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Does Objectively Measured Social-Media or Smartphone Use Predict Depression, Anxiety, or Social Isolation Among Young Adults?

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Abstract
Despite a plethora of research, the link between digital-technology use and psychological distress among young adults remains inconclusive. Findings in this area are typically undermined by methodological limitations related to measurement, study design, and statistical analysis. Addressing these limitations, we examined the prospective, within-persons associations between three aspects of objectively measured digital-technology use (duration and frequency of smartphone use, duration of social-media use) and three aspects of psychological distress (depression, anxiety, and social isolation) among a sample of young adults (N = 384). Across 81 different model specifications, we found that most within-persons prospective effects between digital-technology use and psychological distress were statistically nonsignificant, and all were very small—even the largest effects were unlikely to register a meaningful impact on a person's psychological distress. In post hoc subgroup analyses, we found scant evidence for the claim that digital-technology use is more harmful for women and/or younger people.

Keywords
anxiety, depression, human-computer interaction, loneliness, longitudinal methods

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The past 15 years have witnessed coinciding trends among young people in the United States: rising rates of psychological distress, such as mental disorders and suicidality (Curtin, 2020; Twenge et al., 2019), and the proliferation of digital-technology use, such as smartphones and social media (Vogels, 2019). These concurrent trends instigated an abundance of popular and academic attention, which led to the emergence of the predominant narrative that digital technology is (at least partially) to blame (e.g., Chuck, 2017; Twenge, 2017; Twenge et al., 2018). Given that rates of psychological distress and digital-technology use have increased among young people during the COVID-19 pandemic (Czeisler et al., 2021; Samet, 2020), concerns regarding the putatively harmful effects of digital-technology use have intensified (e.g., Parks, 2021; Richtel, 2021; Shrier, 2021). However, because of key methodological and conceptual issues that are commonplace in the digital-health-effects literature (Griffioen et al., 2020; Kaye et al., 2020), the link between psychological distress and digital-technology use among young people remains inconclusive (Meier & Reinecke, 2020; Tang et al., 2021).

Numerous original studies and systematic reviews have coalesced around a common refrain of limitations that should be addressed by future research. These limitations include the methodological, such as the need for robust measurement, longitudinal designs, and within-persons analyses, and the conceptual, such as the need to investigate how different aspects of digital-technology

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use relate to different aspects of well-being (Dickson et al., 2019; Griffioen et al., 2020; Orben, 2020; Schemer et al., 2021; Tang et al., 2021).

Longitudinal analyses of the association between digital-technology use and psychological distress are critical for two primary reasons. First, by separating within-persons variance from between-persons variance, they make it possible to disentangle within-persons effects—that is, whether deviations in a person's digital-technology use are associated with deviations in their psychological distress—and between-persons effects—that is, whether people who, on average, have higher amounts of digital-technology use also have higher average levels of psychological distress. Second, in addition to variance decomposition, longitudinal analyses can provide insight into temporal dynamics between digital-technology use and psychological distress—that is, how the variables influence each other and themselves over time (i.e., time-lagged effects). Both the variance-separation and time-lagged-effects components are crucial because the question of whether digital-technology use affects psychological distress (or vice versa) is fundamentally a question about within-persons dynamics. Failing to properly account for stable between-persons differences in digital-technology use and psychological distress confounds the estimation of within-persons effects (Hamaker et al., 2015), and failing to incorporate time-lagged effects makes it impossible to identify any temporal or causal precedence between the variables at the within-persons level (Zyphur et al., 2020).

Although there have been many longitudinal studies on the association between digital-technology use and psychological distress (e.g., see Tang et al., 2021), only a handful of studies (Coyne et al., 2020; George et al., 2018; Houghton et al., 2018; Kross et al., 2013; Orben et al., 2019; Puukko et al., 2020; Schemer et al., 2021) have employed analyses that satisfy both of the objectives described above. However, findings are inconsistent. One study (Kross et al., 2013) found that digital-technology use predicted lower well-being but not vice versa, another (Puukko et al., 2020) found the opposite, two studies (Houghton et al., 2018; Orben et al., 2019) found evidence of a reciprocal effect, and three found no significant effects (Coyne et al., 2020; George et al., 2018; Schemer et al., 2021). Although the inconsistent results may be explained, at least in part, by differences in sample characteristics, frequency of data collection, and construct operationalizations, the fact that each of these studies relied on self-report measures to capture digital-technology use may undermine the validity of findings.

Strong evidence indicates that self-reports of the frequency or duration of digital-technology use are not valid measures of actual frequency or duration of digital-technology use. A recent meta-analysis found a modest correlation (r = .38, 95% confidence interval = [.33, .42]) between self-reported and device-logged digital-technology use, which indicates "that self-report measures of media use may not be a valid stand-in for more objective measures" (Parry et al., 2021, p. 7). Furthermore, several studies have found that the error involved with self-reported digital-technology use is systematically related to crucial participant characteristics, such as gender or age (Ernala et al., 2020; vanden Abeele et al., 2013), volume of digital-technology use (Boase & Ling, 2013; Deng et al., 2019; Ernala et al., 2020; Sewall et al., 2020), and level of mental well-being (Burnell et al., 2021; Sewall et al., 2020; Sewall & Parry, 2021). Thus, given the focus on explicating the association between digital-technology use and psychological distress, it is likely that the self-reported digital-technology use data in the extant longitudinal analyses described above—and the subsequent findings and conclusions—are systematically biased by participant characteristics that are fundamental to the phenomenon under investigation.

The Current Study

The current study addresses the methodological and conceptual gaps in the literature described above by leveraging Apple's "Screen Time" application to obtain objective digital-technology use data and employing random intercept cross-lagged panel models (RI-CLPMs; Hamaker et al., 2015) to provide critical insight into the within-persons temporal dynamics—and between-persons associations—between objectively measured digital-technology use and psychological distress among young adults. Specifically, we investigated the following research questions:

> *Research Question 1:* What are the within-persons, temporal dynamics between three common components of objectively measured digital-technology use (duration and frequency of iPhone use and duration of social-media use) and three commonly investigated aspects of self-reported psychological distress (depression, anxiety, and social isolation)?
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> *Research Question 2:* What are the between-persons associations between these components of objectively measured digital-technology use and self-reported psychological distress?
>
> *Research Question 3:* How do the within-persons and between-persons associations vary across different components of digital-technology use and aspects of psychological distress?

Method

Participants and procedures

Participants for this four-wave online panel study were recruited via Prolific—an online participant-recruitment platform that specializes in academic research (Palan & Schitter, 2018). Participants were eligible if they were U.S. residents, 18 to 35 years old, iPhone users, and had 10 or more previous submissions on Prolific with a task-approval rating of 95% or higher. Eligible participants who provided consent to participate in the study were routed to the online Qualtrics (https://www.qualtrics.com/) survey hosted by the University of Pittsburgh for data collection. Participants who successfully completed the Wave 1 survey were followed up for the remaining waves. Data were collected from August through November 2020; waves of data collection occurred approximately 1 month apart. Participants were compensated $4.00 for Wave 1 and $3.00 each for Waves 2 through 4. Participants who completed all four waves received a $2.00 bonus compensation. This study was approved by the University of Pittsburgh Institutional Review Board.

