

NBER WORKING PAPER SERIES

SLEEP: EDUCATIONAL IMPACT AND HABIT FORMATION

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Working Paper 32550
<http://www.nber.org/papers/w32550>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2024, Revised April 2026

We are grateful to the seminar participants at the BEDI Conference, the Roybal Annual Retreat, University of Pittsburgh, the Center for Sleep and Circadian Rhythms Study, the Virtual Seminar on the Economics of Risky Behaviors (VERB), the CHIBE Behavioral Science and Health symposium, the University of Houston, the National University of Singapore, the Freie Universität Berlin, UCSD Spring School on Behavioral Economics, University of Chicago, Harvard University, RAND, University of Southern California, the UCLA Anderson School of Management, the New York Federal Reserve, and the NBER Economics of Health Meetings (Fall 2023). We are grateful to Mallory Avery, Daniel Banko-Ferran, Jialu Dou, Liam Goodwin, Kelly Hyde, Ilaria Prometti, Ben Schenck, Samuel Lindquist, Gabrielle Toborg, and William Wang for their valuable research assistance. We are thankful to the Pittsburgh Experimental Economics Lab for recruiting our participants and the University of Pittsburgh Registrar Office for their help in providing access to students' academic records. We also thank Tomà Lintner for providing data related to educational program effects. We received generous funding from J-PAL North America (Sadoff), the National Institute of Aging (Saccardo, Grant #P30AG034546) and the Pitt Healthy Lifestyle Institute Pilot and Feasibility project (Giuntella). The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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JEL No. C93, I1, I20

ABSTRACT

There is growing evidence on the importance of sleep for productivity, but little is known about the impact of interventions targeting sleep. In a field experiment among U.S. university students, we show that incentives for sleep increase both sleep and academic performance. Motivated by theories of cue-based habit formation, our primary intervention couples personalized bedtime reminders with morning feedback and immediate rewards for sleeping at least seven hours on weeknights. The intervention increases the share of nights with at least seven hours of sleep by 26 percent and average weeknight sleep by an estimated 19 minutes during a four-week treatment period, with persistent effects of about eight minutes per night during a one to five-week post-treatment period. Comparisons to secondary treatments show that immediate incentives have larger impacts on sleep than delayed incentives or reminders and feedback alone during the treatment period, but do not have statistically distinguishable impacts on longer-term sleep habits in the post-treatment period. We estimate that immediate incentives improve average semester course performance by 0.075 - 0.089 grade points, a 0.10 - 0.11 standard deviation increase. Our results demonstrate that incentives to sleep can be a cost-effective tool for improving educational outcomes.

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A randomized controlled trials registry entry is available at
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1 Introduction

There is growing attention to the role of sleep for economic outcomes (Hillman et al., 2006; Mullainathan, 2014; Rao et al., 2021). At the same time, statistics suggest people are not sleeping enough. About a third of Americans sleep less than the recommended minimum of seven hours per night and a similar proportion state that they would like to sleep more (Jones, 2013; Ballard, 2019; Corkett, 2010; CDC, 2023). Sleep deprivation is even worse among adolescents and young adults, with about two-thirds regularly failing to meet sleep guidelines (Wheaton et al., 2018). These statistics have prompted policy concern that poor sleep may be worsening educational outcomes (Group et al., 2014). More broadly, Roenneberg (2013) referred to sleep deprivation as the most prevalent high-risk behavior in modern societies. Yet, little is known about whether interventions targeting sleep can improve productivity and performance.

Our study implements a randomized field experiment among U.S. university students to examine the impact of interventions targeting sleep on sleep habits and academic performance. Our work has three primary aims. First, we address long-standing gaps in understanding the causal relationship between sleep and human capital in a natural setting. Sleep’s importance has garnered widespread attention in popular discourse, fueled by best-selling books (Walker, 2017; Huffington, 2016; Attia, 2023). However, much of the public conversation relies on limited evidence, often from small-scale studies, short-term laboratory experiments, or non-experimental investigations. Unlike previous work, we explore not only the direct causal effects of sleep interventions on sleep and performance but also their opportunity costs and mechanisms, leveraging data on time use, physical activity, well-being, and post-intervention outcomes.

