Understanding the interactions of sleep, social media and mental health for productivity and performance: The role of field experiments

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1. *Introduction*

The study of the wealth and well-being of nations has generally focused on markets. Prior annual proceedings of the Upton Forum have examined the development and functioning of markets, cultural and institutional structures that support markets, and key inputs into markets including energy and human capital. More recently, there is a growing recognition that individual behaviors that take place largely outside of traditional markets are critical for productivity, performance and well-being. And relatedly, personal mental health and subjective well-being are important to understand both as direct measures of welfare, and as critical factors for labor market and human capital outcomes.

In this vein, we examine the role of sleep, social media and mental health for performance and productivity. We focus on these factors for several reasons, which we discuss in more detail throughout the paper. First, there is evidence — largely from naturally occurring data — that they matter for the wealth and well-being of nations. Second, these factors are highly interrelated. This raises a

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host of challenges for identifying causality in naturally occurring data, which field experiments can help address. Third, the existing experimental evidence on these factors is limited. Our goal in this paper is to provide guidance for the design of field experiments that can more fully inform our understanding of the causal linkages between sleep, social media and mental health, and their impact on productivity and performance.

Our approach builds on John List's pioneering research in field experiments. His far-reaching work has demonstrated the potential for field experiments to inform a broad range of economic questions in natural contexts. His field experiments are particularly influential because they unite tests of theory – more generally associated with lab experiments – with tests of policy relevant interventions. That is, economic frameworks motivate the experimental design so that the results can inform the parameters of interest. Finally, these frameworks often incorporate both standard theory and behavioral models in order to inform economic theory more broadly.

The remainder of the paper is structured as follows. In Section 2, we develop a framework for the interactions of sleep, social media and mental health for productivity and performance. We then discuss how the framework can guide the design, analysis and interpretation of experiments. In Section 3, we review the evidence on sleep, social media and mental health through the lens of our framework. Section 4 concludes with a discussion of promising avenues for future research.

2. Framework

Sleep, social media use, and mental health are interlinked phenomena, making their impact on productivity and performance difficult to disentangle from one another. We develop a simple framework to illustrate the identification challenge and help guide empirical design and analysis.

Let Y be productivity or academic performance, H be mental health, M be social media usage and S be sleep. We assume that Y is an additively separable function of H, M and S; H is a function of M and S; M is a function of H and S; and S is a function of H and M:

$$\begin{aligned} Y &= f(H(M,S),M(H,S),S(H,M)) \qquad (1) \\ &= f_1(H(M,S)) + f_2(M(H,S)) + f_3(S(H,M)) \end{aligned}$$

The causal impact on Y from a change in mental health, social media use or sleep, respectively, is:

$$\frac{\partial Y}{\partial H} = \frac{df_1}{dH} + \frac{df_2}{dM}\frac{\partial M}{\partial H} + \frac{df_3}{dS}\frac{\partial S}{\partial H} \tag{2}$$

$$\frac{\partial Y}{\partial M} = \frac{df_1}{dH}\frac{\partial H}{\partial M} + \frac{df_2}{dM} + \frac{df_3}{dS}\frac{\partial S}{\partial M} \tag{3}$$

$$\frac{\partial Y}{\partial S} = \frac{df_1}{dH}\frac{\partial H}{\partial S} + \frac{df_2}{dM}\frac{\partial M}{\partial S} + \frac{df_3}{dS} \tag{4}$$

Equations 2, 3, and 4 decompose the effects of mental health, social media use, and sleep, respectively, on Y. Considering equation 4 for the sake of illustration, we see that the impact of sleep on productivity can be broken down into an unmoderated effect, $\frac{df_3}{dS}$, its effect through the channel of social media, $\frac{df_2}{dM}\frac{\partial M}{\partial S}$, and its effect through the channel of mental health, $\frac{df_1}{dH}\frac{\partial H}{\partial S}$. The framework can be easily adapted to include additional channels. For example, if someone sleeps more, they may have more energy to exercise. Increased exercise may then have an impact on physical and mental health, as well as productivity and performance. Or, if someone sleeps more they may have less time to work or study, which could lower earnings or academic performance. We focus on sleep, social media and mental health because we believe these are three areas of increasing importance with important interactions that are not well understood.

This framework highlights the empirical challenge of identifying causal effects in naturally occurring data. First, naturally occurring data largely provides correlations. For example, there may be a negative correlation between social media use and mental health – i.e., lower levels of mental well-being among those with higher usage of social media. However, from a correlation alone it is not possible to determine whether social media usage is changing mental health (e.g., using social media decreases well-being); mental health is driving social media usage (e.g., when people are depressed they stay home and spend more time on social media); or, the correlation largely reflects an omitted channel such as the effect of sleep on both social media and mental health (e.g., when people don't get enough sleep, they feel more depressed and they have less energy, so they spend more time on social media).

Experiments can help address the identification challenge by exogenously varying one factor and examining the impact on the other factors. For example, as we discuss in more detail below, an experiment may randomly assign limited access to social media and measure the impact on subjective well-being. Related work takes advantage of naturally occurring quasi-experiments that arguably exogenously vary one factor. For example, Braghieri et al. (2022) uses the staggered introduction of Facebook across U.S. colleges as an instrument for increased social media usage and finds that the introduction of Facebook increased instances of depression and anxiety.

