforce lay beliefs that social media apps are like addictive substances whose use we need to curtail. Experiments that show improvements in well-being for people assigned to reduce their social media use (e.g., Hunt et al., 2018) may serve to confirm these beliefs ("digital detox", see Radtke et al., 2022). If participants in such studies, however, arrive with the expectation (or even hope) that reducing their social media use will improve their well-being, more positive ratings over time may be due to these pre-existing beliefs or knowledge of study hypotheses rather than actual improvements (Ferguson, 2024). These *demand* effects are not new to behavioral research and represent a major limitation for behavioral interventions where it is impossible to mask condition allocation (e.g., physical activity interventions; Poitras et al., 2016). Demonstrating their influence on the interpretation of results, demand effects are a key factor in assessing risk of bias for experiments included in systematic reviews (Higgins et al., 2024).

Repeated exposure to messages about social media's harmful effects contribute to a socially constructed view of the realities of using social media (Wolfers, 2024). Over time, norms emerge that define perceptions of the prevalence of specific behaviors and beliefs, as well as perceptions of their social approval (Chung & Rimal, 2016). What people individually *believe*—their attitudes or convictions about what is true—are shaped by norms and do not necessarily reflect objective reality (Barrett, 2013). Teens in one qualitative interview study readily attested to the dangers and addictive potential of social media but could not, when prompted, come up with any concrete examples of harms they personally experienced or witnessed (Lanette et al., 2018). In another study, 91% of adolescents agreed that their digital technology caused one or more impairments in

their lives despite extremely weak links in the same data between technology use and measures of well-being (George et al., 2020).

We suspect that pre-existing beliefs about social media's potential for harm may alter the day-to-day associations between young adults' interactive, broadcasting, and browsing activities on their self-reported well-being. A multi-country study found support for this hypothesis in cross-sectional research with adults: self-reported time spent online was most strongly associated with poorer well-being in people who believe that Facebook is bad for them (Ernala et al., 2022). Adults in another study who endorsed more positive mindsets about their own social media use reported less depression and greater life satisfaction (Lee & Hancock, 2024). In the present study, we measure within-person variation in social media activities—for example, receiving more or fewer likes one day than they receive on a typical day—and test whether associations with same-day well-being differ between young adults who more or less strongly endorse pre-existing beliefs that social media is harmful.

Current study

In the present study, we link Instagram-provided activity data with participants' ratings of their own daily *subjective well-being* (positive and negative affect; life satisfaction). Subjective well-being is selected for its history of reliable and valid measurement, construct maturity, regular appearance in studies linking well-being to social media use, and for its evidence of substantial day-to-day variation (Busseri & Newman, 2024). Taking a developmental perspective, we view Instagram as an environment in which young people encounter opportunities every day for enhanced or diminished well-being. If social media keeps young people engaged through social rewards, well-being outcomes should depend on combinations of content consumed (likes and comments received; feeds browsed) and produced (posts created; likes and comments sent). For example, well-being may be harmed on days when numbers of received likes or comments are low relative to numbers of posts, likes, or comments sent that day. Similarly, well-being may be improved on days of heavy posting, liking, or commenting, but only if numbers of received likes or comments are also high. What counts as "high" or "low" will differ for each person relative to their own averages, and we accordingly pose several confirmatory and exploratory within-person hypotheses.

Confirmatory Hypotheses. Consistent with meta-analytic evidence from prior research, our first hypothesis attempts to confirm that daily time on Instagram will be unrelated to well-being after accounting for effects of messages received and created:

H1: Spending more time on Instagram than usual will be unrelated to daily well-being after accounting for likes, comments, and posts sent and received.

Instead, we hypothesize worse well-being when received messages are low, and better well-being when sent messages are high:

H2a: Young adults will report worse well-being on days when a person's numbers of likes and comments received are lower than usual for them.

H2b: Young adults will report better well-being on days when a person's numbers of likes and comments sent, and posts created, are higher than usual for them.

We anticipate that the associations described in H2 will differ when messages received are in versus out of proportion to messages sent:

H3: Young adults' reported well-being will be worst on days when numbers of likes and comments received are fewer than usual and sent posts, likes, and comments are at or above usual levels (i.e., out of proportion). In contrast, reported well-being will be best on days when numbers of sent posts, likes, and comments are at or above usual levels and likes and comments received are also at or above usual levels (i.e., in proportion).

