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Batching smartphone notifications can improve well-being

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ABSTRACT

Every day, billions of us receive smartphone notifications. Designed to distract, these interruptions capture and monetize our time and attention. Though smartphones are incredibly helpful, their current notification systems impose underappreciated, yet considerable, mental costs; like a slot machine, they exploit our inherent psychological bias for variable rewards. With an app that we developed, we conducted a randomized field experiment ($n = 237$) to test whether batching notifications—delivering notifications in predictable intervals throughout the day—could improve psychological well-being. Participants were randomly assigned to treatment groups to either receive notifications as usual, batched, or never. Using daily diary surveys, we measured a range of psychological and health outcomes, and through our app system, we collected data on phone use behaviors. Compared to those in the control condition, participants whose notifications were batched three-times-a-day felt more attentive, productive, in a better mood, and in greater control of their phones. Participants in the batched group also reported lower stress, lower productivity, and fewer phone interruptions. In contrast, participants who did not receive notifications at all reaped few of those benefits, but experienced higher levels of anxiety and "fear of missing out" (FoMO). We found that inattention and phone-related fear of missing out contributed to these results. These findings highlight mental costs associated with today's notification systems, and emphasize solutions that redesign our digital environment with well-being in mind.

> "When I go online, I feel like one of B.F. Skinner's white Carneaux pigeons"
>
> – Michael Schulson (2015).

In the opinion of the United States Supreme Court (Riley v. California, 2014), our smartphones "are now such a pervasive and insistent part of daily life that the proverbial visitor from Mars might conclude they were an important feature of human anatomy" (Wittes & Chong, 2014). In the words of the justices, "many of these devices are in fact minicomputers that also happen to have the capacity to be used as a telephone. They could just as easily be called cameras, video players, calendars, tape recorders, libraries, diaries, albums, televisions, maps, or newspapers" (Riley v. California, 2014). As a result, this device has spread faster than any technology in human history (DeGusta, 2012). In the U.S., almost eight in ten adults, and more than 90% of people age 18–49, own one Pielot et al., 2014 Pielot et al., 2017(Pew Research Center, 2018).

Notifications – visual cues, auditory signals, and haptic alerts – are the most ubiquitous feature of the most ubiquitous device on the planet. In less than a decade, receiving a notification has become one of the most commonly occurring human experiences. They arrive bearing new information from or about a person, place, or thing: a text from your mom, news about Donald Trump, or a calendar invite for a meeting. People receive, on average, more than sixty notifications a day (Pielot, Church, & de Oliveira, 2014). It's likely that, over the course of reading this article, you will receive a notification on your smartphone.

These alerts, initially developed for an email client, have transformed our relationship with personal computing devices. Instead of a person initiating an interaction with a passive device, the devices themselves have begun actively delivering content and demanding users' attention. According to recent objective market data, people spend upwards of three to 5 h a day on their smartphones (Hackernoon, 2017), and they touch the device an average of 2617 times a day (dscout, 2016). It's no surprise then that almost half of Americans couldn't imagine their life without the device (Smith, 2015).

Notifications are not inherently bad or good: They can help us get to

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our meetings as much as they can derail them. But because we typically attend to them within minutes, they regularly interrupt ongoing activities (Battestini, Setlur, & Sohn, 2010;Pielot, de Oliveira, Kwak, & Oliver, 2014; Shirazi et al., 2014). In other words, it is not the notifications per se that pose a problem, but rather *how* and *when* they are delivered. They arrive at unpredictable times with varying personal relevance.

Often compared to slot machines (Schüll, 2012), smartphone notifications exploit our natural bias for novel, variable rewards (Alter, 2017) in a de facto intermittent reinforcement schedule. With smartphones constantly on hand, it can feel as though we live in digital Skinner boxes in an era of omnipresent, personalized, social, and artificially-intelligent stimuli. Buoyed by foundational research on attention (Pashler, 1999), a developing body of empirical work suggests that digital interruptions can harm mental health and psychological well-being (Kushlev, Proulx, & Dunn, 2016; Mark, Gudith, & Klocke, 2008; Pielot & Rello, 2017; Stothart, Mitchum, & Yehnert, 2015). We set out to experimentally investigate whether batching notifications – delivering them in predictable schedules – could help.

1. Theory and application: variably interrupting attention

The present investigation finds its roots in psychological theory and basic research documenting the cost of task-switching on cognition. Foundational theory holds that people have limited cognitive resources (Navon & Gopher, 1979; Pashler, 1999). According to the time-based resource sharing model of attention (Barrouillet, Bernardin, & Camos, 2004), the very act of switching between tasks, even very briefly, requires additional mental effort, thereby increasing cognitive load (Liefooghe, Barrouillet, Vandierendonck, & Camos, 2008). And the load theory of attention (Lavie, 2010) maintains that higher cognitive load makes people more susceptible to novel distracting stimuli (Lavie, Hirst, De Fockert, & Viding, 2004; Ryff & Keyes, 1995). In addition, classic behavioral research finds that stimuli delivered in a variable-interval schedule are particularly effective at reinforcing repeated behavior (Skinner & Ferster, 2014). By randomly interrupting people in an ongoing activity, smartphone notifications may increase attention residue and cognitive load, which in turn may make people even more prone to task-switching (Leroy, 2009).

This cycle of inattention can compromise emotional well-being by disrupting emotion regulation (Posner & Rothbart, 2007; Zijlstra, Roe, Leonora, & Krediet, 1999) and task performance (Mark et al., 2008; Rubinstein, Meyer, & Evans, 2001). Furthermore, people who are distracted are less likely to savor and thus reap the emotional benefits of positive experiences—more so than people who find faults with events and even those actively trying to suppress positive emotions (Quoidbach, Berry, Hansenne, & Mikolajczak, 2010). On the flip side, attention and concentration have been theorized and shown to be essential prerequisites for experiencing the optimal psychological experience of flow—a state of absorption, engagement, fulfillment, and skill that has been associated with experiencing positive emotions (Csikszentmihalyi, 1990). Relatedly, research on mindfulness and mindful attention shows that directing attention to the present moment predicts greater intensity and frequency of positive emotions (Erisman & Roemer, 2010).

From basic to applied research, a body of empirical work in human-computer interaction has documented the cost of digital interruptions (Adler & Benbunan-Fich, 2012; Baethge & Rigotti, 2013; Kushlev & Dunn, 2015; Mark, Voida, & Cardello, 2012). Though much of the existing research is limited to within-subjects observations in small samples (Duke & Montag, 2017; Mark et al., 2012; Pielot & Rello, 2015; Shirazi et al., 2014), results suggest that experiencing digital alerts in regular batches may help alleviate their costs, while preserving their benefits. In one lab experiment, participants experienced more time pressure, expended more mental effort, and felt more frustrated when they were interrupted by notifications at random intervals while completing standard work tasks (e.g., editing text) than when uninterrupted. Participants who were interrupted only between tasks, however, avoided these negative outcomes (Adamczyk & Bailey, 2004). Thus, the timing of digital interruptions, not only their frequency, may be an important factor. Looking beyond notifications, preliminary evidence suggests that even the mere presence of one's smartphone may impair cognitive capacity (Ward, Duke, Gneezy, & Bos, 2017).