A second challenge is that naturally occurring data may not include measures of the relevant channels. For example, time use data may include measures of social media use and sleep but not mental health. Relatedly, quasi-experimental data from naturally occurring experiments may have measures of the outcome of interest but not the factors of interest. For example, Braghieri et al. (2022) does not include direct measures of Facebook usage (or other related channels such as sleep). These data allow you to estimate the effect of the presence of Facebook on mental health, but are more limited in estimating the direct impact of actual Facebook usage on mental health. Furthermore, they cannot identify the role of interactions with other factors. For example, when Facebook is introduced on a college campus, students may stay up later on social media and get less sleep, which may contribute to the observed decline in mental health.

The framework also highlights the challenge of using laboratory (lab) experiments to estimate the full impact of changes in social media usage and sleep. Lab experiments that exogenously vary one factor over a short time frame in a highly controlled environment may limit the role of interactions with other channels. For example, in a sleep study examining the effects of sleep deprivation, participants may spend several nights in a lab with proscribed sleep time and then perform tasks to measure cognitive ability. This kind of study largely isolates the short run effects of decreased sleep on cognitive performance. It does not allow for the measurement of longer run impacts that may take time to emerge, such as the impact on mental health or overall labor market engagement or academic performance. And because participants are in a sleep lab, there is limited scope for changes in performance via outside channels such as social media habits.

Field experiments can address the limitations of lab experiments by measuring the impact of exogenous movements in one factor on various channels. The framework can help guide the design of those experiments. For example, a field experiment that exogenously varies sleep and measures the impact on performance should also measure the impact on time use (including social media usage) and mental health. Field experiments can also examine the extent to which causal relationships are symmetric. For example, decreasing social media use may improve mental health. But improving mental health may not naturally lead to decreases in social media usage.

The framework can also guide the analysis and interpretation of both experimental and non-experimental data. Outside of the lab it is difficult, if not impossible, to assign individuals to a specific level of social media use, sleep or mental health. Estimating the impact of these factors relies on instruments that cause changes in these factors. In the case of sleep, for example, in non-experimental data, the instruments include differences in sunset times or class start times that affect sleep habits. In randomized field experiments, the instruments include goal setting, sleep aids and incentives to increase sleep. All of these are "encouragement" designs in which sleep itself is not directly assigned and so there may be varying degrees of compliance with the intervention. For example, in a field experiment, Giuntella et al. (in preparation) randomly assign participants to a treatment group in which they receive incentives for sleeping at least 7 hours a night on weekdays. These incentives increase sleep but there is imperfect compliance – i.e., participants in the treatment group do not meet the 7 hour threshold on every weeknight. The experiment yields an estimated intent-to-treat (ITT) impact of the intervention (i.e., incentives) on academic performance. This estimate can be converted into an estimate of the impact of increased sleep on academic performance, $\frac{\partial Y}{\partial S}$, but requires additional assumptions, including that the intervention only affected academic performance via sleep. And, this estimate does not isolate the "direct" effect of sleep on academic performance, $\frac{df_3}{dS}$, from the effects via other channels including mental health, $\frac{df_1}{dH}\frac{\partial H}{\partial S}$, and social media use, $\frac{df_2}{dM}\frac{\partial M}{\partial S}$. It is important to recognize these limitations when interpreting the estimated treatment effects of the intervention.

By measuring each of the potentially relevant channels, we can generate an understanding of which matter most for the question of interest. Experiments can then directly incorporate the most relevant channels into the design. For example, Giuntella et al. (in preparation) find evidence that changes in sleep lead to changes in social media use but less evidence that changes in sleep strongly affect mental health. Future work could test interventions aimed at both sleep and social media within the same experiment to better disentangle their interacted effects on academic performance. More broadly, the framework highlights both the role of field experiments and their limitations in identifying the interacted causal impact of sleep, social media and mental health on productivity and performance.

3. Reviewing the Evidence

3.1 Mental Health

There is growing recognition of the association between mental health and economic outcomes. Greenberg et al. (2021) estimate the economic burden of depression alone to be over $300 billion dollars in the U.S., with workplace costs accounting for about 60% of the total. They estimate that both the overall burden and the share due to the workplace have increased over time. At the individual level, mental health disorders can carry large earnings penalties. A study of workers in Denmark found that depressed people made 34% less, people with bipolar disorder made 38% less, and those with schizophrenia made 74% less (Biasi et al., 2021). At the same time, research into effective treatments for mental illness is well established, with abundant evidence for the beneficial effects of targeted psychotherapeutic and pharmacological treatment programs, and sometimes, most effectively, a combination of both (Cuijpers et al., 2009; Kamenov et al., 2017; Smith & Glass, 1977).

Building on this work, a nascent literature examines the causal impact of mental health interventions on labor market outcomes, economic well-being, human capital investment and criminal activity. In addition, these studies explore novel forms of psychotherapy, such as therapy delivered in group settings by non-professionals, which have the potential to be delivered at a fraction of the cost and at scale. Lund et al. (2020) conduct a meta-analysis of the impacts of mental health interventions in low and middle-income countries. They find that, on average, psychological interventions improve economic outcomes like earnings and investment in middle-income countries, with more mixed effects in low-income countries. We discuss examples of interventions in low, middle and high income countries in more detail below.