Measures

Psychological-distress variables. We used the Patient-Reported Outcomes Measurement Information System (PROMIS) six-item adult short-form instruments to measure depressive symptom severity, anxiety symptom severity (Pilkonis et al., 2011), and social isolation (Hahn et al., 2014). For both the depression and anxiety measures, respondents were asked to rate their symptom severity over the previous 7 days using a 5-point Likert-style scale that ranged from *never* (1) to *always* (5). Specific items for the depression measure included "I felt worthless," "I felt helpless," "I felt depressed," "I felt hopeless," "I felt like a failure," and "I felt unhappy." Specific items for the anxiety measure included "I felt fearful," "I found it hard to focus on anything other than my anxiety," "My worries overwhelmed me," "I felt uneasy," "I felt nervous," and "I felt like I needed help for my anxiety." For the social-isolation measure, respondents were asked to rate how often they experience the following items using a 5-point Likert-style scale that ranged from *never* (1) to *always* (5): "I feel left out," "I feel that people barely know me," "I feel isolated from others," "I feel that people are around me but not with me," "I feel isolated even when I am not alone," and "I feel that people avoid talking to me." The PROMIS measures are scored using an item-response-theory approach and are calibrated to be representative of the general adult U.S. population. We used the HealthMeasures Scoring Service (https://www.healthmeasures.net/)—which encompasses the PROMIS measures—to transform participants' raw scores into standardized T scores (with a mean of 50 and standard deviation of 10). A T score of 50 represents the average level of depression, anxiety, or social isolation among the general adult U.S. population (range = 38.4–80.2).

Objectively measured digital-technology-use variables. At each wave, participants uploaded screenshots from their Screen Time iPhone application to obtain objective data on time spent using digital technology. The Screen Time application passively tracks a variety of device-use metrics and comes preinstalled on all iPhones running iOS Version 12 or later. We provided participants detailed instructions for how to navigate to the application and take and upload the screenshots. To ensure that we obtained a full week of device-logged data, we asked participants to upload screenshots from the past week. We manually extracted three elements of data from the screenshots: (a) past-week screen time—the total duration of time that the device was engaged; (b) past-week "social" time—the total duration of time spent on applications categorized by Apple as social media (e.g., Facebook, Instagram, Snapchat, Messages); and (c) past-week number of "pickups" (i.e., the number of times the device was unlocked and engaged). Raw totals of the pickups variable were rescaled by dividing by 100 when running the statistical analyses.

Control variables.

Pandemic-related distress variables. To account for the potential impacts of the COVID-19 pandemic on psychological distress, we asked participants to complete self-report items that assessed exposure to pandemic-related stressors and perceived pandemic-related impact on psychological distress (depression, anxiety, and loneliness). For pandemic-related stressors, participants were prompted to "Rate how much the following items have contributed to any distress you may be experiencing due to the COVID-19 outbreak over the past month." Stressors included "lost job or income," "loved one got sick or passed away," "not having enough money," "not seeing friends in person," "not seeing family in person," "worried I might get sick," "living alone," "conflict with people I'm living with," "childcare responsibilities," and "difficulty getting food, medications, or other necessities." The response scale for each item ranged from 0 (*not at all*) to 10 (*a great deal*). Each item was dichotomized into presence/absence of distress (in which 0 included responses of "0" or "Not applicable" and 1 included responses of "1" or greater) and then summed to create a sum score of COVID-19-related stressors.

For the pandemic-related impact on psychological-distress variables, participants were queried, "Over the past month, how much has the COVID-19 outbreak, and the resulting changes to daily life, impacted the following aspects of your well-being?" Participants rated COVID-19-related impact on their experience of anxiety, loneliness, and depressed mood. The response scale for each item ranged from −10 (*decreased greatly*) to +10 (*increased greatly*); 0 = *no change*. We included the person means for each of the COVID-19-related distress variables in the statistical analyses.

Sociodemographic variables. Participants' age, gender, education level, and race/ethnicity were assessed with single items at baseline (i.e., Wave 1). Aside from age, these variables were dichotomized for the statistical analyses: gender (1 = female), education level (1 = bachelor's degree or higher completed), race (1 = person of color), ethnicity (1 = Hispanic).

Data diagnostics

Data screening. We implemented robust data-screening procedures to ensure that high-quality data were collected. The most robust check on data quality was the requirement to upload multiple screenshots. This allowed us to check each screenshot for internal consistency (i.e., that the time of day and data provider listed at the top of each screenshot matched for each participant). Needing to upload multiple screenshots also made it very difficult for participants to upload inauthentic screenshots (i.e., images downloaded from the Internet) because it is rare to find publicly available Screen Time screenshots that (a) are internally consistent and (b) contain the exact data we requested for the study. In addition, we included three attention checks at each wave of data collection. Participants who failed two or more attention checks during a single wave were excluded.

A total of 396 participants completed the Wave 1 survey. However, 12 participants were excluded because they submitted false screenshots, failed multiple attention checks, or failed to submit the correct Prolific authentication code (which proves that participants completed the survey), which left a final baseline sample of 384.

Missing data. Most missingness was due to participant attrition (i.e., person-level missingness). Yet attrition rates were relatively low: Wave 1, n = 384; Wave 2, n = 337; Wave 3, n = 318; and Wave 4, n = 308. There was some item-level missingness for the objective digital-technology-use variables caused by participants not having the Screen Time application enabled or operational malfunctions. In addition, if people did not have the "social" category in their top three most used application categories (other examples of categories include "entertainment," "information & reading," and "productivity & finance"), their past-week social-media time was not displayed in the screenshot. These instances were coded as missing (for the item-missing patterns for the digital-technology-use data, see supplementary Table S1 at https://osf.io/crbdk/). There were no differences between participants who completed all four waves of data collection and participants who dropped out at some point during the study with respect to demographics, digital-technology use, or psychological-distress variables. Thus, we used full information maximum likelihood for missing data (Muthén & Muthén, 2017) when estimating the statistical models.

Analytic strategy

Planned analyses. We calculated descriptive statistics for all variables and intraclass correlation coefficients (ICCs) to assess the level of within- and between-persons variance in the time-varying variables (i.e., objective digital-technology-use variables and psychological-distress variables). We estimated RI-CLPMs (Hamaker et al., 2015) to examine the within-persons temporal dynamics and between-persons associations between digital-technology use and psychological distress. Our a priori Monte Carlo power analysis indicated power of 0.9 or greater to detect standardized cross-lagged effects as small as β = 0.15 with a sample size of 384 and four time points (α = .05). RI-CLPMs were estimated using Mplus (Version 8.6; Muthén & Muthén, 2017). Model fit was evaluated using the root-mean-square error of approximation (RMSEA), comparative fit index (CFI), Tucker-Lewis index (TLI), and standardized root-mean-square residual (SRMR). Per Hu and Bentler (1999), we considered RMSEA values of .06 or less, CFI and TLI values of .95 or greater, and SRMR values of .08 or less as demonstrating good fit to the data. The data and analysis code for this study are publicly available via OSF at https://osf.io/mueny.

RI-CLPMs augment the traditional CLPM—a method commonly used for modeling temporal dynamics in panel data—by separating the variance in the time-varying variables into between- and within-persons components (for an illustration of the RI-CLPMs used in this study, see Fig. 1). The partitioning of variance in the RI-CLPM is crucial because time-invariant traits at the between-persons level, such as individual differences in average levels of psychological distress and/or digital-technology use, may confound within-persons dynamics (Curran & Bauer, 2011; Hamaker et al., 2015). This partitioning is done by specifying random intercepts (RI.PD and RI.DTU in Fig. 1) for each time-varying variable (PD_1–PD_4 and DTU_1–DTU_4 in Fig. 1). In our application, the random intercepts reflect trait-like individual differences in psychological distress and digital-technology use and account for the fact that some