People receive dozens of smartphone notifications throughout the day, often attending to them immediately (Chang & Tang, 2015), impairing attention and leading to a wide range of deleterious consequences (Kushlev, 2018; Kushlev et al., 2016). In a recent report in which 30 volunteers turned off notifications for a day, however, researchers concluded that the alerts have “locked us in a dilemma: Without notifications, participants felt less distracted and more productive. But, they also felt no longer able to be as responsive as expected, which made some participants anxious” (Pielot & Rello, 2017). Can delivering notifications in predictable batches help alleviate the deleterious effects of pervasive alerts while preserving their benefits?

2. Present research

To examine whether batching notifications can improve well-being, we conducted a two-week field trial. Using a custom-designed smartphone app that manages notifications, we randomly assigned participants to either receive their notifications as usual, batched in predictable intervals, or never.[2] We tested two different predictable patterns of batching: three times a day and once every hour. We were thus able to examine the effects of batching compared to default, random notification delivery and no notification delivery at all. Based on past research, we can predict that batching should decrease inattention and ultimately increase well-being compared to default delivery. We can also predict that while receiving no notifications at all may yield some of the same benefits, these benefits may be offset by the anxiety associated with missing important updates.

We captured psychological outcomes and measures of phone use with daily surveys delivered through the phone. We adopted a broad exploratory approach to measurement, capturing a wide range of constructs that describe people's daily subjective experiences, such as mood, stress, productivity, and social connectedness. Given the relative dearth of evidence-based on well-powered field experiments, we hoped that this exploratory measurement approach would allow us to identify the benefits and costs of batching, compared to default delivery and no notification delivery at all (see Table 1 for a complete list of outcomes).

3. Methods

3.1. Sample

Participants (N = 237) were recruited in March 2017 via Amazon's Mechanical Turk (MTurk), an online labor market commonly used for research in the social, health, and behavioral sciences (Berinsky, Huber, & Lenz, 2012; Buhrmester, Kwang, & Gosling, 2011; Crump, McDonnell, & Gureckis, 2013; Paolacci & Chandler, 2014). We set out to recruit at least 50 participants per condition, thus aiming to be able to detect omnibus effects of our primary between-subjects comparisons of medium size, Cohen's $f = 0.24$ (Cohen, 1988); sensitivity analyses using G*Power 3.1 indicated that our final sample size of 237 participants allowed us to detect omnibus condition effects of similar magnitude, Cohen's $f = 0.22$, with 80% power.

Specifically, we recruited a sample of smartphone owners residing in India ($M_{age} = 30.3$; Sex: 19% female; Employment: 85% employed

[2] The application was designed specifically for the purposes of the study by Synapse Inc, represented here by the third through fifth authors. The application is not commercially available.

Table 1
Measures and descriptive statistics for study outcomes.

CATEGORY	CONSTRUCT	SCHEDULE	SOURCE	#ITEMS	ITEMS DETAILS	RESPONSE SCALE	Α
Well-being	Mood	Daily	–	1	*Right now, I am feeling …*	1-Bad; 7-Good	
	Positive Affect (Happiness)	Weekly	Scale of Positive and Negative Affect (SPANE)	6	All items	1-Very rarely or never 5-Very often or always	.91
	Negative Affect	Weekly	Scale of Positive and Negative Affect (SPANE)	6	All items	1-Very rarely or never 5-Very often or always	.91
	Work Enjoyment	Daily	–	1	*Today, I enjoyed work.*	1-Not at all 7-Very much	
	Stress	Daily	–	1	*Today, I felt stressed.*	1-Not at all 7-Very much	
		Weekly	Perceived Stress Scale (PSS)	5	Items used by Kushlev & Dunn (2015)	1-Strongly disagree 7-Strongly agree	.87
	Anxiety	Daily	–	1	*Today, I felt anxious.*	1-Not at all 7-Very much	
		Weekly	State-Trait Anxiety Inventory (STAI)	6	Six item version validated by Marteau and Bekker (1992)	1-Not at all 4-Very much	.89
	Social Connectedness	Daily	–	1	*Today, I felt connected to other people.*	1-Not at all 7-Very much	
		Weekly	Social Connectedness Scale (SCS)	5	State version used by Kushlev et al. (2017) seems to have 3 authors (if I found the correct article), so not sure if et al. is correct	1-Not at all 7-Very much	.89
..	Depression	Weekly	C. for Epid. Studies Depression (CES-D-10)	10	Adapted to measure state levels (i.e., today)	1-rarely or none of the time 4-All of the time	.87
	State Mindfulness	Weekly	Mindful Attention Awareness Scale (MAAS-5)	5	All items	1-Not at all 7-Very much	.91
	Perceived Productivity	Daily	–	1	*Today, I was productive.*	1-Not at all 7-Very much	
		Weekly	–	3	Face-valid scale by Kushlev and Dunn (2015)	1-Not at all 5-Very much	.84
Attention	Concentration	Daily	–	1	*Today, it was easy for me to concentrate on what I was doing.*	1-Not at all 7-Very much	
	Distraction	Daily	–	1	*Today, I felt distracted.*	1-Not at all 7-Very much	
	Inattention	Weekly	Adult ADHD Self-Report Scale (ASRS-Part A)	6	Items from WHO short version adaptable for measuring daily levels	1-Never 5-Very Often	.88
Fear of missing out	Notifications FoMO	Daily	–	1	*Today, I felt I was missing out on important notifications.*	1-Not at all 7-Very much	
	Phone FoMO	Weekly	C-FoMO-Scale	9	Chosen to measure phone-related FoMO	1-Strongly disagree 7-Strongly agree	.95
	General FoMO	Weekly	Fear of Missing Out Scale (FoMOs)	6	Items adaptable for measuring daily levels	1-Not at all true for me 5-extremely true for me	.88
Phone outcomes	Interrupted by Notifications	Daily	–	1	*Today, I felt I was interrupted by notifications.*	1-Not at all 7-Very much	
	Phone Control	Daily	–	1	*Today, I felt like …*	1-My phone was in control of me 7-I was in control of my phone	
	Intentional Checking	Daily	–	1	*Today, I checked my phone …*	1-For no reason 7-For specific reasons	
	Social Pressure to Respond	Daily		1	*Today, I felt pressure to immediately respond to people on my phone*	1-Not at all 7-Very much	
	Phone Overuse	Daily	–	1	*Today, I used my phone more than I wanted to.*	1-Not at all 7-Very much	
	Phone Addiction	Weekly	Smartphone Addiction Scale (SAS)	10	Items adaptable for measuring daily levels	1-Strongly disagree 7-Strongly agree	.85
	Nomophobia	Weekly	Nomophobia Questionnaire (NMP-Q)	14	Items adaptable for measuring daily levels	1-Strongly disagree 7-Strongly agree	.86

part or full time, 6% actively looking for employment, 7% students; Occupation: 42% information technology, 21% management, finance, & insurance, 15% healthcare & education; 13% manufacturing). About two-thirds reported receiving notifications (68%) and checking their phones (64%) at least 'a few times an hour', while 4 in 5 admitted to checking notifications in the middle of a conversation at least 'sometimes' (82%) and 'often' needing to have their phones 'within reach and immediately available' (77%).[3]