A strand of this literature focuses on the impact of psychotherapy on criminally-engaged or at-risk populations. Little et al. (1993) found that Cognitive Behavioral Therapy (CBT) reduced rates of recidivism by 32%, from 54.9% in the control group to 37.1% in treatment. Heller et al. (2017) studied two separate

CBT programs conducted in the Chicago area. The first, known as Becoming A Man (BAM), targeted adolescent students at risk of criminal involvement. The program was mainly composed of exercises which prompted participants to slow down their thinking and reflect on the beliefs and assumptions they held, with the goal that it would help them make the context-appropriate decision in more consequential (and perhaps dangerous) settings. BAM reduced overall crime committed by 28 to 35%, and violent crime even more so, by 45 to 50%. Perhaps more impressively, early evidence suggests it even increased graduation rates by 12 to 19%. The other CBT program they studied, which was conducted in a juvenile detention center, reduced recidivism by 21%. Although the impact on recidivism is smaller than that in Little et al. (1993), it reinforces the important role therapy could play for incarcerated individuals and those at risk for criminal activity.

Blattman et al. (2017) studied an 8 week-long CBT program, with additional $200 cash transfers to some, administered to criminally-engaged men in Monrovia, Liberia. The therapeutic course of the program had significant short-run effects. Participants engaged in fewer thefts, sold fewer drugs, and were less likely to carry a weapon. The authors argue that therapy helped these men to reinvent their own self-perception and weigh the long-term benefits of an activity more heavily than the short-term gain. Indeed, participants who received therapy responded more positively to questions regarding their self-identity and mental health two to five weeks out from the start of sessions. Therapy also affected these individuals' time preferences: when asked whether they would rather receive, say, 1,000 Liberian Dollars now or 1,100 Liberian Dollars in two weeks, they were more likely than the control group to wait the two weeks in order to receive the larger sum. However, these effects did not persist in one-year follow-up surveys. On the other hand, those who received a $200 cash transfer on top of CBT still engaged in less criminal activity a year out from the intervention.

In sum, the research strongly suggests that psychotherapy and counseling programs can affect one's decision to engage in criminal activity, with mixed evidence on how long the impacts last. It appears that mixing counseling and psychotherapy with an additional monetary or labor component can produce stronger results. Alongside the outcomes reported in Blattman et al. (2017), new evidence from a Chicago-based program providing 18 months of CBT alongside employment and other social supports offers additional evidence for this (Bhatt et al., 2023). Individuals enrolled in the program through the referral of a community outreach worker (as opposed to through a referral algorithm created by the researchers) were 79% less likely to be arrested for shooting and/or homicide and were 45% less likely to be a victim of violent crime. Individuals enrolled in the program algorithmically saw no such reductions.

Another strand of research focuses on therapeutic outcomes for individuals already suffering from mental health issues such as depression. Bhat et al. (2022) studied two programs conducted in the Indian state of Goa. The first was known as the Healthy Activity Program (HAP), in which a counselor met with depressed individuals weekly for 6-8 weeks. The primary goal of counseling was "behavioral activation", to encourage depressed individuals to schedule and engage in activities which they enjoyed. This program significantly reduced depression. As far out as five years from the beginning of treatment, participants were 11 percentage points less likely to be depressed than a control group. Similar to the BAM program in Heller et al. (2017), the HAP program changed how participants thought about their decisions. For example, it made them less overconfident, measured by how much they overvalued their performance relative to peers in an activity. The result was a more even-handed perception of self, with lower magnitudes of both under- and over-confidence.

The second program these authors studied attempted to address depression in pregnant women through the same technique of behavioral activation. However, unlike the HAP program, this program only marginally decreased perinatal depression in the short term and had no long-term effects. Furthermore, it did not alter patterns of thought as the HAP program did. Neither of these programs impacted employment or consumption.

These findings overlap somewhat with that of Angelucci and Bennett (2022), which explored the impacts of pharmacotherapy and group therapy sessions in a group of 1,000 depressed adults (86% of whom were women) in the Indian state of Karnataka. The intervention reduced depression. It also increased investment in children, increasing both school enrollment and attendance. Interestingly, the program did not increase employment or earnings. In fact, those just receiving pharmacotherapy worked over five fewer hours per week during their treatment of approximately four months. And those who received both pharmacotherapy and counseling worked over three hours less per week after the program ended. This is interesting given that an explicit goal of counseling was to help participants with work-related challenges and give them personalized advice on employment and other money-making opportunities.

Expanding out from specific populations like the criminally at-risk and depressed, a third strand of this literature studies the effect of psychotherapy and cash transfers on both mental health and economic well-being in general populations. Barker et al. (2021) studied the impacts of CBT on human capital in rural Ghana. Like all of the programs previously discussed, it had significant positive effects on mental health. Interestingly, there was no differential impact between those with high and low baseline mental distress, suggesting CBT can help those who do not suffer from mental illness just as much as those who do. The program also increased perceived economic well-being: participants reported higher economic status, and projected a higher economic status in five years, compared to a control group. It's important to note that these results were measured just two to three months out from a twelve week course, a much shorter time span than some of the previous studies discussed above.

