

NO MORE FOMO: LIMITING SOCIAL MEDIA DECREASES LONELINESS AND DEPRESSION

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Introduction: Given the breadth of correlational research linking social media use to worse well-being, we undertook an experimental study to investigate the potential causal role that social media plays in this relationship. **Method:** After a week of baseline monitoring, 143 undergraduates at the University of Pennsylvania were randomly assigned to either limit Facebook, Instagram and Snapchat use to 10 minutes, per platform, per day, or to use social media as usual for three weeks. **Results:** The limited use group showed significant reductions in loneliness and depression over three weeks compared to the control group. Both groups showed significant decreases in anxiety and fear of missing out over baseline, suggesting a benefit of increased self-monitoring. **Discussion:** Our findings strongly suggest that limiting social media use to approximately 30 minutes per day may lead to significant improvement in well-being.

Keywords: social media; social networking sites; Facebook; Snapchat; Instagram; well-being; depression; loneliness

Social Networking Sites (SNS) have become a ubiquitous part of the lives of young adults. As of March of 2018, 68% of adults in the United States had a Facebook account, and 75% of these people reported using Facebook on a daily basis. Moreover, 78% of young adults (ages 18–24) used Snapchat, while 71% of young adults used Instagram (Smith & Anderson, 2018). Widespread adoption of social media has prompted a flurry of correlational studies on the relationship between social media use and mental

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health. Self-reported Facebook and Instagram usage have been found to correlate positively with symptoms of depression, both directly and indirectly (Donnelly & Kuss, 2016; Lup, Trub, & Rosenthal, 2015; Rosen, Whaling, Rab, Carrier, & Cheever, 2013; Tandoc, Ferrucci, & Duffy, 2015;). Higher usage of Facebook has been found to be associated with lower self-esteem cross-sectionally (Kalpidou, Costin, & Morris, 2011) as well as greater loneliness (Song et al., 2014). Higher usage of Instagram is correlated with body image issues (Tiggemann & Slater, 2013).

In a large population based study, Twenge and colleagues (Twenge, Joiner, Rogers, & Martin, 2017) found that time spent on screen activities was significantly correlated with more depressive symptoms and risk for suicide-related outcomes, although the correlations with SNS use specifically were quite small, and only significant for girls. A major limitation of that study was that the data bases used suffered from restricted range in SNS use, with the highest category (almost every day) being endorsed by more than 85% of females in the samples (Daly, 2018). This simply cannot capture differences in use as they occur naturally. Checking Facebook for 5 minutes almost every day is surely different than spending hours a day on SNS platforms.

Two studies have used prospective, naturalistic designs. Using experience sampling, Kross and colleagues (2013) found that Facebook use predicts less satisfaction with life over time. In a two-week diary design, Steers, Wickham, & Acitelli (2014) found that the relationship between Facebook use and depressive symptoms was mediated by social comparisons. Indeed, several studies have demonstrated that social comparison and peer envy often play a major role in these findings (Tandoc et al., 2015; Verduyn et al., 2015).

Thus, there is considerable evidence that SNS use is associated with reductions in well-being. However, the vast majority of work done in this domain has been correlational in design, which does not allow for causal inferences. Two studies (Kross et al., 2013 and Steers et al., 2014) used prospective longitudinal designs, but were not experimental. It is quite possible that more depressed or lonely individuals use SNS more in an attempt to connect with others. Similarly, it is possible that individuals with lower self-esteem or poorer self-image are more prone to engage

in social comparison by spending time on SNS sites. Only experimental studies can address the direction of causality definitively.

In our review of the literature, we were able to find only two experimental studies, both of which examined only Facebook use. The first study found that subjects assigned to passively scroll through Facebook (as opposed to those assigned to actively post and comment) subsequently reported lower levels of well-being and more envy, indicating not only that Facebook impacts mental health but also that the way in which we engage with Facebook matters (Verduyn et al., 2015). It is reasonable to think that the longer one spends on social media, the more one will be engaging with it in a passive way (as opposed to actively posting content, commenting, etc.) In the second study, subjects who were randomly assigned to abstain from Facebook for a week demonstrated improved satisfaction with life and affect (Tromholt, 2016). While this study was a considerable improvement methodologically on prior work, the ecological validity of the study is somewhat suspect. First, the intervention lasted only one week. While it is interesting that subjects showed measurable increases in well-being over this short time, it is unclear whether this would have been sustainable. Second, many users have grown so attached to social media that a long-term intervention requiring complete abstinence would be unrealistic; limiting SNS use seems more likely to be acceptable and sustainable. Third, this study relied upon self-report to measure compliance with study instructions—there was no objective measure of actual time spent on Facebook. Lastly, both of these studies only explored the effects of Facebook usage. While Facebook is the most widely used SNS among adults, many other sites, especially Snapchat and Instagram, attract large numbers of users and play a major role in these users' lives; this is most notably true for young adults.