Second, we examine the role for behavioral interventions in improving academic performance, particularly in higher education. The large body of prior work in this area has primarily targeted academic outcomes directly through either performance-based incentives, academic advising, or both. These policies have demonstrated limited impact and are often high cost (Lintner, 2024). We test whether a relatively low-cost intervention targeting sleep can have a downstream impact on educational performance and examine the mechanisms underlying our effects.

Finally, we investigate habit formation in sleep. To do so, we continue to track participants after the intervention ends and examine the persistence of sleep behavior post-intervention. In addition, we test variants of our primary intervention to identify the key features that drive habit formation.

We ran the experiment over seven semester-long waves from Spring 2019 to Spring 2022.

The 1,149 participants wore tracking devices (Fitbits) that measured sleep, heart rate, and physical activity; downloaded a custom smartphone app linked to their Fitbit data, which delivered our interventions; and, answered survey questions to capture information about their time use, performance on creativity and quantitative tasks, and well-being. The study included a one- to four-week baseline period, followed by a four-week intervention period and a one- to five-week post-intervention period (the post-intervention period lasts at least four weeks for 71% of participants).

Our intervention aimed to develop persistent sleep habits beyond the treatment period by leveraging theories of cue-based habit formation. This approach emphasizes the role of contextual cues, repetition, and immediate reinforcement of desired behaviors in generating “automatic” habits (Verplanken and Wood, 2006; Wood and Neal, 2007; Wood and R  nger, 2016). These theories suggest that repeatedly rewarding a behavior performed in response to a consistent cue can gradually strengthen the association between the cue and the reward that reinforces the action. Once the association is established, the cue may automatically trigger the desired action with little or no cognitive effort, even in the absence of a reward (Dickinson, 1985). Accordingly, we paired daily sleep cues with immediate rewards for meeting sleep goals, aiming to create an association between the cue and the desired sleep behavior during the intervention. In the post-intervention period, we retained the cue but removed the rewards to test whether sleep habits persisted in response to the cue alone.

Specifically, we set a goal for treated participants to sleep at least seven hours by 9 am on weeknights (Sunday - Thursday), following recommended guidelines (Panel et al., 2015). During the intervention period, participants received personalized bedtime reminders every weeknight, prompting them to follow a self-selected bedtime routine to get at least seven hours of sleep. On weekday mornings, they learned whether they had met their sleep goal and, if successful, received an immediate financial reward of \$4.75. After the four-week intervention, we stopped the financial reward but maintained bedtime cues and morning feedback until the end of the semester (typically an additional four weeks). In secondary treatments, we tested variants of our primary intervention by providing rewards with a delay rather than immediately and by turning off either the rewards or the cue and feedback.

Our primary analysis compares the no intervention *Control* group to the *Immediate Incentives* group, in which participants received bedtime cues, morning feedback and immediate incentives for each weeknight they met the sleep goal during the intervention period.¹ At baseline, participants met the goal of sleeping at least seven hours on approximately 43% of

¹The primary analysis pools two sub-treatments of the *Immediate Incentives* group: the main variation that continued to receive reminders and feedback in the post-treatment period and a variation that did not receive reminders and feedback in the post-treatment period. We do not find significant differences in the post-treatment effects of the two groups.

nights. During the treatment period, the intervention increases the rate of sleeping at least seven hours on weeknights by an estimated 11.5 percentage points ($p < 0.001$), 26 percent higher than baseline. The treatment effects persist into the post-intervention period but are smaller: an estimated 5.2 percentage points ($p < 0.001$), a 12 percent increase compared to baseline. We estimate that average weeknight sleep increases by 19 minutes during the treatment period and eight minutes during the post-treatment period ($p < 0.001$).²

Turning to the educational impact of sleep incentives, Immediate Incentives improve average semester course performance by an estimated 0.075 - 0.089 grade points ($p = 0.044$ and $p = 0.034$, respectively), a 0.10 - 0.11 standard deviation (SD) increase. We find evidence of similar sized treatment effects on grade point average in the semester following the intervention, but no impact two semesters after the intervention. Treatment effects are largest in classes that take place midday and, in exploratory analysis, are driven by impacts in STEM courses.