A similar study treated a general population in rural Kenya with therapy sessions, cash transfers, or both, for five weeks (Haushofer et al., 2020). Individuals who received just therapy showed no differences from the control group 12 months out from the intervention. There was no lasting impact on their mental health or economic outcomes such as asset holdings, consumption, or revenue. Indeed, the treatment arm that received both therapy and cash transfers had quite similar mental health and economic outcomes to the arm that just received cash transfers. Taken together, these results suggest that, while CBT can be beneficial for individuals without mental illness, its effects on those with mental illness are longer lasting.

The evidence reviewed suggests treating mental health issues can improve important outcomes in the household such as investment in children, but evidence on how these strides might extend to the workplace is mixed. Why did the improvements in mental health exhibited in these studies fail to materialize into tangible economic improvements like labor market productivity, and in some cases, even decrease time worked? It could be in part mechanical – time spent on the mental health intervention could serve as a substitute for time spent in the labor market. The lack of increases in earnings or time worked could also be due to the significant constraints on female employment where some of the studies took place (Bhat et al., 2022). Future work could examine the role of heterogeneity in both social context and individual mental health in understanding the differential impacts of mental health interventions.

Another important general insight from these studies is that effective psychotherapy need not be delivered by professional psychologists, psychiatrists, or therapists. Rather, it can be delivered by trained community members and at a fraction of the cost. Programs that pair this type of therapy with a lump-sum cash transfer are remarkably cost-effective, and tend to result in stronger effects than either therapy or cash transfers in isolation. Recent studies in this vein test the impact of low-cost mindfulness interventions either delivered through group therapy or online apps (Cassar et al., 2022; Shreekumar & Vautrey, 2022). Shreekumar and Vautrey (2022) find that the $13-per-month app "Headspace" reduces rates of depression by 0.46 standard deviations (SDs), anxiety by 0.38 SDs, and stress by 0.47 SDs, reductions which are comparable to those usually achieved by professional therapists. Cassar et al. (2022) find that a university course on mindfulness also reduced levels of depression, anxiety, and stress. They find mixed effects on academic performance. In the short-run, the course decreased students' grades by 0.26 SDs. The authors suggest that a possible reason for this is that students enrolled in the course spent more time on self-care related activities such as sleeping and relaxing, possibly at the expense of time studying. By contrast, in the longer-run, they find evidence that the intervention increased grades by 0.28 SDs, a result mainly driven by the individuals who engaged in mindfulness practices outside of the course.

To our knowledge, these studies have not examined the interactions of mental health with social media use and sleep. Future work could estimate the impact of mental health interventions on these channels in order to better understand whether they are contributing to (or perhaps dampening) the effects of mental health improvements on performance. As evidenced by Cassar et al. (2022), an additional promising avenue for future work is to examine these questions in the context of academic performance, where the relationship between components of our mental health—like stress—and academic achievement can be complicated and are not well-understood.

3.2 Social Media

There has been rapid growth in research examining the impact of social media (see Aridor et al. (forthcoming) for a comprehensive review). Prior work has examined the political ramifications of social media. This includes studies of communication amongst protesters engaged in pro-democratic movements, such as during the Arab Spring (Howard et al., 2011; Tufekci, 2017); as well as research on self-siloing into groups with like-minded ideological views and higher consumption of misinformation (Allcott et al., 2019; Gentzkow & Shapiro, 2011).

Another strand of research has focused on the relationship between social media and mental health. Multiple meta-analyses examine the interplay between depression and social media usage. Some find a positive correlation between social media use and depression (Appel et al., 2016; Keles et al., 2020). Yet others emphasize that results are split between positive, negative, and null results (Odgers & Jensen, 2020). Furthermore, even when a positive correlation between social media and mental health is found, it may not be economically significant. For example, Odgers and Jensen (2020) emphasize that social media usage explains "less than 0.5% of the variance in symptoms [of mental health in studies] with poor adjustment for relevant confounding factors and estimates that are virtually always derived from correlation designs." Similarly, an 8-year-long longitudinal study on adolescents found that as they grew, and consequently spent more cumulative time on social media, their mental health did not change as a result. They only found an association between social media and mental health when looking across individuals, instead of within individuals across time. This suggests that social media does not cause poor mental health. Rather, that mental health, or some unknown third factor, is in fact driving social media use (Coyne et al., 2020).

To address concerns with causal identification, a number of lab and field experiments have examined the impact of social media use on measures of mental health and well-being. We summarize the design of these studies in Table 1 and, when possible, report the estimated impacts in Figure 1.

The lab experiments tend to induce more social media use by having participants in the treatment condition use some type of social media. The time span for use in lab experiments is rather short, from 7 to 20 minutes (Engeln et al., 2020; Sagioglou & Greitemeyer, 2014). These experiments generally find increases in feelings of subjective well-being, while not significantly moving feelings of depression or negative affect.

Unlike lab experiments which induced an increase in social media use, field experiments attempt to limit social media use. They do so over a much longer period of time, from one day to three quarters of a school year (Collis & Eggers, 2022; Przybylski et al., 2021). These studies generally find either null effects or that limiting social media usage decreases feelings of depression and increases one's feelings of subjective well-being. As discussed above, field experiments are able to capture longer-term effects in natural environments which allow multiple channels to interact.