The current study was designed to be a rigorous, ecologically valid, experimental study of the impact on well-being of limiting (but not eliminating) the use of multiple SNS platforms over an extended period of time. We improve upon prior studies in several ways. First, the study is experimental, allowing for causal inferences to be made. Second, we gathered objective data on actual usage, both during a baseline phase (to account for the

effects of self-monitoring) and during the active intervention phase. Third, we included three major SNS platforms (Facebook, Snapchat, and Instagram). Fourth, we limited usage to 10 minutes per platform per day, as this seems far more realistic than asking people to abstain from SNS use completely. Many organizations, student groups, businesses, and so on rely on social media posts to communicate with members and customers about meeting times, events, etc. It is unrealistic to expect young people to forego this information stream entirely. Finally, we measured well-being at multiple time points, including before and after the initial self-monitoring baseline, at multiple time points throughout the intervention, and at one-month follow-up after the intervention formally ended.

METHODS

PARTICIPANTS

A total of 143 subjects (108 women, 35 men) were recruited from a pool of undergraduates at the University of Pennsylvania, and began the study on a rolling basis. Seventy-two subjects participated in the fall semester, and 71 in the spring. The subject pool consisted of students enrolled in psychology courses for which they could participate in studies to earn course credit. Subjects were required to have Facebook, Instagram, and Snapchat accounts, and to own an iPhone.

MEASURES

Subjective Well-Being Survey

To measure well-being, we used a battery consisting of seven validated scales. Given the lack of experimental research on our topic, we decided to use a wide variety of well-being constructs that have been found to correlate with social media usage. The survey also included a consent form and questions regarding demographic information (age, sex, and race). The scales comprising the subjective well-being survey are listed below.

Social Support. The Interpersonal Support and Evaluation List (ISEL; Cohen & Hoberman, 1983) consists of 20 items scored on a 0–3 scale (definitely false to definitely true). We modified item 8 slightly to make it specific to Philadelphia (If I wanted to go on a trip for a day to Center City, I would have a hard time finding someone to go with me). Items pertain to accessibility of social support and include statements such as “When I feel lonely, there are several people I can talk to” and “If I decide one afternoon that I would like to go to a movie that evening, I could easily find someone to go with me.” The ISEL has good construct validity and good internal consistency with $\alpha = 0.77$ (Cohen, Mermelstein, Kamarck, & Hoberman, 1985).

Fear of Missing Out. The Fear of Missing Out Scale (FoMOs; Przybylski, Murayama, DeHaan, & Gladwell, 2013) is a validated measure of distress related to missing out on social experiences ($\alpha = .87$). It consists of 10 items scored on a scale of 1 (not at all true of me) to 5 (extremely true of me); items include statements such as “I get anxious when I don’t know what my friends are up to,” “Sometimes, I wonder if I spend too much time keeping up with what is going on,” and “I fear others have more rewarding experiences than me.”

Loneliness. The UCLA Loneliness Scale (revised UCLA Loneliness Scale; Russell, Peplau, & Cutrona, 1980) measures perceived social isolation. The original version was revised to include reverse-scored items and consists of 20 items, scored on a scale of 1 (never) to 4 (often). Sample items include statements such as “No one really knows me well,” “My interests and ideas are not shared by those around me,” and “I feel in tune with the people around me” (reverse scored). The scale has good construct validity and internal consistency with $\alpha = 0.94$ (Russell et al., 1980).

Anxiety. The Spielberger State-Trait Anxiety Inventory (STAI-S; Spielberger, Gorsuch, & Lushene, 1970) is a widely used measure of anxiety symptoms. The inventory consists of two instruction sets, which measure state (in-the-moment) and trait (general) anxiety. We only used the state anxiety version, which consists of 20 items such as “I feel worried” and “I feel calm” (reverse scored). Subjects can respond on a scale of 1 (not at all) to 4 (extremely so).