[3] Additionally, 96 participants completed the onboarding survey, but not continue to participate in the study. Further, though recruitment through MTurk afforded us the ability to collect a large sample for statistically meaningful analyses, this online approach to recruitment can also yield non-compliance, especially in the context of a longitudinal study with daily

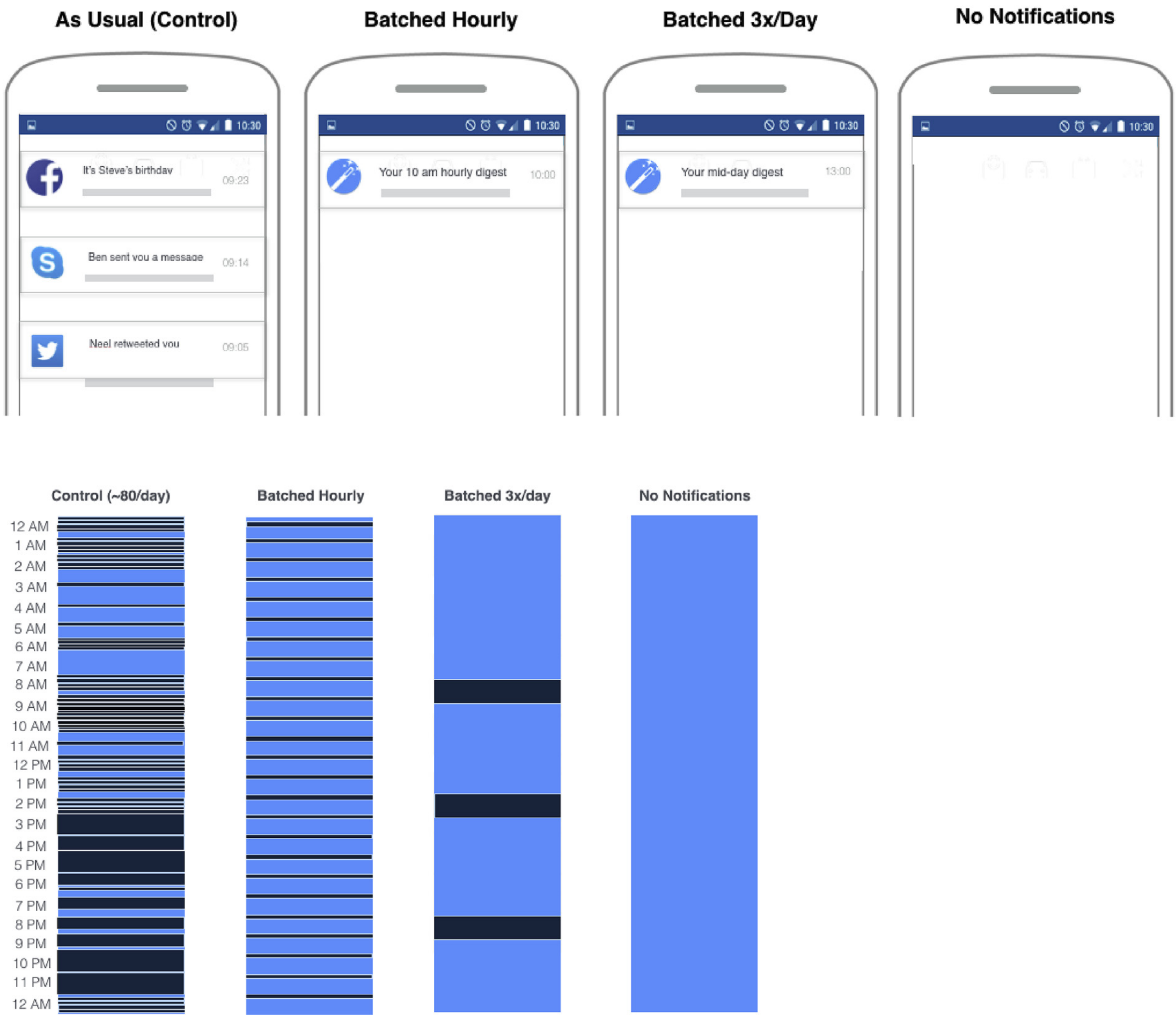


Fig. 1. Examples of participants' notification delivery in each condition.

Participants were compensated $2 to sign up (e.g., download our app and take the baseline survey), and $1 for each completed survey during the study. As an extra incentive, participants who completed the study—a priori defined and explained to participants as completing three-fourths of the daily surveys—were entered into a raffle to win $500. The Duke University IRB approved the study (#E0066).

(*footnote continued*)
measures. Thus, we analyzed only data from participants who completed the study, a priori defined and explained to participants as completing three-fourths of the surveys. An additional 108 (31%) participants did not complete the study by this indicator. Importantly, participants in the treatment groups were as likely to complete the study as those in the control group (p = .67), suggesting that the exclusion did not compromise random assignment. We saw no notable differences in the demographics between the included sample and total sample (M_{age} = 29.8, 19% female; Employment: 84% employed part or full time, 6% actively looking for employment, 8% students; Occupation: 42% information technology, 21% management, finance, & insurance, 13% healthcare & education; 13% manufacturing). Additionally, the total sample was similar to the included sample in phone-related behaviors: Again, about two-thirds reported receiving notifications (68%) and checking their phones (66%) at least ‘a few times an hour’, and 4 in 5 ‘often’ needing to have their phones ‘within reach and immediately available’ (80%). Slightly more people in the total sample admitted to checking notifications in the middle of a conversation at least ‘sometimes’ (90%).