We next examine potential mechanisms for the impact of our intervention on academic performance. We find evidence that Immediate Incentives not only increase sleep hours via earlier bedtime but also sleep regularity, which persists after the intervention ends. Examining time use, the intervention leads to declines in self-reported screen time, which includes internet browsing, TV/videos and games, and excludes screen time for studying. The changes in screen time are similar in magnitude to the increases in sleep and are concentrated around bedtime. We find that total study time does not change during the intervention period, but there is suggestive evidence of a reallocation of study time from evening hours to morning hours, with little change in other time use. We do not find treatment effects on physical activity, or end-of-semester mental health, though we find evidence that treated participants report they are better able to cope with stress during the intervention period. We also find no evidence that the effects on performance are due to direct effects of receiving cash rewards. Together, our results suggest that incentives to sleep lead to more regular sleep habits, which displace screen time, and shift study time to earlier hours of the day, which may contribute to the improvement in academic performance.

Finally, we explore sleep habits and mechanisms of habit formation. As noted above, we find persistent but smaller increases in sleep in the post-intervention period, which lasts an average of four weeks. We also observed sustained improvements in sleep regularity, suggesting that the intervention led participants to establish more stable routines. However, we find little evidence that these routines are triggered by the intervention's external cue to go

²Our focus on weeknight sleep is in line with prior work that examines the impact of school and class start times, which occur on weekdays. We find no evidence of substitution between incentivized weeknight sleep and unincentivized sleep, including sleep that occurs during the day (i.e., naps), on weekends and during holidays.

to bed earlier, as there is no lasting impact on average bedtimes. Comparing our primary Immediate Incentives intervention to secondary treatments, we find that immediate incentives significantly outperform variants with delayed or no rewards during the intervention period. However, in the post-intervention period, the effects of the primary and secondary treatments are not statistically distinguishable. Together, our results suggest that interventions targeting sleep can shift bedtime habits, though it is less clear whether these habits become truly “automatic.”

Our study is the first to show that an intervention targeting sleep can improve academic performance. Our findings contribute to the large literature on improving educational outcomes, particularly among college students (e.g., [Angrist et al., 2014](#); [Lavecchia et al., 2016](#), provide reviews). Compared to previously examined policies, such as financial aid, mentoring and support services, and performance-based incentives, our intervention is highly cost-effective. For example, in a recent meta-analysis, [Lintner \(2024\)](#) estimates that performance-based financial incentives improve college students’ grade point average (GPA) by an average of 0.041 grade points with average costs of about \$460 per student per semester. By comparison, we estimate that Immediate Incentives to sleep improve GPA by about twice the effect size (0.075 - 0.089 grade points) at less than one-quarter of the cost (about \$112 including the cost of the Fitbit). Our results suggest that improving academic achievement through sleep interventions may be more cost-effective than incentivizing performance directly. This finding is akin to recent work showing that incentives for exercise can improve educational achievement ([Cappelen et al., 2017](#)).

We also contribute to the growing literature on the economics of sleep. Seminal studies using time-use surveys find a negative association between sleep and work hours, though they do not establish causality ([Biddle and Hamermesh, 1990](#); [Basner et al., 2007](#)).³ Studies using naturally occurring data reveal that later sunset times are associated with lower cognitive performance, poorer educational outcomes, reduced earnings, and worse physical and mental health. These effects are primarily attributed to reduced sleep ([Giuntella et al., 2017](#); [Gibson and Shrader, 2018](#); [Giuntella and Mazzonna, 2019](#); [Jin and Ziebarth, 2020](#); [Lindquist and Sadoff, 2023](#), provide a review). In the context of education, U.S.-based estimates suggest that a one-hour later shift in sunrise or class start time increases sleep by an average of 6 - 36 minutes and has either no discernible impact on academic achievement or leads to an improvement in grades and test scores by 0.06 - 0.16 SD ([Carrell et al., 2011](#); [Heissel and Norris, 2018](#); [Groen and Pablonia, 2019](#)). The causal impact of our intervention aligns with

³Related work finds a positive correlation between sleep and health outcomes ([Cappuccio et al., 2010](#)), as well as sleep and academic performance ([Creswell et al., 2023](#)). Other studies show that sleep deprivation affects decision-making, ethical behavior, social decisions, and voting behavior ([Dickinson and McElroy, 2017](#); [Dickinson and Masclet, 2023](#); [McKenna et al., 2007](#); [Holbein et al., 2019](#)).

these findings from natural experiments: it increases weeknight sleep by an estimated 19 minutes during treatment and eight minutes post-treatment, and grades by 0.10 - 0.11 SD.