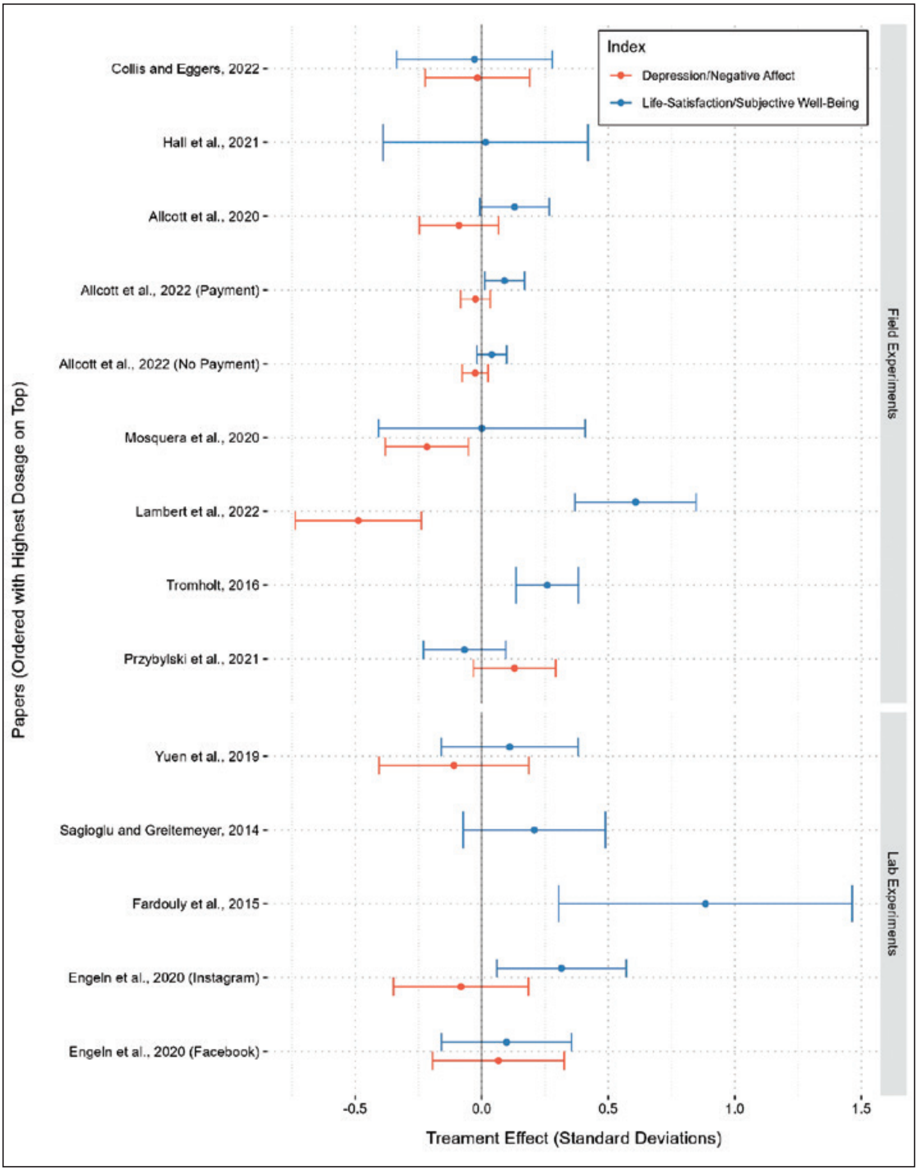
It is important to note that we are working with a small sample of studies on this topic, and therefore the high variation in treatment effects is due in part to chance. The studies also vary significantly in their design. As already noted, some studies focus on the acute effects of social media while others focus on their chronic ones. Some focus on a particular population (most commonly University students) while others recruit from a general population of social media users. Some treatments include full abstinence from social media, while others limit it to a cutoff (for example, 10 minutes a day) or encourage reductions in usage. Some limit usage of just one site (most commonly Facebook), others limit all social media use. These dimensions are likely crucial in determining whether social media is a net positive or negative experience. The studies also vary in how they measure the outcomes, with some using validated scales and others a single question asking, for example, if someone feels depressed.

An especially important dimension along which the interventions vary is in how much of a change they induce on time spent on social media (summarized in column 3 of Table 1). For example, studies that focus on chronic effects span in intensity from, on the low end, not using Facebook for five days (Vanman et al., 2018) to, in the extreme, using social media for only ten minutes a day for three quarters of a school year (Collis & Eggers, 2022). Interestingly, we do not find a strong relationship between treatment intensity and treatment effect when comparing across studies. This may contradict the "Goldilocks" theory of social media use. This is the idea that some amount of social media use is beneficial but as use increases the drawbacks overtake the benefits and it becomes a net harm (Dienlin & Johannes, 2022; Przybylski & Weinstein, 2017). Looking across studies of different treatment intensities we find no evidence of this relationship. If this were the case, we would expect a U-shaped curve in treatment effects in Figure 1, as papers are in order from highest (at the top) to lowest treatment intensity.