3.2. Design

To carry out this work, we developed an Android application that batched participants’ notifications and delivered measures of psychological outcomes. We managed the study with TurkPrime (Litman, Robinson, & Abberbock, 2017), a research suite that interacts with MTurk, to facilitate longitudinal communication and compensation. Participants provided consent, downloaded our app, and took an onboarding trait survey measuring individual differences. In the first week of the study (baseline phase), participants received notifications as they normally do; all participants were simply asked to complete a daily survey containing our measures. This design allowed participants to familiarize themselves with the measures and their schedule of administration. In the second and third week of the study (experimental phase), participants continued to complete the same measures but were randomly assigned to have their notifications received normally (i.e., variably throughout the day), in a batch every hour on the hour, in a batch three-times-a-day (at 9am, 3pm, and 9pm), or not at all (see Fig. 1). In the present work, we focus on differences between conditions during the experimental phase.

Participants in each experimental condition were informed about the change in the schedule of delivery of their notifications immediately prior to the change taking effect. Notably, participants were advised that they could always access any specific notifications by opening the associated app, giving them the flexibility to check for important or urgent notifications whenever necessary. In other words, we only manipulated the delivery of notifications (e.g., to the lock screen), rather than preventing messages from being accessed at all.

3.3. Measures

We included a wide range of measures assessing different aspects of well-being, attention, fear of missing out (FoMO), and various phone-related behaviors. We also triangulated on the strengths of two different approaches of measuring these outcomes: single-item measures administered daily and multi-item scales administered weekly. Regardless of the schedule of administration, all measures were administered in daily diary surveys administered after 7pm local time, asking participants about how they had felt and what they had done "today." We calculated person-level composites for all constructs by averaging these measures across the two weeks of the intervention. Details, reliability, and descriptives of these measures are shown in Table 1. We also assessed perceptions of the intervention post-study.

Single-item measures. The single-item measures assessed a range of psychological outcomes—concentration, distraction, stress, anxiety, mood, productivity, social connectedness, and work enjoyment—as well as phone-specific outcomes: feeling of being interrupted by notifications, sense of missing out on notifications, sense of control over phone, social pressure to respond to others, phone overuse, and intentional phone checking (see Table 1 for details).

Multi-item measures. To obtain more reliable measures of our key outcomes, we adapted items drawn from existing validated scales, selecting items that were adaptable to assess daily (state) levels of each construct. Thus, we measured inattention (Kessler et al., 2005), stress (Cohen, Kamarck, & Mermelstein, 1983), phone-related fear of missing out (Hato, 2013), fear of missing out (Przybylski, Murayama, DeHaan, & Gladwell, 2013), anxiety (Marteau & Bekker, 1992), perceived productivity (Kushlev & Dunn, 2015), depression, positive and negative affect, mindfulness, smartphone addiction (Kwon et al., 2013), nomophobia, and social connectedness (see Table 1 for details). To avoid overburdening participants with those full-scale assessments, we employed a Balanced Latin Squares design (Williams, 1949): Each week, participants had to complete each of those measures only once with the order of administering these measures randomized across seven separate sub-samples. All measures, therefore, can only be interpreted as indicating each person's score for each week (rather than for any particular day of the week).

Other measures. With Harari and colleagues' recommendations in mind (Harari et al., 2016), we also passively measured how frequently people unlocked their phones through the Android operating system. Finally, several weeks after the experimental phase-, we asked participants to reflect on their experiences during the study. After prompting participants to "think about your time during the study," we assessed their perceptions of the benefits they experienced (e.g., feeling more attentive, less stressed, more productive).

4. Results

4.1. Analytic strategy

We submitted all outcomes as measured during the two-week experimental phase to between-subject analyses of variance (ANOVAs) across all four conditions (batched 3x/day, no notifications, hourly, control). Details of all analyses, including effect sizes, are presented in Table 2. The hourly batching condition did not differ from the control, except for a single effect on the daily measure of feeling interrupted by notifications (Table 2). For simplicity, we present results visually for the three other conditions in Fig. 2.

4.2. Omnibus effects

Key mediators: attention and phone-related fear of missing out. We found significant main effect of batching on our key hypothesized mediator, inattention ($F_{3,209} = 5.65$, $p < .001$), and we also found effects on the corresponding single-item measure of concentration ($F_{3,233} = 2.72$, $p = .045$). Interestingly, we found no effect on daily feelings of distraction ($F_{3,233} = 0.41$, $p > .250$). Thus, because being distracted by notifications is now the new normal, it is possible that people in the control condition may have been unable to detect how distracted they were, despite being able to recognize lapses in their concentration on specific tasks.

Though we did not find an omnibus effect on our measure of general FoMO ($F_{3,214} = 1.74$, $p = .151$), we did observe the predicted effect on our more targeted measure of phone-related FoMO ($F_{3,214} = 3.48$, $p = .017$) and its corresponding single-item measure: sense of missing important notifications ($F_{3,233} = 4.59$, $p < .01$). Overall, then, we see evidence for the hypothesized mediators of both the positive and negative effects of batching on well-being (see Table 2).

Primary outcomes. Turning to our key indicators of well-being, we detected omnibus effects on psychological outcomes across both the single items—stress ($F_{3,233} = 2.78$, $p < .05$), and perceived productivity ($F_{3,233} = 2.74$, $p < .05$), as well as a marginal effect on anxiety ($F_{3,233} = 2.14$, $p = .095$)—and their corresponding multi-item scales: stress ($F_{3,215} = 3.62$, $p < .05$), anxiety ($F_{3,211} = 3.19$, $p = .025$), and perceived productivity ($F_{3,216} = 3.18$, $p < .05$). Though we found an omnibus effect of condition on mood ($F_{3,233} = 2.95$, $p < .05$), we only found evidence for a marginal effect on negative affect ($F_{3,213} = 2.56$, $p = .056$), but not on positive affect ($F_{3,213} = 0.56$, $p > .250$). This pattern suggests that notifications may be causing increases in negative indicators of well-being rather than decreasing positive indicators. In support of this possibility, we also did not find a beneficial effect of batching on enjoyment of work (see Table 2). Conversely, we also did not find any evidence for a detrimental effect of batching on lowering sense of social connection with others. Finally, similar to the general measure of FoMO, our brief two-week batching intervention had no effects on relatively stable indicators of well-being like depression or on relatively stable metacognitive skills like mindfulness (*p*s $> .25$; see Table 2 for details).

Phone-specific feelings and behaviors. Our intervention had overall effects on the self-reported feelings and perceptions of phone use: feeling interrupted by notifications throughout the day ($F_{3,233} = 5.35$, $p < .001$), and control over phone ($F_{3,233} = 2.75$, $p < .05$), with marginal effects on how intentionally people used their phones ($F_{3,233} = 2.20$, $p < .09$) and the objectively-measured frequency of phone unlocks ($F_{3,233} = 2.52$, $p = .059$). Despite these effects, we did not find effects of condition on perceived phone overuse or on phone addiction (*p*s $> .250$). Consistent with psychological theory on task switching, these findings suggest that the observed effects of the intervention on well-being may be due to decreases in phone interruption and checking rather than the total time of phone use. We did not detect omnibus effects on phone-related social pressure ($p > .250$), reducing the possibility that batching may have made participants feel pressured by others to respond sooner. Similarly, there was no effect on nomophobia ($p > .250$), suggesting that batching likely does not alter people's perceptions of the availability of their phones (see Table 2 for details).