Depression. The Beck Depression Inventory (BDI-II; Beck, Steer, & Brown, 1996) is a standard clinical measure of depressive symptoms. It consists of 21 items covering the vegetative, affective and cognitive symptoms of depression. Respondents can indicate the severity of each symptom on a scale of 0–3 (e.g., for the symptom loss of pleasure, one can respond: “I get as much pleasure as I ever did from the things I enjoy,” “I don’t enjoy things as much as I used to,” “I get very little pleasure from the things I used to enjoy,” or “I can’t get any pleasure from the things I used to enjoy”).

Self-Esteem. The Rosenberg Self-Esteem Scale (RSES; Rosenberg, 1979) assesses how one feels about oneself. It consists of 10 items, scored 0 (strongly disagree) to 3 (strongly agree), with higher scores indicating more positive feelings about oneself. Items include “I feel that I have a number of good qualities,” “I feel I do not have much to be proud of” (reverse scored), and “I take a positive attitude toward myself.”

Autonomy and Self-Acceptance. The Ryff Psychological Well-Being Scale (PWB; Ryff, 1989) operationalizes psychological well-being in 6 dimensions. We selected the dimensions of autonomy and self-acceptance, as these dimensions are most pertinent to the potential effects of social media. We utilized the 42-item version, selecting the 14 items belonging to these two dimensions. Items are scored on a scale of 1 (strongly disagree) to 6 (strongly agree), with higher scores indicating higher levels of well-being. Examples of items from the autonomy subscale include “My decisions are not usually influenced by what everyone else is doing” and “I tend to worry about what other people think of me” (reverse scored). Examples of items from the self-acceptance subscale include “I like most aspects of my personality” and “In many ways, I feel disappointed about my achievements in life” (reverse scored).

Objective Measure of Social Media Usage

To track usage of social media, we had subjects email screenshots of their iPhone battery usage at specified increments. iPhones automatically track the total minutes each application is actively

open on the screen. The battery screen allows users to display their usage for the past 24 hours or 7 days. We provided instructions on how to get to this screen with every reminder to send in a screenshot. In the spring semester of the study, subjects were also asked to estimate their daily usage of Facebook, Instagram, and Snapchat before starting the baseline self-monitoring period.

PROCEDURE

FALL SEMESTER

Subjects signed up via the online website for the University psychology subject pool. Upon signing up, they were directed to a secure Qualtrics platform where they saw the consent form, and then completed the baseline survey of mood and well-being measures. Subjects were then sent a welcome email describing the study in more detail. This email informed them that, starting that night, they would be sending in a screenshot of their battery screen displaying the past 24 hours of usage in minutes. They were told they would be doing this each night for the next four weeks. Subjects were told to use social media as usual until they received their next email. If they had signed up for the study but had not yet completed the baseline survey, they were sent a similar email detailing the study, with the added reminder to complete the baseline survey. They were not told to send in screenshots until they completed the baseline survey.

One week after completing the baseline survey, subjects were emailed their second survey. This survey was identical to the baseline survey, but excluded the BDI-II (it was assumed that depression would not fluctuate much on a week-by-week basis). Subjects were asked to send a screenshot of their battery usage, and then received their group assignment. The control group was instructed to continue to use social media as usual, while the intervention group was told to limit their usage on Facebook, Instagram and Snapchat to 10 minutes per platform per day.

Subjects continued to send in nightly screenshots for the next 3 weeks. They also continued to take the survey at the end of each week (the surveys at the end of the 2nd and 3rd weeks did not include the BDI-II, but the survey at the end of the 4th week—

i.e. at the end of the intervention phase—did include the BDI-II). At the end of the fourth week, they were sent a wrap-up email after completing their survey and sending in their screenshot. This email explained that they would be receiving course credit shortly and that they were essentially done with the study, with the exception of a one-time follow-up that would be sent out around a month later. This follow-up included the survey (with the BDI-II), and a final screenshot of their usage for the past 24 hours.

SPRING SEMESTER

In the spring semester the procedure was essentially the same as in the fall, albeit with two changes. First, we decided to include the BDI-II in all surveys that were sent out. We regretted not having as much intermediate data on depression levels. Second, instead of having subjects send in screenshots every night, we instructed them to send in screenshots displaying the past 7 days of usage once a week. This was done for two reasons. First, although subjects were encouraged to send in screenshots at around the same time each night, subjects inevitably sent screenshots in earlier or later than the time they had sent in the screenshot the previous night. Having a screenshot sent in an hour early compromises the quality of data in the context of a 24-hour window much more than it does in the context of a 7-day window. Second, because the study lasted for four weeks, nightly screenshots were a significant logistical commitment for both the subjects and researchers. We expected that people would be more likely to send in all screenshots if they were just asked to send in 5 screenshots, as opposed to 29. In addition, it was more manageable for researchers to promptly follow up with subjects who did not send in a screenshot. This reduced error variance from subjects submitting screenshots at different times, or forgetting to send specific screenshots. Unfortunately, the battery usage app resets each time the phone is turned off. Thus, for a few subjects, we had to extrapolate weekly usage from fewer

than seven full days of battery usage. However, given that this is the first study to attempt to measure usage objectively (rather than relying on retrospective self-report) we are confident that our usage data are more reliable and valid than those of previous studies.