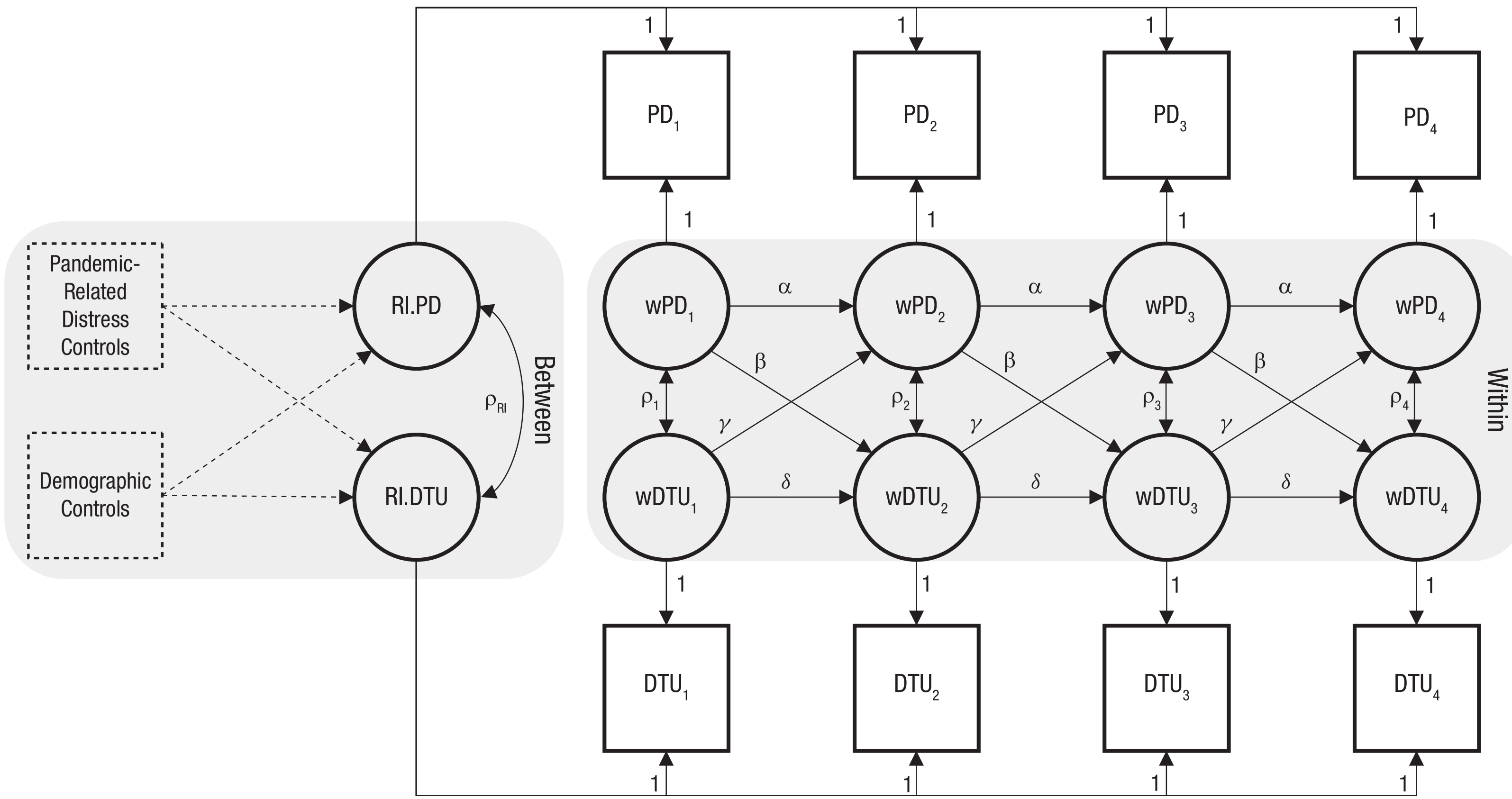

Fig. 1. Path diagram illustrating the random-intercept cross-lagged panel models used in the current study. Squares represent observed variables. Circles represent the latent random intercepts (RI.PD and RI.DTU) and within-persons deviations (wPD and wDTU). Single-headed arrows represent regression paths, and double-headed arrows represent (residual) correlations. At the within level, autoregressive paths (α and δ) are denoted by horizontal arrows, cross-lagged paths (β and γ) by diagonal arrows, and within-waves (residual) correlations (ρ) by vertical double-headed arrows. Autoregressive and cross-lagged paths were constrained to equality over time. At the between level, random intercepts were correlated (ρ_{RI}) and were regressed on the time-invariant control variables. The control variables and their regression paths are depicted with dashes to signify that they were included in only a portion of the computed models. PD = psychological distress; DTU = digital-technology use; RI = random intercept.

people have higher (or lower) average levels of psychological distress and digital-technology use than others. Because the random intercepts partial out the between-persons variance from the time-varying variables, the remaining within-persons components (wPD_1–wPD_4 and $wDTU_1$–$wDTU_4$ in Fig. 1) reflect state-like deviations from the person's typical level of psychological distress or digital-technology use for a given point in time. These time-varying within-persons components constitute the dynamic portion of the model, in which they are regressed on themselves (i.e., autoregressive effects α and δ) and each other (i.e., cross-lagged effects β and γ) at a time lag of 1—which in our study represents 1 month. For this study, we are particularly interested in the cross-lagged effects, which signify how deviations from a person's typical digital-technology use during a particular month predict deviations from their typical psychological distress the next month or vice versa.

Our modeling approach followed that of Hamaker and colleagues (2015). We modeled every combination of the three time-varying digital-technology-use variables (screen time, social media, and pickups) with the three time-varying psychological-distress variables (depression, anxiety, and social isolation), which resulted in nine distinct classes of RI-CLPMs. For each model, we included random intercepts (RI.PD and RI.DTU in Fig. 1) by creating latent factors for each psychological-distress and digital-technology-use variable, regressing the latent factors on each of the measurement occasions for each variable (PD_1–PD_4 and DTU_1–DTU_4 in Fig. 1), and fixing the factor loadings to 1. The time-varying within-persons deviations from a person's typical level of psychological distress or digital-technology use (wPD_1–wPD_4 and $wDTU_1$–$wDTU_4$ in Fig. 1) were modeled by specifying a latent variable for each measurement occasion (PD_1–PD_4 and DTU_1–DTU_4 in Fig. 1) and constraining the factor loading to 1. The residual variances of the observed variables (PD_1–PD_4 and DTU_1–DTU_4 in Fig. 1) were constrained to 0 so that all between- and within-persons variance would be captured by the trait-like (RI.PD and RI.DTU in Fig. 1) and state-like (wPD_1–wPD_4 and $wDTU_1$–$wDTU_4$ in Fig. 1) latent factors, respectively. Correlations among the random intercepts (ρ_{RI}) were specified to estimate associations among individual differences in digital-technology use and psychological distress, whereas within-waves correlations (ρ_1–ρ_4), autoregressive paths (α and δ), and cross-lagged paths (β and γ) were specified among the wave-specific residual latent factors (wPD_1–wPD_4 and $wDTU_1$–$wDTU_4$ in Fig. 1) to investigate the within-persons dynamics of digital-technology use and psychological distress. In line with the recommendations of Orth and colleagues (2021), given the equal intervals between measurement occasions (1 month) and the relatively brief period of observation (4 months), we did not expect systematic differences in the structural parameters across waves; thus, the autoregressive and cross-lagged paths were constrained to equality over time.

At the between-persons level, we specified the pandemic-related-distress variables and demographic control variables as predictors of the random intercepts to examine whether these variables predicted individual differences in average levels of digital-technology use and psychological distress and to control for their effects among the within-persons structural parameters (i.e., autoregressive and cross-lagged effects). Because of collinearity between the perceived pandemic-related impact on psychological-distress items, we specified a latent factor variable—with the anxiety, depression, and loneliness items as indicators—as a predictor of the random intercepts in the models. Age was grand mean centered.

In line with the recommendations of Simmons and colleagues (2011), given that results of statistical analyses may vary substantially across different covariate specifications, we reran each of the models with three different control-variable specifications to examine the robustness of results: (a) no control variables, (b) demographic variables only, and (c) all the demographic and pandemic-related-distress variables described above. Note that when presenting the results of Research Question 2—the between-persons associations between the objectively measured digital-technology use and psychological distress—we report the correlation between the random intercepts only for the models with no controls. This is because the models in which controls are included as predictors of the random intercepts would transform the correlation between the random intercepts into a residual correlation, which is no longer answering the same question specified in Research Question 2.