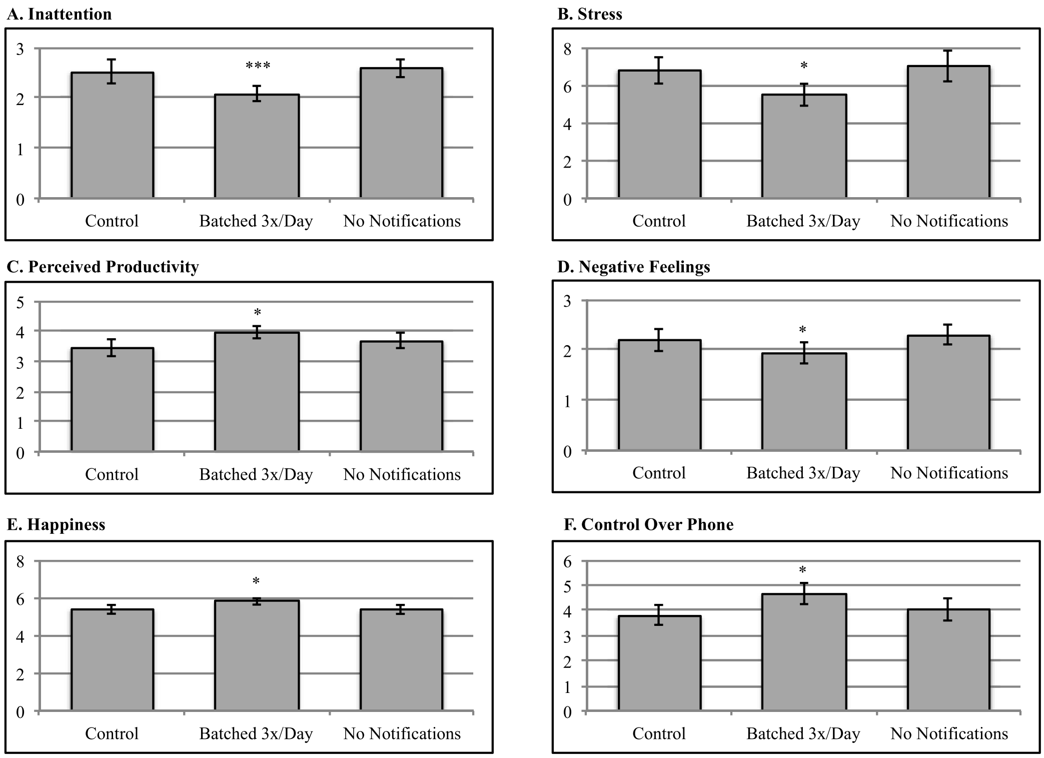
Thinking back: People do not perceive efficacy yet do want to download an app. Participants did not report any awareness of the effects we observed across outcomes, including inattention, FoMO, stress, mood, productivity, and so forth. These findings suggest that the effects we observed are unlikely due to participant beliefs about the effects of our treatments.

Planned contrasts. We next conducted planned-contrasts analyses to probe the omnibus effect of condition. We focus on describing the substantive differences of the control condition with batching 3x/day

Table 2
Omnibus effects of condition and unadjusted pairwise comparisons of each treatment with control (default notification settings).