These results suggest that the effects of social media are heterogeneous across people and contexts, as well as levels and types of usage. It can be difficult to separately identify the role of these dimensions in naturally occurring data because they are often correlated with one another. For example, consider the case of the effects of social media on young people, a topic that has rightly received considerable attention (Auxier & Anderson, 2021; Wells et al., 2021). Social media usage is more statistically associated with negative mental health outcomes in young people than in adults, and within that group, young women more so than young men (Orben et al., 2022). However, young people use social media more than adults, and young women use social media more than young men (Vogels et al., 2022). These overlapping features make studying social media's particular harm on young people difficult: is it that it has a differential impact on young people? Or that it has a differential impact on women, who make up the majority of social media users? Or is it that spending more time on social media, as young people do, is harmful? Or maybe a combination of all three? Future work in field experiments could focus on causally identifying the heterogeneous effects of social media by gender, age and other critical demographic groups. As discussed above, these studies should gather rich data capturing the multidimensional impacts of social media, which may not be available in naturally occurring data.

Another nascent frontier for research on social media is its effect on labor market outcomes and productivity. Early evidence on this topic suggests social media may have significant negative impacts on both. For example, Marotta and Acquisti (2017) looks at the effect of the social media blocking app "Freedom" on how Amazon Mechanical Turk workers complete tasks. Those assigned to have social media blocked completed 8 more tasks per hour and earned $0.80 more than a control group. As Gen Z, the first generation to grow up using social media, enters the workforce, research on this topic will become even more pressing. Relatedly, even though many of the studies have been conducted with students, there is sparse evidence on how social media interacts with sleep and academic performance. While there is quasi-experimental evidence that social media use reduces self-perceived academic performance (Braghieri et al., 2022), the longest-run social media intervention to date found no measurable impact. Collis and Eggers (2022), which reduced usage to ten minutes a day for three quarters of a school year, found that the intervention had no impact on academic success. There is also little evidence on the impact of social media interventions on sleep which, as we discuss below, is potentially important for understanding the interacted effects on performance.

Figure 1: Effect of Limiting Social Media on Depression and Life-Satisfaction.



Notes: Dosages were determined by length of treatment and, if multiple treatments had the same duration, intensity of treatment (e.g., limiting versus abstaining social media). Error bars are 95% confidence intervals.

Table 1: Field Experiments on Effects of Social Media on Mental Health

Paper	Sample	Treatment(s)	Social media usage	Mental health & other outcomes
Tromholt (2016)	Facebook Users (N = 1,095)	A week off Facebook	87% full abstinence (over 60 minutes at baseline)	Envy, life satisfaction
Hunt et al. (2018)	University Students (N = 143)	Three weeks of social media limited to 10 minutes a day	19 minutes avg. less social media from 41 minutes baseline [1]	Loneliness, depression
Vanman et al. (2018)	Facebook Users (N = 138)	Five days off Facebook	120 minutes avg. less Facebook from 171 minute baseline	SWB, Stress
Allcott et al. (2020)	Facebook Users (N = 2,897)	Four weeks off Facebook	59.9 minutes avg. less Facebook from 74.5 minutes baseline	Depression, political polarization, SWB, WTA
Mosquera et al. (2020)	University Students (N = 1,765)	2 weeks off Facebook	95% full abstinence (112 minutes at base line)	WTP, depression, SWB, news polarization
Hall et al. (2021)	University & CC Students (N = 130)	4 weeks off all social media	20% full abstinence (68.76 minutes at baseline)	Loneliness, SWB, Life Quality
van Wezel et al. (2021)	University Students (N = 76)	7 days of 50% reduced Facebook	13.5 minutes avg. less Facebook from 25.6 minutes baseline	SWB, FOMO
Przybylski et al. (2021)	University Students (N = 297)	1 day off social media	50% full abstinence	Positive and negative affect, self-esteem, life satisfaction
Lambert et al. (2022)	Social media users (N = 154)	7 days off Instagram, Tiktok, Facebook, and Twitter	79 minutes avg. less social media from 71 minutes baseline [2]	SWB, depression, anxiety
Allcott et al. (2022)	Facebook Users (N = 2,126)	a) Three weeks with goal setting app ("limit") b) $50 for every hour reduction in daily avg. over three weeks ("bonus")	20 minutes avg. less screentime from 153 baseline 60 minutes avg. less screentime from 153 baseline	SWB, addiction, depression, concentration
Collis and Eggers (2022)	University Students (N = 122)	Three quarters of social media limited to 10 minutes a day	15.45 minutes avg. less social media from 24.5 baseline	Academic performance, SWB

[1] Baseline and change in use estimated from Hunt et al. (2018) Figure 1
[2] Baseline usage was based on self-report data which is frequently underestimated

Table 2: Lab Experiments on Effects of Social Media on Mental Health

Paper	Sample	Treatment(s)	Mental health & other outcome(s)
Sagioglou and Greitemeyer (2014)	Mechanical Turk Users (N = 263)	Facebook for 20 minutes	Positive and negative affect, meaningful activity
Verduyn et al. (2015)	University Students (N = 67)	10 mins active (vs passive) Facebook Use	SWB, envy, active vs. passive use
Fardouly et al. (2015)	University Students (N = 122)	Facebook for 10 minutes	Negative affect, body dissatisfaction
Yuen et al. (2019)	University Students (N = 312)	Facebook for 20 minutes	Positive and negative affect, envy, meaningful activity
Engeln et al. (2020)	Female University Students (N = 308)	a) Facebook for 7 minutes b) Instagram for 7 minutes	Positive and negative affect, body dissatisfaction, social comparison