The non-experimental studies do not exogenously vary sleep or test policies aimed at improving sleep habits. Moreover, as illustrated in Figure A.1, there is a dearth of well-powered experimental studies that measure the impact of sleep on either labor market or academic performance.⁴ Sleep laboratory studies find that short-term severe sleep deprivation worsens cognition and mood, but cannot estimate the effect of moderate sleep improvements in natural settings (Banks and Dinges, 2007; Killgore, 2010; Lim and Dinges, 2010). Avery et al. (2022), Barnes et al. (2017) and Breig et al. (2020) examine sleep in field experimental contexts but do not link sleep to productivity and performance. The only prior field experiment testing the impact of sleep interventions on performance does not find an impact of increased nighttime sleep on productivity among highly sleep-deprived workers in India (Bessone et al., 2021). By contrast, in our study, as in the broader U.S. context, average sleep is of higher quality and baseline sleep is closer to the recommended minimum of seven hours a night. Our study also takes place over a longer time horizon, which may better capture the effects of moderate, sustained increases in sleep on performance.

Lastly, we contribute to the literature on habit formation by studying interventions that combine cues, feedback, and rewards, which prior work has largely examined separately. For example, Wellsjo (2021) and Byrne et al. (2022) focus on cues and feedback, while studies leveraging incentives typically distribute rewards with a delay (Gneezy et al., 2011; Royer et al., 2015; Hussam et al., 2022; Beshears et al., 2021; Milkman et al., 2014). We show that making rewards immediate significantly increases their impact on repeated behaviors, extending prior findings that immediate rewards outperform delayed rewards for one-time actions (Levitt et al., 2016). Finally, our examination of habit formation suggests that participants establish their own routines rather than relying on externally provided bedtime cues. These results align with prior research showing that interventions allowing for flexibility tend to be more effective (Beshears et al., 2021).

The remainder of the paper is structured as follows. In Section 2, we describe the experimental design, data and analysis. Section 3 presents the the treatment effects on sleep hours and academic performance. Section 4 examines mechanisms for the effect of our intervention on educational outcomes. Section 5 discusses mechanisms for habit formation in sleep. Section 6 benchmarks the results relative to prior findings, and Section 7 concludes.

⁴The figure summarizes 123 studies referenced in books and leading articles on sleep (Attia, 2023; Huffington, 2016; Bessone et al., 2021; Creswell et al., 2023) including their sample size, citation count, study type (field experiment, lab experiment, non-experimental) and outcome measures (whether they include educational and/or labor market outcomes).

2 Experimental design and data

We conducted our experiment in seven semester-long waves from Spring 2019 to Spring 2022 among students at the University of Pittsburgh (Pitt). We measured sleep using wearable trackers and delivered our interventions targeting sleep via text messages and a custom smartphone app. Our outcome data come from the wearable trackers (sleep and physical activity), survey measures (time use, performance on creativity and quantitative tasks, and well-being); and administrative records (academic transcripts).

2.1 Wearable trackers and custom smartphone app

To gather objective measures of sleep in a natural setting, we had participants in our study wear Fitbits that estimate sleep patterns based on movement and heart rate data. The use of such wearable trackers allowed us to depart from dependence on sleep diary methods, which have been shown to significantly overestimate sleep (Lauderdale et al., 2008; Bessone et al., 2021). Fitbits, which are among the most popular wearable trackers, are well-suited for monitoring sleep in natural settings due to their portability and unobtrusiveness, and are the most utilized wearables for biomedical research purposes (Wright et al., 2017). In our study, we used Fitbit Charge HR, Charge HR 2, Charge HR 3, Alta HR, and Inspire 2, which all capture both movement and heart rate. Recent studies have validated the accuracy of heart rate-enabled Fitbits for population-based sleep studies when compared to actigraphy, a widely used method for outpatient sleep screening (Haghighy et al., 2019). In the Supplementary Online Appendix (Giuntella et al., 2024), we discuss in more detail the limitations and advantages of wearables like Fitbits to capture detailed sleep metrics.