Post hoc analyses. In addition to running the models on the full sample as described above, in response to reviewer and editorial feedback, we also ran subgroup RI-CLPMs to examine whether the within- and between-persons effects of interest differed by gender or age category. Given recent suggestions that the association between psychological distress and digital-technology use is stronger for women than men (Twenge et al., 2020) and for younger people than older people (Heffer et al., 2019, p. 468), these subgroup analyses may provide insight into these important and unresolved claims. Following the RI-CLPM subgroup-modeling approach explicated by Mulder and Hamaker (2021), we specified two classes of RI-CLPM subgroup models: (a) men versus women and (b) ages 18 to 24 versus ages 25 to 35.[1] In these subgroup RI-CLPMs, the gender and age category variables serve as the grouping variable, which allows for the models to be estimated separately within each group. As a result, group-specific model estimates are obtained for every parameter, providing added insight into how effects differ across groups. Consistent with our analyses on the full sample, we constrained the autoregressive and cross-lagged effects to equality over time within each group. We also conducted the control-variable robustness checks for each subgroup analysis, but with one modification: In the gender-subgroup analyses, we did not include gender as a predictor of the random intercepts, and in the age category analyses, we did not include age as a predictor of the random intercepts. Note, however, that because we did not design our study with the intention of estimating subgroup models, these analyses may be underpowered. Thus, the results should be viewed as exploratory.

Multiple comparisons. Given the debate about methodological bias in digital-effects research (e.g., Orben & Przybylski, 2019; Twenge et al., 2020), we elected not to perform an α correction when presenting results to avoid potential accusations of being biased toward null results. Instead, we presented *p* values at multiple levels of significance ($p < .05$, $p < .01$, and $p < .001$) alongside model estimates in supplementary Table S2 at https://osf.io/zmh3a/. Thus, readers may adopt an α correction when interpreting results by focusing on a particular significance threshold.

Results

Descriptive statistics

Table 1 provides sample demographics and descriptive statistics for all variables included in the analyses. Mean age of the sample was 24.5 years (*SD* = 5.1 years); 54% identified as White, 15% identified as Hispanic, and close to half reported obtaining at least a bachelor's degree education. Overall, participants averaged 47.5 hr of screen time, 15.5 hr of social media, and 678 pickups over the past week. Mean depression, anxiety, and social isolation T scores were 54.6, 56.6, and 49.6, respectively—which indicates that this sample had higher than average rates of depression and anxiety and average levels of social isolation.

ICCs for the time-varying variables indicated that 21% to 29% of the variation in the digital-technology-use and psychological-distress variables was within persons. Specifically, screen time, social media, and pickups had ICCs of .75, .74, and .71, respectively, and depression, anxiety, and social isolation, had ICCs of .79, .73, and .77, respectively.

Table 1. Sample Demographics and Descriptive Statistics

Variable	Value
Sociodemographics	
Age (years)	*M* = 24.5 (*SD* = 5.1)
Race	
White	208 (54%)
Asian	114 (30%)
Black	28 (7%)
Multiracial	18 (5%)
Other	16 (4%)
Hispanic	57 (15%)
Gender	
Female	217 (57%)
Male	161 (42%)
Other	6 (2%)
Education level	
Graduate degree	61 (16%)
Bachelor's degree	122 (32%)
Some college	131 (34%)
High school	70 (18%)
Objective technology use	
Screen time (hr)	*M* = 47.5 (*SD* = 25.0)
Social media (hr)	*M* = 15.5 (*SD* = 11.5)
Number of pickups (×100)	*M* = 6.78 (*SD* = 3.3)
Psychological distress variables	
Depression	*M* = 54.6 (*SD* = 9.9)
Anxiety	*M* = 56.6 (*SD* = 9.8)
Social isolation	*M* = 49.6 (*SD* = 10.1)
Pandemic-related covariates	
Exposure to pandemic-related stressors (sum)	*M* = 4.6 (*SD* = 2.1)
Perceived impact on depression	*M* = 2.1 (*SD* = 3.5)
Perceived impact on anxiety	*M* = 2.6 (*SD* = 3.6)
Perceived impact on loneliness	*M* = 2.7 (*SD* = 3.8)

Note: Values are *n*s with percentages in parentheses unless otherwise noted.

Results of RI-CLPMs

Below, we describe the results of the planned analyses with the full sample of participants across control-variable specifications, followed by the results of the subgroup post hoc analyses. We present unstandardized parameters for the within-persons cross-lagged effects and Pearson correlations for the between-persons associations. Although unconventional, in line with the recommendations of Funder and Ozer (2019) and Orben (2020), we believe presenting the unstandardized cross-lagged effects offers a more intuitive understanding of effect size in this case. Previous work on the PROMIS anxiety and depression measures used in our study indicate that a minimally important difference—defined as "the smallest difference in score . . . which patients perceive as beneficial and which would mandate, in the absence of troublesome side effects and excessive cost, a change in the patient's (health care) management" (Guyatt et al., 2002, p. 377)—is 3 to 4 points (Kroenke et al., 2019, 2020). Given the easy-to-understand units of the digital-technology-use variables (i.e., duration of time [hr]; number of pickups [in increments of 10]), we can compare the unstandardized *b*s for the cross-lagged effects against the benchmark of a minimally important difference in the psychological-distress variables.

To illustrate, a cross-lagged effect size (*b*) of 0.50 for social-media use as a predictor of anxiety indicates that a 1-hr deviation from a person's typical level of social-media use at time *T* predicts a 0.50 increase from their typical level of anxiety at time $T + 1$. Thus, if we assume monotonical linear effects, it would require a 6- to 8-hr within-persons deviation in social media at time *T* to register a meaningful increase in anxiety at time $T + 1$. Conversely, a cross-lagged effect size (*b*) of 0.50 for anxiety as a predictor of social-media use indicates that a 3- to 4-unit deviation (a minimally important difference) from a person's typical level of anxiety at time *T* predicts a 1.5- to 2-hr increase from their typical amount of social-media use at time $T + 1$.

Results of full-sample analyses.

Model fit statistics. Comparing across sets of control-variable specifications, the models with demographic controls only and the models with no controls fit the data slightly better than the models with all controls. Generally, the demographic-controls-only models exhibited the best fit out of the three specification sets. Yet, overall, almost every model across the different specifications met our thresholds for good fit described above. Several of the models in the all-controls specification had RMSEAs, CFIs/TLIs, or SRMRs that were slightly outside (i.e., .01–.03 points) our thresholds, and one of the models in the no-controls specification (social media and social isolation) had an RMSEA of .063, which is slightly outside the threshold of RMSEA ≤ .06. Every model in the demographics-only specification exhibited good fit to the data according to our thresholds. Taken together, we consider the RI-CLPMs specified in our analyses to be acceptable fits to the data. For complete fit statistics for all models, see supplementary Table S3 at https://osf.io/a6u9z/.

Within-persons prospective effects. Estimates for the full-sample cross-lagged effects across the digital-technology-use–psychological-distress variable combinations and control-variable robustness checks are illustrated in Figure 2. For the cross-lagged parameters regarding digital-technology use as a predictor of psychological distress, effect sizes ranged from $b = 0.012$ to $b = 0.130$ for social media, $b = -0.002$ to $b = 0.069$ for screen time, and $b = -0.197$ to $b = 0.362$ for pickups. For the psychological distress as a predictor of digital-technology use cross-lagged parameters, effect sizes ranged from $b = 0.031$ to $b = 0.185$ for anxiety, $b = 0.022$ to $b = 0.117$ for depression, and $b = -0.045$ to $b = 0.071$ for social isolation. With one exception (depression as a predictor of screen time), effect sizes in the models with only demographic controls or no controls were consistently larger than in the models with both demographic and pandemic-related-distress controls. In terms of statistical significance, social media and screen time predicting anxiety were significant in the demographics-only and no-controls specifications, and anxiety predicting pickups was significant in the no-controls specification.