3.3 Sleep

Based on lab studies, sleep scientists have long been aware that lack of adequate sleep can have detrimental impacts on attention, memory, cognition, and mood (Banks & Dinges, 2007; Killgore, 2010). Lack of sleep is a worldwide problem, with more than 1 in 3 US adults sleeping less than 7 hours a night, the recommended minimum (Liu et al., 2016). Evidence suggests the problem may be substantially worse in low-income countries. Experiments conducted in India and Sri Lanka revealed that adults average 5.6 and 6.4 hours of sleep, respectively, each night (Bessone et al., 2021; Schokman et al., 2018).

Sleep, or lack thereof, has clear economic implications. Due to decreased cognitive function and "cyberloafing", the sleep-deprived are less productive, with one study from Australia estimating the cost of sleep deprivation at 0.8% of the country's economy (Hillman et al., 2006; Mullainathan, 2014). However, until recently, sleep has failed to attract much attention from the economic field.

Prior work has demonstrated that sleep and working hours are inversely correlated: those who sleep more work less (Basner et al., 2007; Biddle & Hamermesh, 1990; Pfeifer, 2015). However, it is difficult to draw causal conclusions from these data, as much of it is founded on self-reported sleep time, which usually overestimates actual time spent sleeping. More importantly, the presence of omitted variables which effect sleep and work time simultaneously cannot be ruled out. As the authors mention in Biddle and Hamermesh (1990), the pioneering work on sleep in economics, "we have not strictly established the direction of causation, if any, between sleep time and labor supply. It may be that variations in individuals' sleep time are beyond their control and that their labor supply changes in response to these variations."

To disentangle causation from correlation, economists have exploited quasi-experimental settings to estimate the impact of sleep on labor market outcomes (Carrell et al., 2011; Gibson & Shrader, 2018; Giuntella et al., 2017; Giuntella & Mazzonna, 2019; Jagnani, 2021; Jin & Ziebarth, 2020). The earlier the sun sets, the earlier people go to sleep. Thus time zone border areas, which have similar levels of natural light but are an hour apart on the clock, provide plausibly random variation in sleep time. These studies find that individuals living in the later, Eastern side of a time zone border have lower wages, with estimates ranging from .44% to 3%.

More recently, researchers have begun to use field experiments to examine the impact of interventions aimed at increasing sleep (Avery et al., 2022; Bessone et al., 2021; Giuntella et al., in preparation). Bessone et al. (2021) encouraged some of their 452 study participants in Chennai, India to sleep more, and paid others for sleeping longer than baseline. Although these encouragements and financial incentives did cause people to sleep more, their findings did not align perfectly with evidence from lab-based sleep studies. For example, contrary to previous evidence and expert predictions, the authors found that sleeping longer at nighttime had no effect on cognitive skills or subjective well-being. However, individuals who took a thirty minute nap at work in the afternoon scored higher on cognition tests, reported more positively on their well-being, and were more productive at work. The authors are not able to identify whether naps are more effective because of their timing or because they take place in a higher quality sleep environment than nighttime sleep. The null effects of nighttime sleep could be due to low quality of sleep, the added time needed to be in bed in order to sleep which substitutes for time working, or because baseline sleep is so far from recommended amounts that small increases do not have a significant impact.

In the U.S., Giuntella et al. (in preparation) encouraged college students to sleep more through app reminders on their phone as well as payments for sleeping at least 7 hours a night. Unlike Bessone et al. (2021), they found increases in performance, with significant GPA increases amongst those who received payments. Important for informing the framework discussed above, Giuntella et al. (in preparation) additionally find that the sleep intervention leads to lower social media usage but does not have discernible impacts on mental health.

Table 3 summarizes the field and quasi-experimental evidence on the impacts of sleep on outcomes related to productivity and performance.

A clear finding shared in studies is that both encouragement and financial incentives work in getting people to sleep more, and perhaps unsurprisingly, payment works better than encouragement. And the financial incentive need not be large. Bessone et al. (2021) paid participants a maximum of 120 rupees ($1.70) and Giuntella et al. (in preparation) paid $4.75 for sleeping at least 7 hours. However, there are mixed findings on the effect of sleep on labor market and academic outcomes. Future work could explore the extent to which sleep environments, sleep timing, baseline sleep and demographic differences may help explain the heterogeneous impacts of increased (or decreased) sleep.

4. Discussion

As noted above, the small but growing literature on sleep, social media and mental health demonstrates substantial heterogeneity in findings. Future work could explore the extent to which the differential treatment effects are explained by differences in the interventions (e.g., intensity, length of time), the population (demographics, types of participants who select into the study, social context) and baseline levels of the behavior of interest.