To address the possibility that message effects are explained by young adults' pre-existing beliefs about the harmful addictive potential of social media, we hypothesize that associations between received messages and well-being (see H2a), if negative, will be stronger for people with stronger beliefs about social media addiction. Associations between sent messages and well-being (see H2b) will not be moderated by beliefs.:

H4a: The association described in H2a will be stronger for young adults who more strongly endorse beliefs that social media is addictive.

H4b: The association described in H2b will not differ as a function of young adults'

beliefs about social media addiction.

Exploratory Tests. Prior research has been mixed as to whether browsing is positively or negatively related to well-being. We test the effects of browsing, including whether its effect is moderated by beliefs, but pose no directional hypotheses:

E1: Test whether young adults' daily well-being is related to browsing more content (i.e., a higher number of views) than usual on a given day.

E2: Test whether the association described in E1 differs for young adults who more strongly endorse beliefs that social media is addictive.

Causal considerations and alternative models. The proposed study uses an observational design to provide indirect evidence for or against the claim that social media use causes harm. In some

cases, a causal pathway is more clearly unidirectional (e.g., receiving fewer likes may cause worse mood, but mood cannot cause receipt of fewer likes), but in others, reverse causation is equally plausible (e.g., better mood may cause a person to post, comment, or like others' content, just as the content itself might cause a change in mood). Although the findings of the present study alone cannot establish causation, we address some limitations to causal inference with additional exploratory tests of whether subjective well-being predicts the next day's likes, comments, posts, and views:

E3: Test whether daily well-being one day is related to posts, likes, and comments sent, and views recorded, the next day.

E4: Test whether the associations described in E3 differ for young adults who more strongly endorse beliefs that social media is addictive.

Method

Participants and Procedure

This study will recruit *n* = 300 young adult Instagram users to complete an intake survey followed by 15 daily surveys distributed semi-randomly over a 30-day period. To ensure a feasible study design within the time limits for data collection set by Meta Platforms, Inc., we chose to limit our recruitment to participants not requiring parental consent. From the Instagram account for our research lab, we will recruit participants using a combination of gift card giveaways and paid advertisements to direct traffic to our account and promote the study [masked author citation]. People who engage with our Instagram ads will be directed to complete an eligibility screening survey. Eligibility criteria include being 18 to 25 years old, living in Canada, and reporting regular Instagram use (daily use of the platform; posting content on the platform at least 3 times per week). Results from a study of 18-year-olds found that posting was rare relative to liking and commenting (G. Ferguson et al., 2024), and tests involving content posting frequency rely on having most or all participants post content on at least some of the days sampled in the present study.

Recruitment and eligibility verification. Eligible respondents will be invited to provide their name and email address for follow-up communication. We will use gender reported on the eligibility screening survey, if necessary, to selectively follow up with a higher proportion of men and non-binary respondents to avoid over-recruiting women. From prior experience, we anticipate many illegitimate respondents, including bots and impostors, and we screen out many of these cases by examining email addresses and responses to trap questions designed to be endorsed by people trying to gain access to a study. Respondents who pass initial quality checks will be invited to schedule a 5-minute eligibility verification meeting with a research assistant on Zoom where they will show government-issued identification on camera. This procedure was successful in a previous

study of young adults that recruited *n* = 306 out of 322 people (95%) who attended a Zoom-based eligibility verification meeting [masked author citation].

Intake survey. Respondents who pass eligibility verification will be invited by email to complete an online survey in Qualtrics software assessing demographic information, social media use history, and a measure of beliefs about social media harm. Participants who complete this survey will receive a $10 gift card. Further data quality criteria and details of our attention check and bot management strategy are available on our OSF project page: *https://osf.io/3bd6t/?view_only = 843fea80d48044dda3941395fb16704b*). Participants who express an interest in continuing to the daily assessment phase of the study (and whose intake data is complete and passes our quality checks) will be invited to register for this phase by completing a new consent form, a data sharing consent provided by Meta Platforms, Inc., and a cell phone number for receiving survey text message prompts.

Daily assessment phase and Instagram data gathering. On the first day of this phase, participants will be redirected within their Instagram app to authorize Meta to share selected data with our research team for 30 days. When this step is complete, participants will begin receiving text messages from our team, powered by *Inclivio.com* messaging software. For 15 days over the 30-day authorization period, text message prompts will arrive in the morning with a link to a brief Qualtrics survey querying the previous day's activities and subjective well-being. Up to two reminders will be sent during the day, with surveys becoming unavailable after 2:00 PM local time. Each participant will receive survey prompts on a semi-random schedule with a gap of no more than 2 days between surveys. To minimize lost data, participants who miss a survey one day will be automatically prompted to complete a survey the next day on which they are not already scheduled for a survey. After the daily assessment phase, participants receive a gift card worth $1 for each survey they completed.