Between-persons correlations. Estimates for the full-sample between-persons associations across the digital-technology-use–psychological-distress variable combinations are illustrated in Figure 3. For social media, effect sizes ranged from $r = .074$ with social isolation to $r = .116$ with anxiety. For screen time, effect sizes ranged from $r = .088$ with social isolation to $r = .124$ for anxiety. For pickups, effect sizes ranged from $r = -.107$ with depression/anxiety to $r = -.084$ with social isolation. However, only the correlation between screen time and anxiety was statistically significant.

Results of subgroup analyses.

Model fit statistics. In both the gender-subgroup and age-subgroup models, the demographics-only specification consistently displayed the best fit to the data; all submodels met criteria for good fit. The no-controls specification generally displayed good fit across the gender subgroup models, with one exception (social isolation–pickups model). However, in the age subgroup models, the no-controls specification was slightly above our threshold for good fit with respect to RMSEA (social media–depression, social media–social isolation, pickups–depression, and pickups–social isolation) and SRMR (social media–social isolation and pickups–social isolation). On the other hand, the all-controls specification exceeded our SRMR threshold in every subgroup model across the gender and age RI-CLPMs and exceeded our RMSEA and CFI/TLI thresholds in more than half of the submodels. Consequently, we advise readers to apply caution when interpreting the results of the all-controls specification for the subgroup models.

Within-persons prospective effects. Estimates for the cross-lagged effects across all combinations of digital-technology-use and psychological-distress variables, subgroup models, and control-variable robustness checks

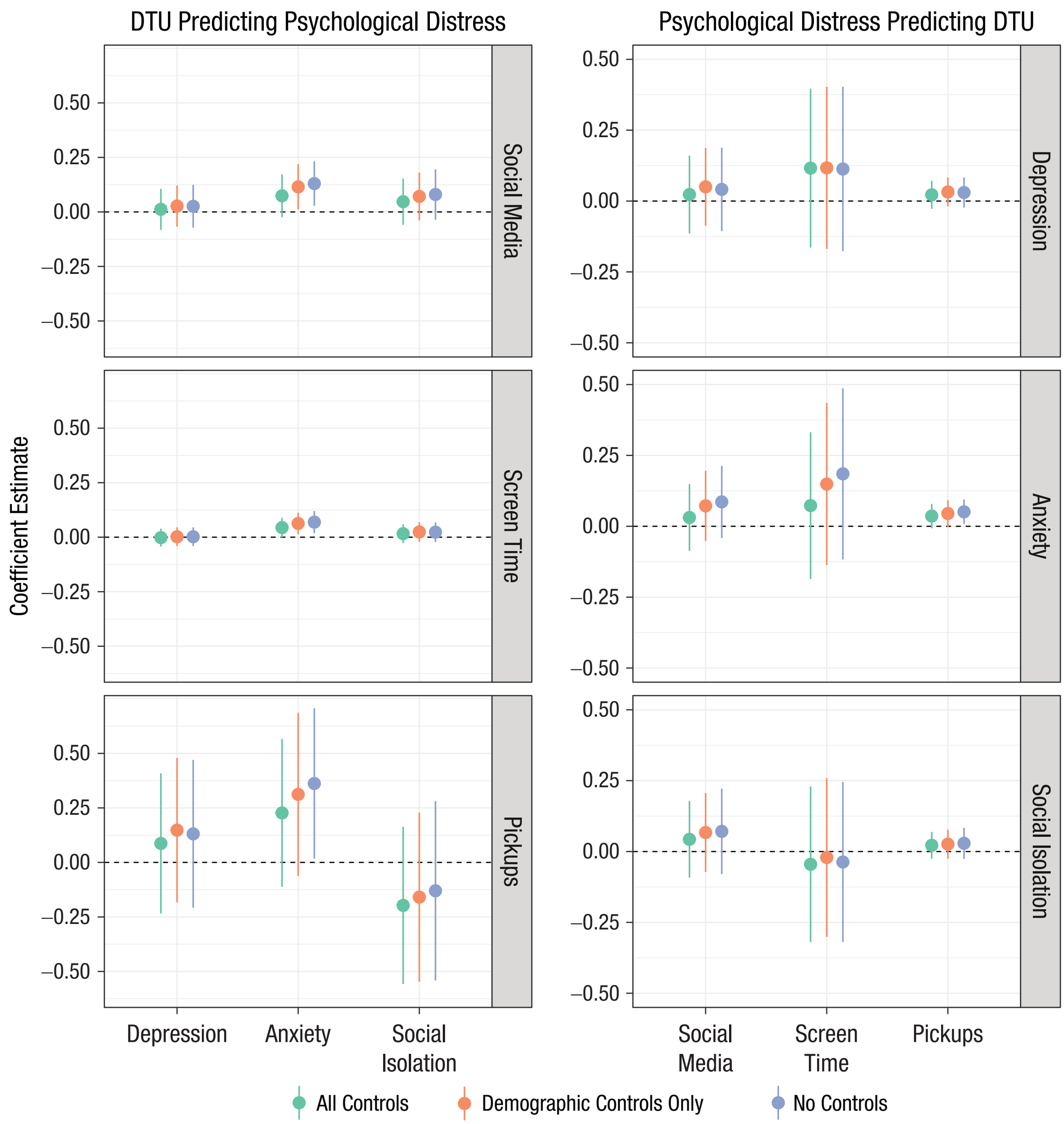

Fig. 2. Coefficient plot showing within-persons cross-lagged effects of digital-technology-use (DTU) variables predicting psychological-distress variables (left) and psychological-distress variables predicting DTU variables (right) for the full sample. Each cross-lagged effect was estimated across three control-variable robustness checks: pandemic-related-distress and demographic controls, demographic controls only, and no controls. Coefficient estimates (y-axis) are unstandardized bs. Circles represent point estimates, and vertical bars represent 95% confidence intervals.

are illustrated in Figure 4; estimates for the full-sample analyses are included for comparison. Generally, estimates did not vary substantially across control-variable specifications for each cross-lagged effect.

Using the effects from the demographics-only specification for simplicity, for both men and women, social-media use did not significantly predict depression or social isolation but did predict anxiety (men: $b = 0.196$, $p < .05$; women: $b = 0.125$, $p < .05$). This was also true of screen time (men: $b = 0.098$, $p < .01$; women $b = 0.082$, $p < .01$). Pickups did not significantly predict any psychological distress variable for men but did predict anxiety for women (women: $b = 0.427$, $p < .05$). Conversely, for cross-lagged effects of psychological distress as a predictor of digital-technology use, anxiety significantly predicted social-media use and pickups

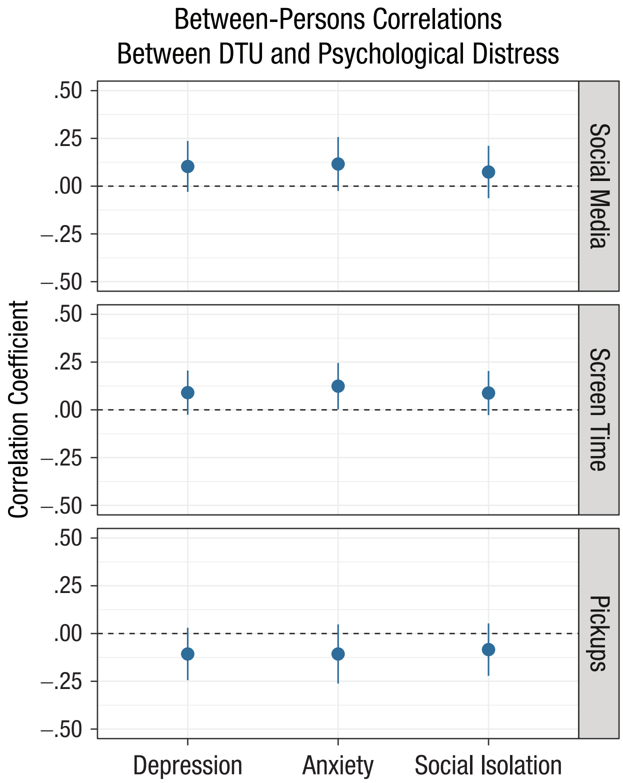

Fig. 3. Coefficient plot showing estimates for between-persons correlations between digital-technology-use (DTU) and psychological-distress variables for the full sample. Correlation coefficient (y-axis) is Pearson's r. Circles represent point estimates, and vertical bars represent 95% confidence intervals.

among women (social media: $b = 0.182$, $p < .05$; pickups: $b = 0.074$, $p < .01$), depression significantly predicted pickups only among men (pickups: $b = 0.076$, $p < .01$), and social isolation did not significantly predict any digital technology use for either men or women.