One source of concern with studies that rely on wearable trackers is that the devices require continued engagement via daily syncing (i.e., regularly connecting the tracker to a smartphone to update the collected data). To ensure high sync levels for our study, we developed a custom-made smartphone app that connected to the Fitbit API, which allowed us to monitor sync rates daily and notify participants with low sync rates in order to keep them engaged. The custom-made app also allowed us to deliver our interventions to improve sleep habits via push notifications and the app itself. The app features included the ability to send reminders; provide immediate individualized feedback based on participants' sleep as measured by the Fitbit; and redeem rewards. We discuss the interventions in more detail in Section 2.3

2.2 Sample, recruitment and timeline of the experiment

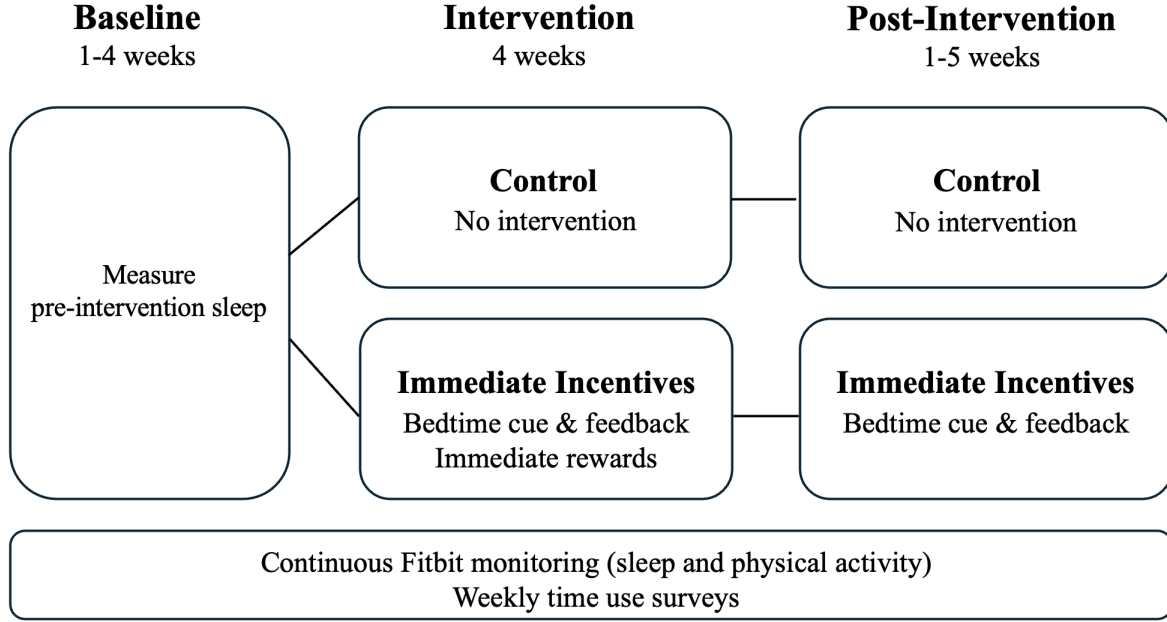
The experiment took place at the University of Pittsburgh, was approved by the University of Pittsburgh Institutional Review Board and was pre-registered in the [AEA RCT registry \(AEARCTR-0003235\)](#). We discuss deviations from the pre-registration plan in Appendix C.

We recruited participants through the Pittsburgh Experimental Economics Laboratory (PEEL) and invited them to participate in a semester-long study on wellness for a guaranteed minimum payment of \$30 by the end of the semester, including a \$6 upfront payment for the initial enrollment session, and the opportunity to receive additional earnings based on luck, as well as their choices during the experiment. To be eligible for our study, participants had to have a smartphone and be willing to wear and routinely synchronize a wearable device (Fitbit) during the semester. We began the experiment in Spring 2019 and enrolled participants every semester (Fall and Spring) until Spring 2022. We ran the experiment in seven consecutive waves, with modest-sized cohorts to accommodate the number of participants we could recruit through the lab every semester, as well as the number of Fitbits we had. Our final sample includes 1,149 participants.

Figure 1 shows the timeline of the study. In each wave, the study lasted for approximately 10 weeks. We initiated participant recruitment in the first few weeks of the semester and enrolled participants in the experiment on a rolling basis. Upon enrollment, we measured baseline sleep for one to four weeks. In our analysis, we restrict the baseline to the two weeks before the start of the intervention because fewer than half of subjects have more than two weeks of pre-intervention data. At the end of the baseline period, we randomly assigned participants to either a control group or treatments designed to improve sleep habits, which lasted for four weeks (*intervention period*). After the 4-week intervention period, we continued to follow participants for an additional 1-5 weeks until the end of the semester (*post-intervention period*), at which point we asked them to return the Fitbit and fill out an endline survey. The median number of weeks in the post-intervention period is four and 70% of participants have at least four weeks of post-intervention data. The study always ended during the last week of classes, before final exams. Across different waves, the start of the recruitment period depended on laboratory availability. This affected the timing of the treatment period and the length of the post-treatment period, which varied in each wave due to differences in recruiting start dates, the speed of participant enrollment, and the semester schedule. Importantly, the treatment itself was always four weeks long. The timeline of the experiment for each of the seven waves is depicted in Appendix Figure A.2.⁵