Measures

Assessed at study intake

Demographics. We ask participants to report their gender identity, age in years and months, race, whether they are currently a student, whether they live with their parents or other elder guardians/relatives, and their parents' highest level of education attained.

Pre-Existing Beliefs about Social Media Addiction. We were unable to find any multi-item instruments that measure beliefs about harmful effects of social media. Instead, we adapted a widely-used social media "addiction" scale (Bergen Social Media Addiction Scale [BSMAS], Andreassen et al., 2012; 2016) by altering the prompt and item stems to ask for evaluations of "young people in general" as opposed to self-reflections. The measure begins with the following preamble:

"Below you will find some descriptions that correspond to beliefs some people hold about young people's relationship to social media (Instagram, TikTok, Snapchat, and other platforms). How much do you personally agree or disagree with each of the following statements about young people (teens, young adults) in general?"

Sample items, replacing the pronoun "I/me" with "Young people", include "Social media makes young people feel an urge to use it more and more," and "Young people become restless or troubled if they are prevented from using social media." Response options on a 5-point scale range from *strongly disagree* to *strongly agree,* and we calculate the mean of the items.

The original BSMAS has been validated and re-used extensively (e.g., Brailovskaia & Margraf, 2024; Monacis et al., 2017; Watson et al., 2020). Altering the frame of reference to "young people in general" allowed us reduce confounding between beliefs about social media and appraisals of participants' own well- and ill-being. We trialed the revised measure on a screening survey of undergraduates from our departmental research participation pool (n = 544 after removing probable careless or inattentive respondents) and found it to have adequate internal consistency (=.71) and weak correlations with self-reported mental health (anxiety, r =.17; depression, r =.12) and life satisfaction (r =.02). In contrast, we found in a different survey of undergraduates measured four years earlier (n = 286) that the original BSMAS had stronger correlations with mental health (anxiety, r =.36; depression, r =.39). We report in a supplement (see *https://osf.io/3bd6t/?view_only = 843fea80d48044dda3941395fb16704b*) additional measures we will collect at study intake to assess the validity of this revised instrument for the present study.

Assessed in daily surveys

Subjective well-being. We use a single-item measure of life satisfaction shown in other daily diary research to load strongly with positive and negative affect at both between- and within-subject levels (Busseri & Newman, 2024). Participants are asked: "How satisfied were you with your life yesterday?". Response options on a 7-point scale range from *very dissatisfied* to *very satisfied.*

We measure positive and negative affect using the 10-item short form of the *Positive and Negative Affect Schedule* (Thompson, 2007), validated in multiple samples and across countries. Participants will see the prompt: "Yesterday, to what extent did you feel…" completed by five different positive states (*determined, attentive, alert, inspired, active)* and five negative states (*afraid, nervous, upset, ashamed, hostile),*, presented in random order. Response options are "Very slightly or not at all," "A little," "Moderately," "Quite a bit," and "Extremely". Each day, we will take the averages of participants' positive and negative item responses. Life satisfaction, mean positive affect, and mean negative affect will be separate daily outcomes in our analyses.

Direct Interaction. Each day we ask participants to estimate the extent of their direct interactions with other people, defined as face-to-face or on the phone (Kross et al., 2013), using the following

prompt: “Yesterday, how much did you interact with other people directly?” (0 = not at all; 10 = a lot).

Passively recorded on Instagram. Participants will authorize Meta to record and securely share with us data describing their account activity, messages sent and received, and time spent on Instagram:

Account and connections data: A report for each day will indicate each participant’s number of followers, number of accounts following, and number of close friends. For each, we will take the mean over all 30 days as a between-persons covariate. Time (in years) since account creation will be calculated as the difference between the first day of Instagram data collection and the account creation date supplied by Meta for each participant.

Activity data: A report for each day will provide a time stamp for each instance of a like, a comment, a view, and a post. According to the data definitions provided by Meta, a *like* is logged when a participant uses the heart button on another user’s content, or when a heart is received on the participant’s own content; a *comment* is logged when a participant creates a comment on another user’s account, or when a comment is created on the participant’s own account; a *view* is logged when non-advertising content posted by a user account is visible in full on the screen, and covers at least 50% of the screen for at least 250 milliseconds; and a *post* is logged when a participant creates a piece of content on their account. For likes and comments, the direction of the message (sent vs. received) is also reported. For each day, we will sum the number of likes (sent and received), comments (sent and received), views, and posts.