Again, using the effects from the demographics-only specification, social media, screen time, and pickups significantly predicted anxiety only among 18- to 24-year-olds (social media: $b = 0.137$, $p < .05$; screen time: $b = 0.061$, $p < .05$; pickups: $b = 0.429$, $p < .05$). Conversely, for cross-lagged effects of psychological distress as a predictor of digital-technology use, only anxiety significantly predicted pickups among 18- to 24-year-olds ($b = 0.061$, $p < .05$).

Between-persons correlations. Estimates for the between-persons associations across all digital-technology-use–psychological-distress variable combinations and subgroup models are illustrated in Figure 5. Only two of the random intercept correlations across the subgroup RI-CLPMs were statistically significant, and both occurred in the 25- to 35-year-olds group (screen time–anxiety: $r = .222$, $p < .05$; social media–anxiety: $r = .246$, $p < .05$).

Discussion

The goal of this study was to gain a better understanding of temporal dynamics between digital-technology use and psychological distress among young adults. Critically, by employing (a) objective measures of digital-technology use and (b) a within-persons prospective analysis and (c) examining how different aspects of digital-technology use (social media, screen time, and pickups) relate to different components of psychological distress (anxiety, depression, and social isolation), we overcame common methodological limitations that have undermined the quality of results among extant research. In addition, we tested the robustness of results across different control-variable specifications and, in post hoc analyses, investigated whether effects varied by gender or age category. Overall, we found that the prospective effects of social media, screen time, and pickups on anxiety, depression, and social isolation—and vice versa—were small and statistically nonsignificant across most specifications and subgroups. In other words, using digital technology more or less than usual did not predict meaningful changes in psychological distress over time or vice versa.

The use of an objective measure to capture digital-technology use is a critical contribution of the present study. Note that our use of the word "objective" to describe device-logged measures of digital-technology use does not connote that these measures perfectly capture actual use (see Limitations) but, rather, that they do not depend on the respondent's subjective experiences, abilities, or beliefs. Conversely, self-report measures of digital-technology use, like any self-reported behavior, are heavily influenced by respondents' subjective cognitive functions such as self-awareness, attention, and perception (Tourangeau, 1984). As a result, the accuracy of self-reported digital-technology use depends in part on respondents' baseline levels of cognitive functioning and various phenomena that affect cognitive functioning, such as mental health or age (Ernala et al., 2020; Sewall et al., 2020; vanden Abeele et al., 2013). The subjective influences underpinning self-reported digital-technology use suggest that these measures are capturing a construct more related to *perceived* use than *actual* use; accordingly, "the antecedents, correlates, and consequences of perceived use are likely distinct from those of actual use" (Sewall et al., 2020, p. 395). As the first study to analyze the prospective, within-persons association between digital-technology use and psychological distress using

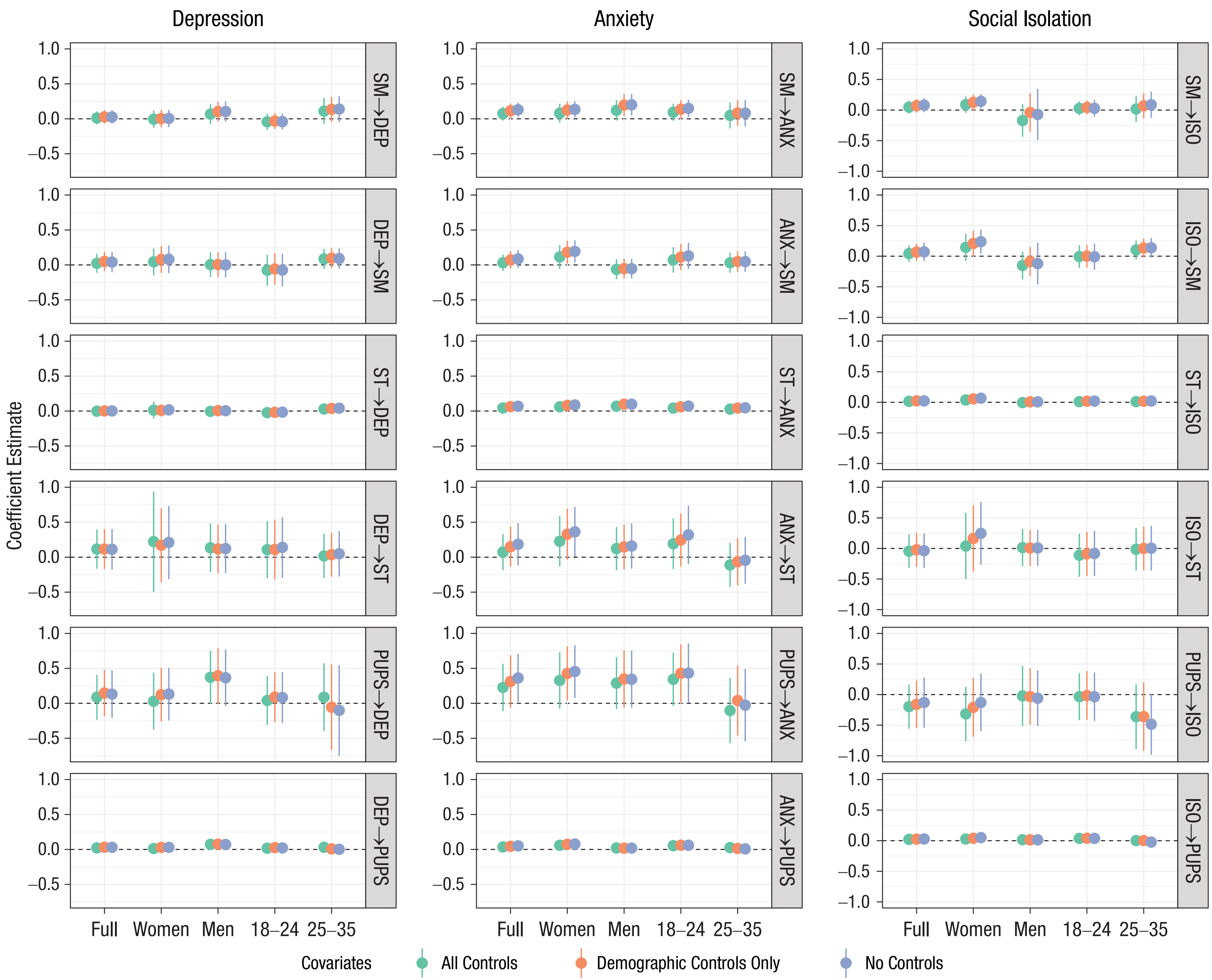

Fig. 4. Coefficient plots showing cross-lagged effects across all robustness checks and subgroups. The labels on the right side of each plot describe the predictor-to-response cross-lagged effect being estimated. For example, the top left plot shows the cross-lagged effect of social media predicting depression (SM→DEP) across different model specifications. Coefficient estimates are unstandardized *b*s. Circles represent point estimates, and vertical bars represent 95% confidence intervals. ST = screen time; PUPS = pickups; ANX = anxiety; ISO = social isolation.