RESULTS

CORRELATIONAL AND PROSPECTIVE RESULTS PRIOR TO RANDOMIZATION

We found that baseline depression, loneliness, anxiety, perceived social support, self-esteem, and well-being did not actually correlate with baseline social media use in the week following completing the questionnaires. That is, more distressed individuals did not use social media more prospectively. Baseline Fear of Missing Out, however, did predict more actual social media use prospectively ($r = .20, p < .05$). Similarly, actual usage during the first week of baseline monitoring was not associated with well-being at the end of the week, controlling for baseline well-being. These results are somewhat at odds with prior research, which often finds an association with estimated, self-reported social media use and measures of well-being prospectively.

In the spring, we asked subjects to give us estimates of their use (essentially retrospective self-report data as is used in most correlational studies of social media use and well-being). Interestingly, estimated use *was* significantly negatively correlated with perceived social support ($r = -.24, p < .05$) and marginally negatively correlated with both self-esteem ($r = -.23, p = .056$) and overall well-being ($r = -.21, p = .08$). Estimated use and actual use were significantly, but only modestly correlated with each other ($r = .31, p = .01$). Eliminating three univariate outliers from the data (people who estimated over 900 minutes, or 15 hours of use per week) yielded even more modest results ($r = .26, p < .05$). That is, people were not very good at estimating their actual use, and retrospective self-report bias appears to explain at least some of the correlational findings.

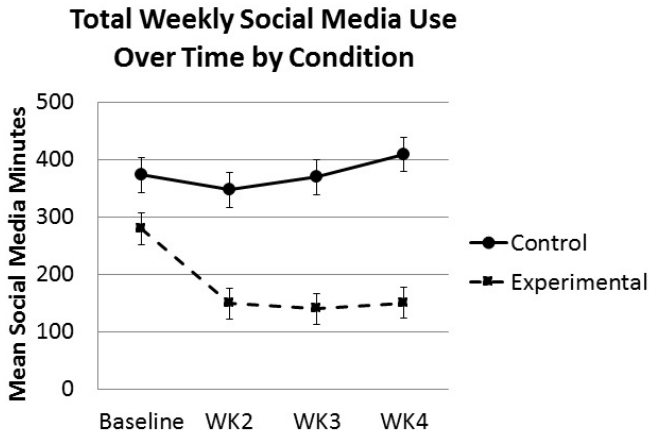


FIGURE 1. Total weekly social media use over time by condition.

EXPERIMENTAL RESULTS

MANIPULATION CHECK

First, we ensured that subjects in the experimental condition did indeed limit their usage by conducting an independent samples t-test at each week of the intervention. Although not every subject complied perfectly with the established time limit, on average the experimental group used significantly less social media than the control group for week one, $t(117) = 5.69, p < .001$, week two, $t(119) = 6.516, p < .001$, and week three, $t(113) = 5.78, p < .001$, of the intervention. On average, the experimental group also remained within the limit of 210 minutes per week at weeks one ($M = 179, SD = 140$), two ($M = 166, SD = 149$), and three ($M = 176, SD = 155$). See Figure 1.

EFFECT OF CONDITION ON LONELINESS

We then ran an analysis of covariance to determine the effect of condition on loneliness. Controlling for baseline loneliness and actual usage, subjects in the experimental group scored signifi-



FIGURE 2. Loneliness at week 4 by condition.

cantly lower on the UCLA Loneliness Scale at the end of the intervention, $F(1,111) = 6.896, p = .01$. See Figure 2.