⁵In Fall 2019, due to recruitment issues at PEEL, we recruited two groups of participants and had them start the intervention in a staggered way, as shown in Figure A.2. In Spring 2020, the semester schedule was changed by the university closure prompted by the onset of the COVID-19 pandemic. Students enrolled in

Figure 1: Timeline of the experiment



To enroll in the study, participants completed an initial session at the laboratory (Spring 2019 - Spring 2020) or over Zoom (Fall 2020 - Spring 2022), completed an intake survey, received the Fitbit and installed our custom smartphone app.⁶ During the intake session, participants consented to wear and sync the Fitbit throughout the semester, answer weekly surveys, and grant us access to their academic records. They were informed about their right to withdraw from the study at any time with no penalty. Participants left the initial session with a one-page reminder outlining what was expected of them during the study and agreed to return the Fitbit at the end of the semester. The intake survey administered to participants

the study in Spring 2020 learned about the university moving to remote learning during spring break (mid-March 2020), and continued to stay enrolled in the study until the end of the semester. In the Appendix, we conduct sensitivity analyses that exclude the Spring 2020 wave.

⁶From Fall 2020 onwards, we adjusted some of the intake procedures due to changes in the lab and university protocols during the COVID-19 pandemic. In March 2020, Pitt shifted to remote instruction for the remainder of the term due to the onset of the COVID-19 pandemic. In Fall 2020, Pitt used a “Flex@Pitt” model, combining in-person and remote instruction with the option for students to attend fully online depending on their preferences and safety concerns. In-person activities were limited. In Spring 2021, The Flex@Pitt model continued, with most courses remaining hybrid or fully online. Instead of filling out one unique survey during the intake session, the survey was split into an enrollment survey that participants filled out at enrollment while on Zoom and a follow-up survey that was emailed to them a few days later. In Spring 2019-Spring 2020 and Fall 2021-Spring 2022, participants picked up the Fitbit from PEEL and received a \$6 payment. In Fall 2020 and Spring 2021, participants received the Fitbit via mail as the University of Pittsburgh combined in-person and remote instruction with the option for students to attend remotely depending on their preferences and safety concerns.

collected information on socio-demographic characteristics and baseline measures of well-being.

Over the course of the study, participants in all treatments received reminders to sync their Fitbit via text message and the app (see Figure B.5). They also received weekly surveys that captured time use, performance on creativity and quantitative tasks, and well-being. We describe the survey measures in more detail below (Section 2.4).

2.3 Treatments

In total, 1,219 individuals completed an enrollment survey. In order to be randomized, participants had to have at least one day of Fitbit data in the baseline period.⁷ At the end of the baseline period, we randomized 1,149 participants to treatment groups, which are displayed in Table 1. Participants in the *Control* group ($N = 380$, waves 1-7), received no intervention and continued to wear their Fitbits and fill out surveys until the end of the semester. Participants in the treatment groups received interventions to improve sleep habits.

In all treatments, we set the goal of sleeping at least seven hours per night by 9 am on weeknights (Sunday through Thursday). We established 9 am as a key constraint, based on previous studies emphasizing the significance of sleep timing and the alignment of biological rhythms with the environmental light-dark cycle (Roenneberg and Merrow, 2016). Additionally, we aimed to reduce the likelihood that our intervention would encourage skipping classes scheduled at 9 am. Notably, about 80% of the participants wake up before 9 am at baseline.