Time data: A report for each day will provide time stamps for each time Instagram is brought into the foreground on a participant’s phone (start time) and when it is backgrounded or closed (end time). The sum of all time windows on a given day (end minus start times) will capture total time spent on Instagram each day.

Sensitivity data: We requested measures from Meta to support sensitivity analyses in this study. Daily reports will also indicate whether participants’ accounts were set to “private”. For views and posts, time-stamped reports will indicate whether content was shared to a “close friends” list versus a larger audience.

Analysis Strategy

Our design has a two-level structure with daily observations (Level 1) nested within persons (Level 2). We will use multilevel linear models to estimate (via full information maximum likelihood) within-person message effects on subjective well-being and their interactions with time, beliefs in social media harm, and outgoing messages.

Planned analyses. Each hypothesis is tested with a minimum of three multilevel models (one for each measure of subjective well-being). For compound hypotheses referring interchangeably to the effects of likes and comments, separate models will be tested for each type of message. Table 1 summarizes the series of confirmatory analyses to be performed. For each model, one term (denoted "H" in Table 1) represents the test of the corresponding hypothesis. Within-person fixed effects are obtained by person-mean centering the raw daily scores before adding them to the model (see Curran & Bauer, 2011; Howard, 2015). Resulting effects represent how subjective well-being changes when a given person deviates above or below their own typical levels of the predictor (we described these deviations in the hypotheses as sending or receiving likes, comments, etc. "more than usual" and "less than usual", where "usual" is a person's own average over the days assessed). Exploratory tests are performed using the same strategy as described for confirmatory analyses. For models testing interaction hypotheses (H3, H4a, H4b, E2, E4), effects will be probed using the Johnson-Neyman interval method (Bauer & Curran, 2005) to describe the range of values of the moderator over which the tested associations exceed our inference and effect size criteria (described below).

Table 1

Summary of Proposed Effects to be Tested for Each Confirmatory Hypothesis

	H1	**H2a**		**H2b**			**H3**		**H4a**		**H4b**		
		L	*C*	*L*	*C*	*P*	*L*	*C*	*L*	*C*	*L*	*C*	*P*
Within-person													
Time on IG	**H**	X	X	X	X	X	X	X	X	X	X	X	X
Likes Rec'd	X	**H**					X		X				
Comments Rec'd	X		**H**					X		X			
Posts created	X			**H**			X_1	X_1			X		
Likes sent	X				**H**		X_2	X_2				X	

Comments sent	X					**H**	X_3	X_3					X
Direct interaction	X	X	X	X	X	X	X	X	X	X	X	X	X
Between-person													
Mean time on IG									X	X	X	X	X
Beliefs in harm									X	X	X	X	X
Mean direct interaction									X	X	X	X	X
Covariates									X	X	X	X	X
Interactions													
Likes R'd x Posts							**H_1**						
Likes R'd x Likes sent							**H_2**						
Likes R'd x													

Comm sent							**H_3**					
Comments R'd x Posts								**H_1**				
Comments R'd x Likes								**H_2**				
Comments R'd x Comms								**H_3**				
Likes R'd x Beliefs									**H**			
Comments R'd x Beliefs										**H**		
Likes sent x Beliefs											**H**	
Comms sent x Beliefs												**H**
Posts x Beliefs												**H**

Note. H1 = Hypothesis 1, etc. L, C, and P differentiate whether likes received, comments received, or posts appear in that version of the hypothesis test. Terms marked with an "X" or "H" are included in the model specified in that column. Those marked and bolded "H" are the terms corresponding to the test of the specified hypothesis. For Hypothesis 3 (H3), different versions of the models tested are marked with subscripts (i.e., H3-L and H3-C each have three model versions).

Complete study design tables consistent with the *Peer Community In* recommendations for Registered Reports (*https://rr.peercommunityin.org/help/guide_for_authors*) are available in Supplemental Tables 1 and 2 on our OSF page (*https://osf.io/3bd6t/?view_only = 843fea80d48044dda3941395fb16704b*).

In all models, we will attempt to estimate random effects for every within-person predictor and interaction term (see Barr et al., 2013). Models with many random effects are often intractable, so we introduce random effects into each model one at a time. Where loglikelihood deviance tests show that model fit is superior when a random effect is included versus excluded, the random effect will be retained if possible. Random effects that can be excluded without loss to model fit may be dropped if needed to ensure model convergence, modeling the associated predictor as a fixed effect only.