	Measure Type	Effects (vs. control)				Omnibus Test				Batched 3x/Day		Batched Hourly		No Batching (No Notif.)		No Batching (Control)	
		Batching 3x daily		No Batch (No Notif.)		F	η^2	p	N	$M^{(SD)}$	d	$M^{(SD)}$	d	$M^{(SD)}$	d	M	SD
Inter. by Notif.	Single	Benef.	↓	Benef.	↓	5.35^{***}	0.06	< .001	237	$2.32^{(1.18)*}$	−0.48	$2.39^{(1.09)*}$	−0.43	$2.07^{(1.05)***}$	−0.74	2.86	1.09
Phone Control	Single	Benef.	↑		–	2.75^{*}	0.03	.043	237	$4.65^{(1.46)**}$	0.58	$4.02^{(1.78)}$	0.14	$4.03^{(1.78)}$	0.15	3.79	1.53
Unlocks	Passive	Benef.	↓		–	$2.52^{\dagger}$	0.03	.059	237	$37.2^{(18.2)**}$	−0.60	$50.0^{(53.5)}$	−0.15	$46.04^{(30.7)}$	−0.30	57.3	43.0
Inattention	Scale	Benef.	↓		–	5.65^{***}	0.08	< .001	213	$2.08^{(0.49)**}$	−0.65	$2.40^{(0.70)}$	−0.16	$2.59^{(0.65)}$	0.10	2.52	0.81
Concentration	Single	Benef.	↑		–	2.72^{*}	0.03	.045	237	$5.18^{(1.05)**}$	0.54	$4.86^{(1.07)}$	0.27	$4.83^{(1.20)}$	0.23	4.55	1.25
Mood	Single	Benef.	↑		–	2.95^{*}	0.04	.033	237	$5.83^{(0.68)*}$	0.49	$5.58^{(0.98)}$	0.17	$5.35^{(1.02)}$	−0.07	5.42	0.99
Negative Affect	Scale	Benef.	↓		–	$2.56^{\dagger}$	0.04	.056	217	$1.93^{(0.71)\dagger}$	−0.34	$2.03^{(0.71)}$	−0.21	$2.30^{(0.80)}$	0.14	2.19	0.80
Stress	Scale	Benef.	↓		–	3.62^{*}	0.05	.014	219	$5.49^{(2.07)*}$	−0.56	$6.53^{(2.20)}$	−0.12	$7.02^{(3.15)}$	0.07	6.82	2.60
	Single	Benef.	↓		–	2.78^{*}	0.03	.042	237	$2.42^{(0.79)*}$	−0.40	$2.64^{(0.97)}$	−0.17	$2.94^{(1.14)}$	0.12	2.81	1.08
Perc. Prod.	Scale	Benef.	↓		–	3.18^{*}	0.04	.025	220	$3.97^{(0.70)**}$	0.58	$3.61^{(0.75)}$	0.18	$3.68^{(0.95)}$	0.23	3.45	1.04
	Single	Benef.	↑		–	2.74^{*}	0.03	.044	237	$4.94^{(0.90)**}$	0.52	$4.71^{(1.18)\dagger}$	0.28	$4.50^{(1.15)}$	0.10	4.37	1.26
FoMO (Phone)	Scale	Detrim.	↑	Detrim.	↑	3.48^{*}	0.05	.017	218	$3.38^{(1.27)*}$	0.43	$3.25^{(1.31)}$	0.32	$3.64^{(1.40)**}$	0.60	2.85	1.22
FoMO (Notif.)	Single	Detrim.	↑	Detrim.	↑	4.59^{**}	0.06	.004	237	$3.62^{(1.41)**}$	0.68	$3.23^{(1.26)\dagger}$	0.40	$3.47^{(1.52)**}$	0.53	2.74	1.15
Anxiety	Scale		–	Detrim.	↑	3.19^{*}	0.04	.025	215	$4.14^{(1.05)}$	0.17	$4.16^{(1.05)}$	0.19	$4.57^{(1.00)**}$	0.56	3.95	1.17
	Single		–	Detrim.	↑	$2.14^{\dagger}$	0.03	.095	237	$2.89^{(1.10)}$	0.19	$2.75^{(1.11)}$	0.06	$3.17^{(1.25)**}$	0.41	2.68	1.10
Intent. Checking	Single		–	Detrim.	↑	$2.20^{\dagger}$	0.03	.089	237	$4.78^{(1.13)}$	0.12	$4.82^{(1.24)}$	0.15	$5.19^{(1.29)**}$	0.43	4.63	1.30
Phone Addict.	Scale		–		–	0.65	0.01	.581	205	$2.85^{(1.20)}$	−0.05	$2.79^{(1.18)}$	−0.10	$3.09^{(1.22)}$	0.16	2.90	1.12
Nomophobia	Scale		–		–	0.80	0.01	.493	212	$4.85^{(1.65)}$	0.07	$4.88^{(1.66)}$	0.09	$5.22^{(1.89)}$	0.27	4.74	1.68
Phone Overuse	Single		–		–	0.09	0.00	.966	237	$3.51^{(1.28)}$	0.08	$3.40^{(1.33)}$	−0.01	$3.46^{(1.38)}$	0.04	3.41	1.19
Social Pressure	Single		–		–	0.57	0.01	.636	237	$3.27^{(1.19)}$	−0.07	$3.07^{(1.29)}$	−0.23	$3.25^{(1.32)}$	−0.09	3.36	1.25
Distraction	Single		–		–	0.41	0.01	.749	237	$2.61^{(1.00)}$	−0.06	$2.61^{(1.01)}$	−0.06	$2.79^{(1.19)}$	0.11	2.67	1.02
State Mindful.	Scale		–		–	0.14	0.00	.935	206	$2.97^{(1.25)}$	0.06	$2.92^{(1.29)}$	0.02	$3.04^{(1.17)}$	0.12	2.90	1.18
Positive Affect	Scale		–		–	0.56	0.01	.642	217	$2.27^{(0.38)}$	0.18	$2.20^{(0.44)}$	0.02	$2.16^{(0.46)}$	−0.06	2.19	0.49
Work Enjoy.	Single		–		–	0.22	0.00	.883	237	$4.90^{(0.99)}$	1.00	$4.86^{(1.06)}$	0.06	$4.75^{(1.16)}$	−0.03	4.79	1.24
Connectedness	Scale		–		–	0.67	0.01	.572	217	$1.28^{(0.18)}$	−0.04	$1.09^{(0.14)}$	0.10	$1.40^{(0.18)}$	0.20	1.18	0.16
	Single		–		–	0.75	0.01	.523	237	$4.64^{(1.06)}$	0.22	$4.39^{(1.03)}$	0.00	$4.35^{(1.21)}$	−0.03	4.39	1.21
Depression	Scale		–		–	0.81	0.01	.490	212	$9.35^{(5.33)}$	−0.30	$9.74^{(5.20)}$	−0.23	$10.26^{(5.89)}$	0.13	10.98	5.54
FoMO (General)	Scale		–		–	1.78	0.03	.151	217	$2.39^{(0.82)}$	0.00	$2.44^{(0.82)}$	0.05	$2.70^{(0.89)\dagger}$	0.37	2.39	0.77

Note. ↑ = a relative increase in outcome in treatment compared to control; ↓ = a relative increase in outcome in treatment compared to control; – = no difference between treatment and control. Cohen's ds represent the difference between each condition and control. Test of significance are not adjusted for multiple comparisons. †$p < .10$ $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.



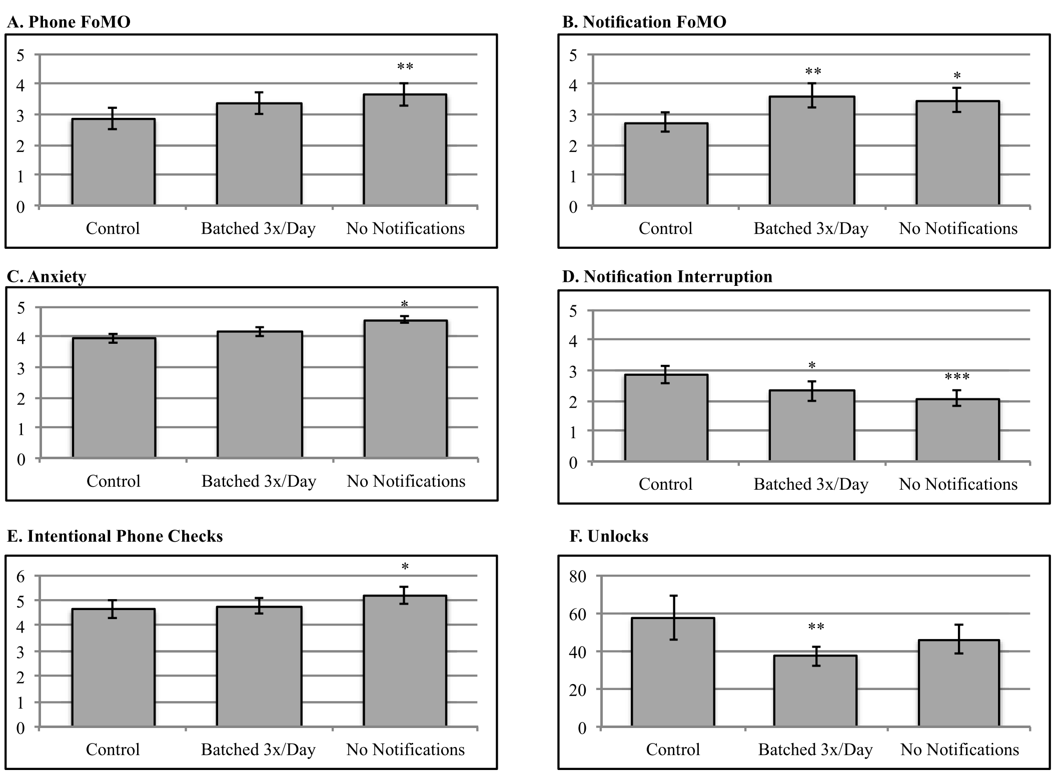


Fig. 2. *The effects of Batching 3x/Day and No Notifications on main effects. Note. Error bars represent 95% CIs, *p < .05, **p < .01, ***p < 001.*

and no delivery conditions, which we defined as effects of at least medium size, Cohen's d ≥ 0.50. Table 2 presents the means and standard deviations for each condition, along with the effect sizes of the comparisons.