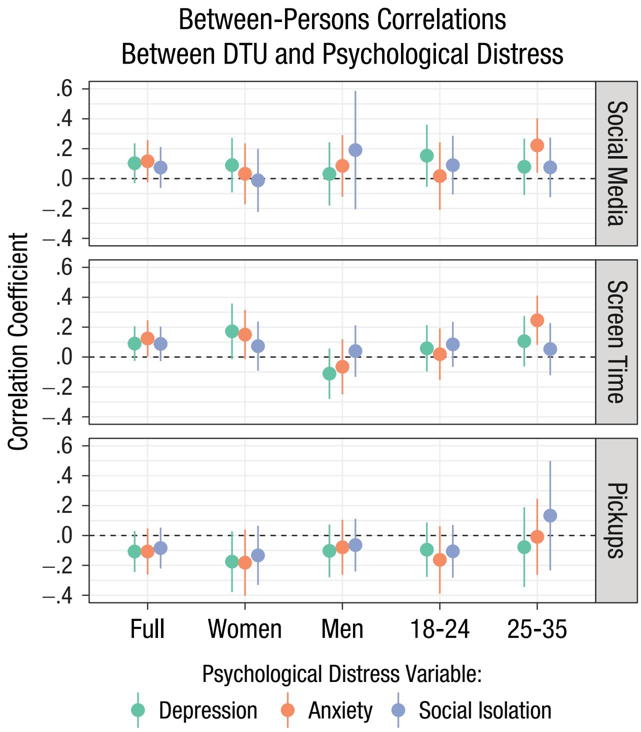

Fig. 5. Coefficient plot showing between-persons correlations between digital-technology-use (DTU) and psychological-distress variables across subgroups. Correlation coefficient (y-axis) is Pearson's r. Circles represent point estimates, and vertical bars represent 95% confidence intervals.

objective measures of digital-technology use, our findings inform the field in a number of important ways. We discuss three below.

Scant evidence for prospective effects

We found limited evidence that three distinct yet commonly investigated aspects of digital-technology use—duration and frequency of smartphone use and duration of social-media use—exhibited meaningful prospective associations with three commonly investigated aspects of psychological distress—depression, anxiety, and social isolation. By "meaningful," we are not strictly referring to statistical significance but also practical significance. That is, in addition to knowing the trustworthiness of the effect (i.e., statistical significance), it is important to understand whether the size of the effect would have any meaningful or real-world impact on an individual (see Funder & Ozer, 2019). As mentioned in the Results section, thanks to previous validation work on the PROMIS measures used in this study (Kroenke et al., 2019, 2020), we can identify a meaningful effect size as one that results in a 3- to 4-point change on the psychological-distress measures. With this confluence of statistical and practical significance in mind, the overall results of our study indicate that—except in the most extreme cases—whether a person uses more or less digital technology than usual in the current month will not have a meaningful effect on the person's typical level of psychological distress the next month.

To illustrate, across the full-sample RI-CLPMs, the largest statistically significant cross-lagged effect was for social media as a predictor of anxiety in the no-controls specification ($b = 0.130$, $p < .05$). To register a meaningful effect on anxiety would require at least a 3 / 0.130 = 23-hr deviation from a person's typical weekly level of social media use. Given that the average within-persons standard deviation for social-media use was 7.4 hr, a 23-hr deviation from a person's typical past-week social media use is highly unlikely (i.e., > 3 *SD* above the mean). Moreover, taking the largest cross-lagged effect across all RI-CLPM specifications and subgroups (social media as a predictor of anxiety for men in the no-controls specification; $b = 0.201$, $p < .05$), to register a meaningful effect on anxiety would necessitate at least a 3 / 0.201 = 15-hr deviation from a person's typical level of social-media use—a deviation that is 2.4 *SD* above the men's average within-persons standard deviation for past-week social-media use.

To sum up, after estimating a total of 81 permutations of RI-CLPMs across the nine digital-technology-use–psychological-distress variable combinations, three control-variable robustness checks, and three subgroup specifications, we found that most cross-lagged effects were statistically nonsignificant and that all were very small—even the largest of the cross-lagged effects were unlikely to register a meaningful impact on a person's psychological distress. Furthermore, across the 81 RI-CLPM permutations, we tested a total of 270 cross-lagged effects for statistical significance, and 36 of these effects had $p < .05$. However, given $\alpha = .05$, we would expect 13 of the 270 effects to be significant by chance alone. If we adopt a corrected α, even a very liberal one such as $p < .01$ instead of $p < .05$, 29 of the 36 effects would no longer be significant, including the largest effects described above.

Scant evidence for subgroup differences

The ubiquitous use of digital technology among young people in the United States has led many people to wonder whether it is to blame for the elevated rates of psychological distress among this generation—especially women. This question has received substantial academic (Haidt & Allen, 2020; Twenge, 2020; Twenge et al., 2020, 2021) and public interest, exemplified most recently by *The Wall Street Journal*'s exposé of Facebook (Wells et al., 2021). In post hoc analyses, we used subgroup RI-CLPMs to investigate a version of this question—that is, whether the digital-technology use–psychological distress effects were stronger for women than men or among younger participants (i.e., 18- to 24-year-olds) than older participants (i.e., 25- to 35-year-olds). Given the array of estimates across primary variable pairs, subgroups, and robustness checks, it is not possible to discuss every combination of results. However, one way to approach the array of subgroup results is to look across models to find the strongest possible evidence in favor of the claim that digital-technology use is more harmful for women and/or younger people. If selecting the estimates in this way still results in weak evidence, then we can reasonably conclude that the data do not support this claim.

Starting with gender—at the within-persons level, the models with no control variables consistently had the strongest effects both in terms of statistical significance and effect size. Looking across digital-technology use and psychological-distress variable combinations, nine of the 18 cross-lagged effects were statistically significant for women, whereas only three of the 18 were significant for men. Furthermore, 13 of the 18 cross-lagged effects were stronger for women than men.

However, although 13 of the 18 cross-lagged effects were stronger for women than men, there are three important considerations that undermine this evidence. First, social media—which some have suggested is particularly harmful for women compared with other forms of digital-technology use (e.g., Twenge et al., 2020)—had larger effects on depression and anxiety for men than women. Second, eight of the 13 statistically significant cross-lagged effects among women were for psychological distress predicting digital-technology use, whereas only one of these effects (depression as a predictor of pickups) was significant for men. This runs counter to the prevailing claim that digital-technology use is responsible, at least in part, for fluctuations in psychological distress. Rather, the prevalence of psychological distress as a predictor of digital-technology use effects among women suggests that they may be more likely to use digital technology as a coping mechanism, which mirrors recent findings from other studies conducted during the COVID-19 pandemic (Cauberghe et al., 2021; Prowse et al., 2021). Finally, as discussed above, even the largest of significant effects (for women or men) were unlikely to have a meaningful impact. For instance, the largest digital-technology use cross-lagged effect for women was social media as a predictor of social isolation ($b = 0.143$, $p < .05$), which would require at least a 3 / 0.143 = 21-hr deviation from typical past-week social-media use to register a meaningful effect on social isolation. Given an average within-persons social media *SD* of 8.5 hr for women, this level of within-persons deviation is unlikely (i.e., 21 / 8.5 = 2.5 *SD* above average). Likewise, even the largest of the effects for psychological distress as a predictor of digital-technology use for women (anxiety as a predictor of screen time, $b = 0.363$, $p < .05$) was very small in real-world terms—experiencing a minimally important difference in anxiety (i.e., at least a 3-unit change) would amount to a 3 × 0.363 = 1.1-hr deviation from typical past-week screen time.