EFFECT OF CONDITION ON DEPRESSION

Next, we first ran a univariate analysis of variance to assess the effect of group assignment on depression, controlling for baseline depression, actual usage, and the interaction of baseline depression and condition. There was a significant interaction between condition and baseline depression, $F(1, 111) = 5.188, p < .05$. To help with interpretation of the interaction effect, we split the sample into high and low baseline depression. Subjects were considered low in baseline depression if they scored below the clinical cut-off of 14 on the BDI, and high if they scored a 14 or above. When analyzed this way, there were significant main effects of both baseline depression and condition on depressive symptoms at week 4, for High/Low baseline, $F(1,111) = 44.5, p < .001$; for Condition, $F(1,111) = 4.5, p < .05$. In sum, individuals high in baseline depression in the control group saw no change in mean BDI score over the course of the study (at baseline, mean BDI = 22.8, at Week 4 mean BDI = 22.83). In contrast, individuals in the experimental group saw clinically significant declines in

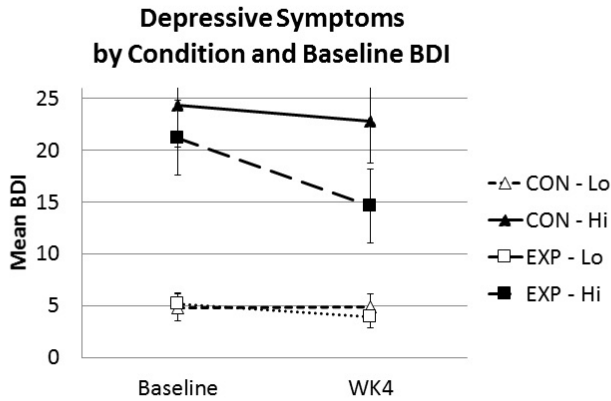


FIGURE 3. Depressive symptoms by condition and baseline BDI.

depressive symptoms, from a mean of 23 at baseline, to a mean of 14.5 at Week 4. Individuals low in baseline depression in the experimental group saw a statistically, but not clinically significant decline of a single point in mean BDI (from 5.1 at baseline to 4.1 at Week 4). Individuals low in baseline depression in the control group, on the other hand, showed neither statistically nor clinically significant change in depressive symptoms (from 5 at baseline to 4.67 at Week 4). See Figure 3.

EFFECT OF CONDITION ON ALL OTHER MEASURES

After running analyses of covariance on interpersonal support, fear of missing out, anxiety, self-esteem, and psychological well-being, we found no significant differences between the two groups.

We did, however, see a slight, but statistically significant decline from baseline to the end of the intervention in fear of missing out in both the control, $t(46) = 3.278, p < .002$, and experimental, $t(65) = 3.568, p < .001$, groups. Similarly, we observed a slight decline in anxiety in both the control, $t(46) = 3.035, p < .004$, and experimental, $t(65) = 2.477, p < .016$, groups.

FOLLOW-UP DATA

Unfortunately, we experienced significant attrition from the study at the final follow-up wave of data collection in both the fall and spring semesters. In total, we were able to collect complete follow-up data (including both objective use and well-being data) from only 30 individuals (21%). We deemed that sample size too small to provide reliable or meaningful results.

DISCUSSION

As hypothesized, experimentally limiting social media usage on a mobile phone to 10 minutes per platform per day for a full three weeks had a significant impact on well-being. Both loneliness and depressive symptoms declined in the experimental group. With respect to depression, the intervention was most impactful for those who started the study with higher levels of depression. Subjects who started out with moderately severe depressive symptoms saw declines down to the mild range by the simple expedient of limiting social media use for three weeks. Even subjects with lower levels of depression saw a statistically significant improvement as the result of cutting down on social media, although a mean decline of one point in BDI score is probably not clinically meaningful. As one subject shared with us “Not comparing my life to the lives of others had a much stronger impact than I expected, and I felt a lot more positive about myself during those weeks.” Further, “I feel overall that social media is less important and I value it less than I did prior to the study.”

Throughout the four-week intervention, subjects in both groups also showed a significant decline in both fear of missing out and anxiety. We posit that this was a result of the self-monitoring inherent in the study. As one subject in the experimental group said “I am much more conscious of my usage now. This was definitely a worthwhile study in which to partake.” Another noted “It was easier than I thought to limit my usage. Afterwards I pretty much stopped using Snapchat because I realized it wasn’t something I missed.” Although there was no statistically signifi-

cant decline in usage in the control group, even those subjects reported that self-monitoring impacted their awareness of their use. For example, one said “The amount of time spent on social media is alarming and I will be more conscientious of this in the future.” Another reported “I was in the control group and I was definitely more conscious that someone was monitoring my usage. I ended up using less and felt happier and like I could focus on school and not (be as) interested in what everyone is up to.”