Batching 3x/day improves well-being. Compared to those in the control group, participants whose notifications were batched 3x/day unlocked their phones fewer times (d = −0.60) and felt more control over their phone (d = 0.58; see Table 2). Accordingly, participants whose notifications were batched three times a day also experienced lower inattention (d = −0.65) and higher concentration (d = 0.54) as compared to controls. At the same time, these participants experienced more and fear of missing out on notifications (d = 0.68) compared to controls. Despite this effect, however, participants who received their notifications in three daily batches also reported improved well-being, compared to controls, reporting marginally better mood (d = 0.49), less negative affect (d = −0.49) and greater stress (d = 0.56). We observed little difference on anxiety (d = 0.17), however, suggesting that the omnibus effect may have been driven by another experimental condition (see Table 2). Participants with notifications delivered in three daily batches also perceived themselves as more productive at the end of the day (d = 0.57).

Never receiving notifications backfires. In contrast to batching notifications three times daily, delivering no notifications at all either had no benefits or produced negative effects (Table 2). Like those in the three-times-a-day condition, participants not receiving any notifications felt less interrupted by notifications (d = −0.74) than controls. But these participants did not feel a greater sense of control over their phone use and, perhaps surprisingly, did not experience a reduction in inattention or an improvement in concentration compared to controls (Table 2). Participants without notifications did, however, experience higher levels of phone-related fear of missing out (d = 0.59) and feelings of missing out on important notifications (d = 0.53) than controls. They also experienced significantly more anxiety than controls (d = 0.56). Thus, the omnibus effect on anxiety was driven by increased anxiety due to receiving no notifications at all (Fig. 2).

Mechanisms: Inattention helps explain the benefits of batching. We next tested our theorizing that inattention might explain the positive effects of receiving notifications batched three times a day, and whether phone-related fear of missing out explains the negative effects of not receiving any notifications. Though we observed corresponding effects on the single-item equivalents of those mediators (e.g., concentration and missing out on notifications), we report the multi-item indicators as they were based on previously validated scales. Similarly, we focus on the effects of the multi-item outcomes. We conducted the mediation analyses with the Hayes PROCESS macro (Hayes, 2013) using 10,000 bootstrap replications.

Inattention[4] fully mediated the effects of batching three-times-a-day (compared to control) on feeling less stressed, Indirect effect (I.E.) = −0.54, 95%CI[-1.12, −0.19], more productive, I.E. = 0.20, 95%CI[0.07, 0.43], and on experiencing a better mood, I.E. = 0.11, 95%CI[0.01, 0.28], and less negative affect, I.E. = -0.20, 95%CI[-0.38, −0.08], leaving non-significant direct effects (see Fig. 3). Moreover, following contemporary mediation guidelines on outcomes without main effects (Zhao, Lynch, & Chen, 2010), we found that to the extent that participants experienced decreased inattention in the batched three-times-a-day (vs. control) condition, batching had beneficial indirect effects on mindfulness (I.E. = 0.44, $p < .01$), social connectedness (I.E. = 0.41, $p < .01$), depression (I.E. = -1.79, $p < .01$) and smartphone addiction (I.E. = -0.46, $p < .01$). In sum, the psychological benefits from batching notifications 3x/day can be partially explained by the reduction in inattention. Finally, as shown in Fig. 4, the higher anxiety in participants who never received notifications (compared to controls) was fully explained by those participants' higher phone-related fears of missing out, I.E. = 0.27, 95%[0.09, 0.53].

Of course, despite the experimental manipulation, we cannot infer causality based on the correlational relationships between mediator and outcome in any models shown in Figs. 3 and 4. Notably, however, reversing mediators and outcomes in the models showed that the condition effects on inattention and phone FoMO could not be fully explained through any of the theorized outcomes (e.g., stress). In other words, the direct effects of condition on both inattention and phone FoMO remained significant, $ps < .05$, after accounting for the indirect effects of condition on attention and FoMO through the theorized outcomes.

Mind the belief-reality-ideal gap: People receive more notifications than they realize. Are people aware of how many notifications they receive? And would they like to receive fewer of these alerts? In the recall survey, we asked these questions about participants' beliefs and ideals, and compared them to how many notifications they actually received (recorded by the batching application). We found that people want to receive about half the number of notifications that they think they receive, and they dramatically underestimate how many notifications they actually receive (see Fig. 5). Though extended analysis of why these gaps exist is beyond the scope of this paper, they may derive from both a general unawareness of automatic daily influences (Bargh, 2002) and a particular unawareness of how and why devices are incentivized to capture their attention (Wu, 2017).

5. Discussion

In the present work, using a custom-built Android application, we modified users' default notification systems. Building on recent empirical work and classical theory, we conducted a field experiment to explore whether batching notifications – delivering them at predictable intervals – could improve users' well-being compared to a control condition: receiving notifications in an endless, variable stream. We find evidence that delivering notifications in three batches a day—but not in hourly batches—improved well-being outcomes such as stress, productivity, and mood. These effects were mediated by the effects of batching on reducing inattention. In contrast, never delivering notifications did not confer the same benefits, producing instead an increase in anxiety mediated by fear of missing out. Thus, batching in a few daily intervals can preserve the usefulness of being notified, while mitigating the harm of variable, unpredictable delivery of notifications.

Interestingly, people in the no-notifications condition did not show improvements in inattention. These findings suggest that any attentional benefits from reducing notification related interruptions may have been nullified by increased cognitive load associated with the anxiety of missing something important. These results may seem inconsistent with previous findings that demonstrated *increased* attention with the silencing of notifications (Kushlev et al., 2016). These findings were based on user-implemented instructions, which participants could override at any time as needed (by simply switching back to ring mode). In contrast, our participants' phones were controlled by an app, which did not allow participants to modify settings. The restrictiveness of the modifications in this study potentially contributed to the increase in cognitive load, as participants experienced anxiety about neglecting notifications. These differences between current and past research highlight the dangers of paternalistic interventions and underscore the importance of granting personal control over phone settings, accommodating the vagaries of daily life. Notably, however, batching notifications three times a day produced larger effects on well-being than observed by Kushlev and colleagues' (2016) user-controlled, do-not-disturb manipulation.