For age—at the within-persons level (demographics-controls-only specification), four of the 18 cross-lagged effects were significant for 18- to 24-year-olds, whereas none of the cross-lagged effects were significant for 25- to 35-year-olds. Furthermore, 11 of the 18 cross-lagged effects were stronger for the younger group than the older group.

However, similar to gender, the social-media effects were not consistently stronger among younger participants; even when taking the largest significant effect across digital-technology use–psychological distress combinations (social media as a predictor of anxiety, $b = 0.137$, $p < .05$), it would require a large deviation (i.e., 2.5 *SD* above average) from typical past-week social-media use to register a meaningful effect.

Taken together, even when selecting results to make the strongest case in support of the claim that digital-technology use is more harmful for women and/or younger people, the evidence is thin. Women and 18- to 24-year-olds in our sample did not consistently have stronger effects for digital-technology use as a predictor of psychological distress—particularly with social-media use, which is purportedly more harmful among these groups. In cases in which effects were statistically significant, it would require an extreme deviation from a person's typical level of digital-technology use to register a meaningful effect on their psychological distress.

Contextual considerations

Our study occurred in fall 2020 during a once-in-a-century pandemic and a highly polarized presidential election. Although typically underacknowledged, all studies are subject to history and contextual effects—the phenomena observed and their associations may be affected by the unique sociohistorical context in which the study took place. National surveys indicate that the disruptions and distress caused by the pandemic contributed to elevated levels of psychological distress and digital-technology use among many young people (Czeisler et al., 2021; Samet, 2020). These elevated levels of psychological distress and digital-technology use were reflected in our study: Average levels of depression, anxiety, and objectively measured digital-technology use were higher in our sample of young adults compared with prepandemic samples (Ellis et al., 2019; Pilkonis et al., 2011; Sewall et al., 2020). Because the pandemic contributed to increases in psychological distress and digital-technology use, an association between the two may be implied and lead some people to infer that the association is causal—and identify digital-technology use as the culprit (e.g., Parks, 2021; Richtel, 2021). The results from our study suggest that the blame on digital-technology use may be displaced given that digital-technology use did not predict meaningful increases in psychological distress.

Note that the unique context in which the study took place may have produced results that are not truly representative of typical digital-technology use–psychological distress dynamics (for a general discussion of conducting studies during the COVID-19 pandemic, see Rosenfeld et al., 2021). The closings of schools, workplaces, and gathering places in response to the pandemic likely forced many people to rely more on digital technology to connect with others. The lack of opportunities for in-person interaction may have caused participants to deviate from their typical patterns of digital-technology use or the affordances they derive from their use. For example, rather than engaging predominantly in negative social comparison on social media—which is typically associated with lower well-being (Frison & Eggermont, 2016)—participants may have used social media more actively to stay connected with friends and loved ones, which is typically associated with higher well-being (Escobar-Viera et al., 2018). However, the lack of a counterfactual makes it impossible to identify whether or by how much our findings were affected by the contextual and historical effects of the COVID-19 pandemic. Thus, it is important for future research to replicate this study at a time that is sufficiently after the pandemic to investigate whether the findings hold.

Limitations

Although the present study addresses an important gap in digital-effects research, there are several limitations. First, our study was conducted with a nonrepresentative sample of young adult iPhone users residing in the United States and recruited from Prolific. It is plausible that iPhone and Prolific users differ systematically from a nationally representative sample of young adults. Although our sample had more racial/ethnic diversity than typical convenience-based samples in the social sciences (Roberts et al., 2020), the overall generalizability of the findings remains limited.

Second, although digital trace data generally provide more accurate and valid measures of digital-technology use than self-reports (Parry et al., 2021), they are not perfect (Jürgens et al., 2019). Technical inconsistencies and potential incongruencies between the captured data and the targeted construct are some of the issues that may contaminate the accuracy of these measures. Furthermore, although some usage-tracking applications have been externally validated (Elhai et al., 2018; Geyer et al., 2021), we are not aware of any published validations of the Screen Time application in the academic literature—making this an important avenue for future research.

Third, it is unclear whether a 1-month lag between waves of data collection is ideal for capturing the dynamics between digital-technology use and psychological distress. Whether there is an ideal time lag to detect these dynamics is an open question. However, the answer depends, at least in part, on the specific research questions of the study and accompanying operationalizations of digital-technology use and psychological distress. Given our focus on depression, anxiety, and social isolation—which would be unlikely to vary substantially at the daily or, perhaps, weekly level (Heinrich & Gullone, 2006; Lovibond, 1998)—and our use of RI-CLPMs, which estimate how deviations from people's typical levels of psychological distress and digital-technology use predict each other, we believe that the 1-month lags used in the current study are appropriate for answering the research questions.

Fourth, our analyses focused on relatively general aspects of digital-technology use. Recently, the field of digital-effects research is shifting toward a more idiographic approach to understanding the associations between digital-technology use and psychological distress (see Beyens et al., 2020; Valkenburg et al., 2021; vanden Abeele, 2021). This approach is signified by a shift away from general measures of digital-technology use (e.g., overall screen-time duration) toward specific aspects of use (e.g., content, affordances, etc.) and focusing on for whom the digital-technology use–psychological distress effects are most salient rather than on aggregate effects. This shift in research priorities and methodologies is much welcomed and holds substantial promise. Although our study is an initial step toward filling an important gap in the digital-effects literature, future studies should employ objective measures of general and specific aspects of digital-technology use and examine how effects vary across individuals and/or groups.

Conclusion

Addressing critical limitations pervasive in digital-effects research, we believe the current study represents a step forward in clarifying the understanding of the prospective associations between digital-technology use and psychological distress among young adults. Although the generality of our findings must be established by future research, our study provides robust evidence that at a time of elevated digital-technology use and psychological distress brought on by the COVID-19 pandemic, fluctuations in digital-technology use did not meaningfully contribute to fluctuations in psychological distress among young adults. We hope that the current research will stimulate future robust investigations into the potential association between different aspects of objectively measured digital-technology use and psychological distress among various populations and for whom these associations are most salient.

Transparency

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Author Contributions

Conceptualization: C. J. R. Sewall, T. R. Goldstein, and D. Rosen. Data curation: C. J. R. Sewall. Formal analysis: C. J. R. Sewall and A. G. C. Wright. Funding acquisition: C. J. R. Sewall and D. Rosen. Investigation: C. J. R. Sewall. Methodology: C. J. R. Sewall, T. R. Goldstein, A. G. C. Wright, and D. Rosen. Project administration: C. J. R. Sewall. Resources: T. R. Goldstein and D. Rosen. Supervision: T. R. Goldstein and D. Rosen. Validation: A. G. C. Wright. Visualization: C. J. R. Sewall. Writing, original draft: C. J. R. Sewall. Writing, review and editing: C. J. R. Sewall, T. R. Goldstein, A. G. C. Wright, and D. Rosen. All of the authors approved the final manuscript for submission.

Declaration of Conflicting Interests

T. R. Goldstein receives royalties from Guilford Press. A. G. C. Wright receives royalties from Cambridge University Press. The author(s) declared that there were no other potential conflicts of interest with respect to the authorship or the publication of this article.

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Open Practices

All data have been made publicly available via OSF and can be accessed at https://osf.io/mueny/. This article has received the badge for Open Data. More information about the Open Practices badges can be found at https://www.psychologicalscience.org/publications/badges.

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Note

1. This age categorization was suggested by a reviewer. Given that we included only young adults (ages 18–35) in our study, this categorization separates 18- to 24-year-olds (i.e., members of Generation Z) from 25- to 35-year-olds (i.e., Millennials). It has been suggested that the effect of psychological distress and digital-technology use is stronger among Generation Z than for older generations, such as Millennials.

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