Power, effect size, and inference criteria. Time and financial restrictions constrain our sample size to *n* = 300 young adults enrolled in the daily assessment phase of our study. Because social media and well-being associations tend to be small, we focus on identifying whether effects are statistically equivalent to zero (Lakens et al., 2018). We simulated a linear mixed-effects model with 300 participants each contributing 15 repeated measures with 15% of observations missing (randomly deleted). Our simulated model included a within-subjects predictor—with a random slope having a variance one quarter the size of the random intercept—whose effect in the population is zero. Over 1,000 replications, 95% of obtained within-subjects effects ranged from –0.053 to 0.054 (in standardized units; code available on our OSF project page: *https://osf.io/3bd6t/?view_only = 843fea80d48044dda3941395fb16704b)*. We will consider any within-subjects effects (and Johnson-Neyman intervals for probed interactions) whose confidence intervals fall entirely within these bounds around zero to be statistically equivalent to zero and in support of the corresponding null hypothesis. Similarly, we will consider any within-subjects effects and Johnson-Neyman intervals whose confidence intervals fall entirely outside these bounds to support the tested hypothesis.

Effect sizes in the present study will be expressed in raw units. Whether small effects of social media use are objectively "small" is a matter of debate, so in the present study we benchmark sizes of message effects against the size of the daily effect of *direct interaction (*i.e., face-to-face or phone calls) in predicting subjective well-being. To be considered meaningful, a message effect must be at least half as large as the size of direct interaction on subjective well-being. Regardless of statistical significance, effects that fail to meet our criteria for exceeding zero and for size relative to the

direct interaction benchmark will not be taken as evidence supporting their corresponding hypothesis.

We rely primarily on effect sizes and boundaries around zero to make inferences about tested effects, but also report statistical significance for each hypothesis test (i.e., terms marked “H” in Table 1). Because each model is repeated across three measures of subjective well-being, we use a Type I error rate of 1.7% (.05/3 =.017) as a threshold for statistical significance and confidence intervals of 96.7% for evaluating whether effects and Johnson-Neyman intervals exceed our boundaries around zero.

Missing data. Participants with incomplete daily survey data will be retained in the analysis. Data provided by Instagram are complete. Days with no time stamps for a given message type will be recorded as zeroes. We will test for and report any differences between participants with more or less missing data by correlating a sum of each participant’s number of missing daily reports with study measures.

Covariate inclusion. Daily time on Instagram will be included in all models. We also include direct interaction as a covariate and benchmarking variable and a code for whether each day is a “weekday” (Monday-Friday) versus “weekend” day (Saturday-Sunday). Models that include beliefs in social media harm—a between-persons predictor—will also include average daily time on Instagram, demographic covariates (gender, age, race, student status, living arrangements, parents’ education), and Instagram background measures (time since account creation, number of followers, number following).

Model sensitivity. Robustness will be evaluated by repeating analyses with and without covariates included. We will also examine model-based diagnostics to identify influential outliers using Cook’s *D,* focusing on those observations with large values relative to other observations. For the subset of observations flagged as potentially influential in one or more models, analyses will be repeated after excluding this subset.

For sensitivity checks involving account settings, we use the following criteria: If at least 10% of observed data occur on days when participants’ accounts are set to “private,” we will split the data into public and private days, repeating analyses on each subset. For each of views and posts, if at least 10% are flagged as shared to a “close friends” list only, we will split the data into close friends vs. larger audience subsets and repeat the analyses on each subset. Where the 10% thresholds are not met, data from the smaller subset will be set aside and analyses repeated on the larger subset only.

Instagram Data Access Pilot Transparency Statement

For a fuller view of this project's process through the *Instagram Data Access Pilot*, supporting documents are openly available here: *https://osf.io/jmhv5/*

Supporting Information

Supporting Information files can be accessed here: *https://osf.io/tpe9h*

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Declarations

Ethics

This research is approved by the Carleton University Research Ethics Board - B (File #122934)

Competing Interests

The authors declare that no conflicts of interest exist.

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Author Contributions

Conceptualization: AH, KG, KT; Data Curation: AH, YG; Formal Analysis: AH; Funding Acquisition: AH, KG, KT; Investigation: AH, YG, KT; Methodology: AH, KG, KT; Project Administration: AH, YG; Supervision: AH; Validation: AH; Visualization: AH; Writing - Original Draft: AH, KG; Writing - Review and Editing: AH, YG, KG, KT