5.1. Limitations

There are important limitations to the present work. First, the treatment lasted for only two weeks. When changing any environmental stimulus, especially one that occurs frequently, there can be transition costs. Participants may take time to adapt to significant behavioral changes (Prochaska & Velicer, 1997), and some effects may emerge after a longer period of time. We need large, longitudinal trials of interventions that promote living well with ubiquitous technologies. Second, we conducted the study through the smartphone but did not capture reliable data on usage, only scratching the surface regarding potential behavioral insights. Third, we prototyped a simple, blanket batching of user notifications, but not all notifications are equally important. Some are more relevant than others at different times and places for different people. The design of future interventions should integrate recent work on personalized, context-aware systems (Kanjo, Kuss, & Ang, 2017; Mehrotra & Musolesi, 2017; Oh, Jalali, & Jain, 2015).

[4] For the mediation analyses, inattention was reversed-scored for a more intuitive interpretation of the coefficient signs.

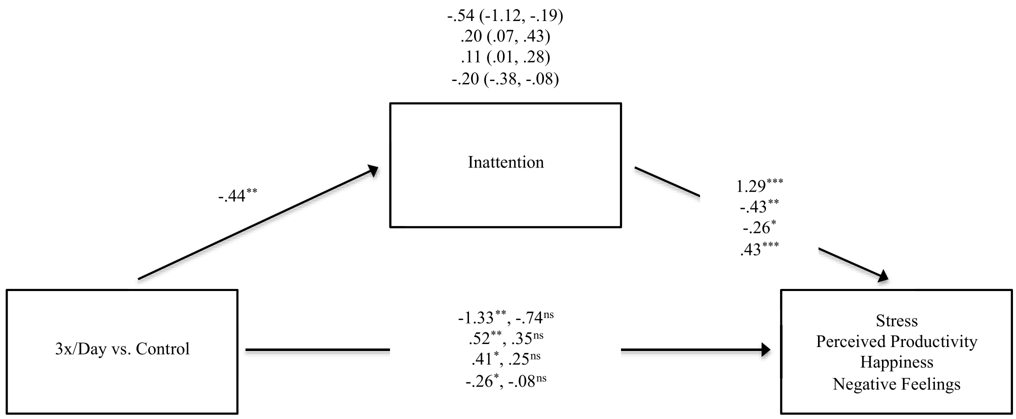


Fig. 3. The indirect effects of batching three-times-a-day on several outcomes through inattention. All bs represent unstandardized regression coefficients. The 95% confidence interval of the indirect effect is shown in parentheses and is obtained through bootstrapping using 10,000 resamples. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$. Direct effects are shown after the total effects, separated by a comma.

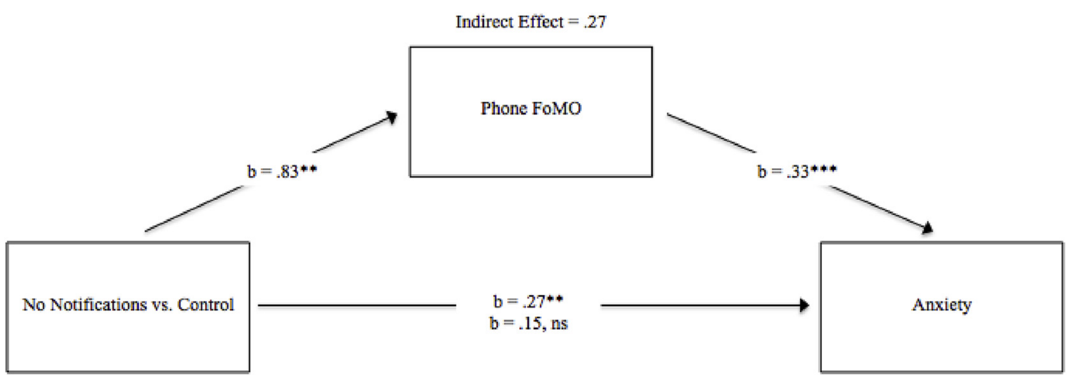


Fig. 4. The effects of never receiving notifications on anxiety through phone FoMO. All bs represent unstandardized regression coefficients. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$.

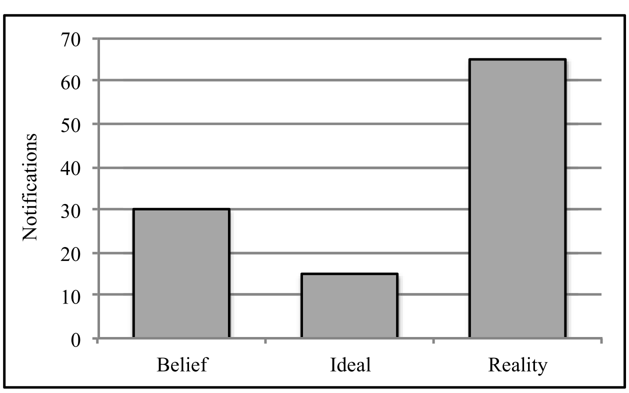


Fig. 5. How many notifications participants think they receive, want to receive, and actually receive.

Fourth, we recruited a sample of disparate individuals whose participation may have put them out of sync with their social environment. We manipulated participants' experience but had no control over the expectations of those around them. Given the power of social norms and expectations (Gillespie, Walsh, Winefield, Dua, & Stough, 2001), future work might test interventions delivered on the organizational or team level. Fifth, it is critical to underscore that despite the experimental manipulation, our mediation models—in particular their mediator-to-outcome paths—are correlational. Our findings cannot, therefore, be used to infer the causal role of attention or FoMO in mediating the observed condition effects on well-being or other outcomes. Focusing on the key mediators and outcomes identified in the current study, future research could deploy multiple brief surveys per day to allow conclusions about the effects of preceding attention state on subsequent well-being.

Finally, our results are based on a sample of participants in India, raising questions about the generalizability of the results. Of course, the majority of research we used to theorize about the effects of our intervention is based on Western samples, making it more, not less likely that our findings would be applicable to Western populations. In fact, our reliance on a non-Western, non-student sample can also be seen as a strength given the overreliance of psychology research on student samples from Western countries (Henrich, Heine, & Norenzayan, 2010).

5.2. Implications

Consistent with our findings, recent correlational data from the World Bank suggests that smartphones may play some role in harming productivity growth (Nixon, 2017). Technology companies, however, often base their design decisions on economic incentives rather than user wellness or societal benefits. Companies are obligated to maximize profits for their shareholders, and money is made by increasing engagement and time on screen. To mitigate this, app developers can build add-in tools designed for well-being (Desmet & Pohlmeyer, 2013; IJsselsteijn, De Kort, Midden, Eggen, & Van Den Hoven, 2006), such as the one in the present work. Given the de facto market monopoly in place today (Taplin, 2017), another available option is to advocate for the passage of thoughtful regulation—at the national, state, or organizational level. If we are to ensure healthy norms and behaviors around ubiquitous technology, we will need to actively encourage research, policy, and design that promotes well-being (Fitz & Reiner, 2016).

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.chb.2019.07.016.

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