Interestingly, our subjects did not show any improvement in social support, self-esteem, or psychological well-being. Perhaps these measures are truly unaffected by social media. It is also possible that the intervention was not long enough to produce any changes in these measures. Or, it could be that the time limit we imposed was either too restrictive or not restrictive enough to bring about positive change in these domains.

With the exception of fear of missing out, well-being at baseline did not predict actual social media use prospectively during the first week of self-monitoring. FOMO, however, did predict more usage, as might be expected. Similarly, actual use during the first week did not predict changes in well-being over that week controlling for baseline. Estimated use, however, *was* negatively correlated with perceived social support, self-esteem, and overall well-being. That is, more distressed individuals believed that they used social media more than less distressed individuals, despite the fact that there were no differences in objective use. Since ours is the first study that we know of to collect objective use data, this highlights the importance of future research not relying on retrospective self-report or estimated use data.

LIMITATIONS

This study had several important limitations. While we did our best to monitor and limit social media usage, we were only able to do so on mobile phones (this was not an issue for Snapchat, which can only be used through the mobile application). While participants were instructed to only use Facebook, Instagram, and Snapchat through the applications on their phones, they still had the ability to use social media on their computers, use

friends' phones, access the websites via the internet on their phone, etc. Furthermore, we could not actually turn someone's social media off if they went over 10 minutes. While most people were compliant with the study instructions, there were individuals in the experimental group who used significantly more social media than they were supposed to.

Moreover, social media does not just include Facebook, Snapchat, and Instagram. While we only measured and manipulated these three platforms, participants could still opt to go on Twitter, Tumblr, Pinterest, Facebook Messenger, dating sites, and so on. Indeed, some subjects noted that they spent a lot more time on dating apps, perhaps as the result of limiting other platforms.

In addition, our sample was a convenience sample of University of Pennsylvania students who had iPhones. We excluded Android phone users only because tracking battery usage data would have required downloading a separate app for those users. However, informal surveys suggested that the vast majority of Penn students were iPhone users, so we are not unduly worried about the sample being biased in this regard. However, future studies should certainly include Android users.

Lastly, we suffered from significant attrition at follow-up, losing 79% of our subjects, largely because we were forced to grant the extra credit for participation prior to the follow-up data collection time point. As a result, there was no incentive for subjects to complete the lengthy battery or take the trouble to submit a screen shot of their usage. This precluded reliable analysis of post-intervention social media habits and well-being. Thus, we were not able to assess maintenance of gains in well-being or to determine whether people reverted to their old use patterns. Future studies should build in incentives for subjects to continue to participate so that this valuable data could be collected.

FUTURE DIRECTIONS

As our study was the first of its nature, there are many opportunities for further investigation. These findings certainly bear replication with a more diverse population. The study should also be replicated with a broader inclusion of social media platforms,

including Twitter, Pinterest, Tumblr, etc. Dating apps in particular might be a fruitful avenue of investigation especially for individuals in their late teens to late twenties. Future researchers should also incentivize follow-up participation to decrease attrition. This will allow for critical analyses pertaining to habit maintenance.

Furthermore, moderators associated with social media use could be assessed further. These could include number of Facebook friends, Instagram followers, length of Snapchat streaks, and so on. These potential moderators could be analyzed in the context of ability to comply with the restrictions, as well as the success of the intervention.

Lastly, the length of the intervention and the length and nature of the limits imposed on usage could be explored in more detail going forward. It may be that there is an optimal level of use (similar to a dose response curve) that could be determined. This would allow for a more nuanced understanding of the amount of social media that is adaptive for most users. Alternatively, one could also explore the utility and impact of apps that actually control or limit the use of other apps (such as App Detox, AntiSocial, and Off the Grid). Informally, however, many students shared with us that either they (or their parents) had tried such apps, but that they are so easy for tech savvy young adults to circumvent that they didn't really work. A better strategy might be apps that increase self-monitoring and awareness of use, such as In Moment and Space. Empirical investigation of their efficacy and impact might well be warranted.

CONCLUSION

Most of the prior research that has been done on social media and well-being has been correlational in nature. A few prospective and experimental studies have been done, but they have only focused on Facebook. Our study is the first ecologically valid, experimental investigation that examines multiple social media platforms and tracks actual usage objectively. The results from our experiment strongly suggest that limiting social media usage does have a direct and positive impact on subjective well-

being over time, especially with respect to decreasing loneliness and depression. That is, ours is the first study to establish a clear causal link between decreasing social media use, and improvements in loneliness and depression. It is ironic, but perhaps not surprising, that reducing social media, which promised to help us connect with others, actually helps people feel less lonely and depressed.

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