From a public policy perspective, it is important to understand the cost-effectiveness of these interventions in terms of their effects on productivity and performance. Is it more cost-effective to receive some form of psychotherapy, detox from social media, or get more sleep? The answer to this depends not only on the elasticity of the targeted behavior (e.g., are people more responsive to incentives for sleep or incentives to decrease social media?), but also on the cross-elasticities of those behaviors with the other channels of interest (e.g., what are the downstream effects on mental health and performance of improving sleep vs. decreasing social media use?).

Taking for example the early evidence from sleep and social media interventions, it appears that sleep interventions, on top of improving academic performance and health, also decrease social media use because individuals substitute screen time

Table 3: Literature on the Effect of Sleep on Productivity and Performance

Paper	Sample	Treatment	Sleep Outcomes	Productivity/Performance Outcomes
Panel A: ***Field Experiments***				
Bessone et al. (2021)	Low-income Indian adults (*N* = 452)	a) Information, encouragement, payments	27 minutes more sleep	No improvements to cognition, productivity, or SWB
		b) Information, encouragement, payments, half hour nap at workplace	8 minutes more sleep	Increases in productivity (0.04 s.d., s.e. = 0.02), SWB (0.08 s.d., s.e. = 0.03), and cognition (0.10 s.d., s.e. = 0.05)
Avery et al. (2022)	British & American university students (*N* = 508)	$3.75 (£2.5) for going to bed by 1 am and $3.75 (£2.5) for sleeping between 7-9 hours	9 minutes more sleep	-.59 less screen time and .42 hours more studying
Panel B: ***Quasi-Experiments***				
Carrell et al. (2011)	American University students	Class starting 50 minutes later	Not reported	.116 s.d. increase in average course grades
Giuntella et al. (2017)	Chinese individuals	Sun setting 30 minutes later (within time zone)	17 minutes less sleep among employed	3% reduction in mental capabilities, 10.5% increase in depression score
Gibson and Shrader (2018)	American individuals	Sun setting 1 hour later (within time zone)	23 minutes less sleep	4% decrease in earnings among employed
Heissel and Norris (2018)	American K-12 students	Class starting 1 hour later (across time zones)	6-13 minutes more Sleep	Increases math scores by .082 s.d., s.e. = .025 (.009 s.d., s.e. = .035), reading scores by .057 s.d., s.e. = .023 (.061 s.d., s.e. = .036) for adolescents (children)
Giuntella and Mazzonna (2019)	American individuals	Sun setting 1 hour later (across time zones)	20 minutes less sleep among employed	4.4% decrease in earnings, 5.5 p.p. increase in obesity rates
Jin and Ziebarth (2020)	German hospital admissions	End of day-light savings time	1.56 minutes more sleep	Decreases all-cause hospital admissions (2.6%), heart attacks (.41%), and heart attacks (.05%)
Jagnani (2021)	Indian K-12 students	Seasonal variation in sunset time	Sun setting 1 hour later leads to 30 minutes less sleep	Sun setting 10 minutes earlier leads to 0.1 s.d. (s.e. = .05) lower test scores, 0.04 (s.e. = .01) less school years
Lusher et al. (2019)	Vietnamese university students	Class starting 1 hour later	4.3 minutes more self-reported sleep	.009 s.d. (s.e. = .005) grade increase for morning courses, null effect for all courses
Groen and Pabilonia (2019)	American high schoolers	Class starting 1 hour later	38 minutes more sleep	.156 s.d. (s.e. = .082) reading score increase, null effects on math, health, employment

Notes: Please refer to Table 4 for the location of sleep outcomes and productivity and performance outcomes within each paper.

for more sleep (Avery et al., 2022; Giuntella et al., in preparation). For example, to take the most recent and largest scale social media intervention, Allcott et al. (2022) paid participants $2.50 for each hour of reduced time on Facebook, Instagram, Twitter, Snapchat, YouTube, or web browsers. As a result, people averaged 56 minutes less screen time each day, meaning they were paid about $2.34. On the other hand, Avery et al. (2022) paid participants $7.50 if they both went to bed between 10 PM and 1 AM and slept 7 to 9 hours. As a result, the portion of people sleeping less than six hours fell by nearly 4%. They also spent about 36 less minutes on social media. So while moving sleep somewhat, their effect on social media was significant and comparable to interventions which explicitly targeted it. While the intervention was more expensive ($7.50 per day versus $2.50), it killed two birds with one stone. Future field experiments in this area should directly compare these different types of interventions in a single population to better understand which are the most cost-effective levers to pull in order to improve health, wealth and well-being.

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5. *Appendix*

Table 4: Reference Locations – The Effect of Sleep on Productivity and Performance

Panel A: *Field Experiments*		
Paper	Sleep Outcomes	Performance Outcomes
Bessone et al. (2021)	Appendix Table A6	Table 4
Avery et al. (2022)	Table 3	Table 3

Panel B: *Quasi-Experiments*		
Carrell et al. (2011)	—	Table 4
Giuntella et al. (2017)	Table 3	Table 5
Gibson and Shrader (2018)	Table 1	Table 3
Heissel and Norris (2018)	Appendix Table A7	Table 4
Giuntella and Mazzonna (2019)	Table 1	Tables 4 & 6
Jin and Ziebarth (2020)	Table 1	Table 2
Jagnani (2021)	Table 1	Table 2
Lusher et al. (2019)	Table 6	Table 5
Groen and Pabilonia (2019)	Table 9	Table 5