Drawing on the habit formation framework outlined earlier, our *Immediate Incentives* intervention (468 participants, waves 1-7) leverages cues, rewards, and repetition to establish persistent sleep habits. To provide participants with a consistent cue, we sent them reminders—both through the app and via text—to meet their target goal of sleeping seven hours per night by 9 am every weeknight (Sunday - Thursday). These reminders had two major components. We established a personalized target bedtime for each participant, an hour earlier than their usual baseline bedtime, based on their individual sleep patterns, and sent reminders to go to bed half an hour before this new goal time. Figure B.1 displays the bedtime reminder.⁸ Second, as the cue-based framework emphasizes the importance of a

⁷We mistakenly assigned eight participants to treatments who did not have any baseline Fitbit data. We conduct sensitivity analyses that exclude these individuals and results do not change (see Section 3).

⁸The bedtime was set approximately an hour before participants' average baseline bedtime rounded to the nearest 30 minutes with a latest bedtime goal of 1 am (e.g., for participants with an average baseline bedtime of 12-12:14 am, we set a goal bedtime of 11 pm; for participants with an average baseline bedtime of 12:15-12:30 am, we set a goal bedtime of 11:30 pm; for participants with an average baseline bedtime of

Table 1: Treatments

	N	Waves	Treatment		Post-Treatment
			Reminders & Feedback	Rewards	Reminders & Feedback
Control	380	1–7	–	–	–
Immediate Incentives	468	1–7	✓	Immediate	✓
<i>Immediate Incentives, Post Cue/Feedback</i>	356	1–7	✓	Immediate	✓
<i>Immediate Incentives, No Post Cue/Feedback</i>	112	5,7	✓	Immediate	–
Total (Main)	848				
Secondary Treatments					
Delayed Incentives	103	1–3	✓	Delayed	✓
Delayed Incentives, No Cue/Feedback	97	1–3	–	Delayed	–
Cue/Feedback	101	1–3	✓	–	✓
Total (All)	1,149				

Notes: The table reports the number of participants enrolled in each of the treatments; whether rewards were immediate or delayed, and whether they received reminders and feedback during and after the intervention. Immediate Incentives pools Immediate Incentives, Post Cue/Feedback and Immediate Incentives, No Post Cue/Feedback.

stable environment in triggering automatic behavior, we encouraged participants to engage in a specific bedtime behavior every weeknight before going to sleep. Participants selected their behavior from a menu of different options before the beginning of the intervention period. Examples included “Turn off your Phone”, “Turn on bedtime music”, “Turn off your computer”, “Turn on meditation app”.⁹

Next, to link sleeping behavior with a reward, we provided participants with immediate financial incentives upon meeting their sleep goal. Every weekday after 9 am, participants received feedback on whether they met their goal of sleeping at least seven hours via the app through push notifications and the app interface. Participants who met their goal received feedback about having achieved the goal and earned a \$4.75 reward, which they redeemed by clicking a button on the app (see Figure B.2.¹⁰ Participants in this treatment received a monetary reward of \$4.75 through a Venmo transfer on the same day.¹¹ Participants who fell short of the sleep target were given feedback indicating that they had not achieved their goal and had missed out on receiving the reward. This feedback also included a negatively-valenced emoji to convey the injunctive message that sleeping less than seven hours was discouraged (see, e.g., [Schultz et al., 2007](#)), and encouragement to make another attempt

2 am or later, we set a goal bedtime of 1 am). We delivered the personalized bedtime reminder via text message. In the app, we delivered a standard message encouraging participants to go to bed early enough to sleep seven hours by 9 am.

⁹On the Friday before the beginning of the intervention period, participants received their intervention-related instructions. As part of these instructions, we asked participants to select a bedtime behavior to engage in before going to bed.

¹⁰Redemption rates were above 95% across waves.

¹¹For logistical reasons, the payment was received after 3 pm each day, which introduced a small delay between the performance of the behavior and the reward. However, the feedback about receiving a reward was provided as soon as participants synced the Fitbit after 9 am.

to meet the sleep target. To encourage repetition of the incentivized sleep behavior, cues and rewards continued every weeknight and weekday of the four-week intervention period. In waves in which the treatment period spanned spring break, we paused the intervention during spring break. Results are unchanged when we restrict the analysis to terms where the intervention was not paused (see the Supplementary Online Appendix (Giuntella et al., 2024)).

At the end of the intervention period, we discontinued the financial rewards and notified participants via text message. In our main variation of the Immediate Incentives treatment, *Immediate Incentives – Post Cue/Feedback* ($N = 356$, waves 1-7), we continued sending participants bedtime reminders (i.e., the cue) and morning feedback on whether they had achieved their sleep goal throughout the post-intervention period, which lasted until the end of classes. The feedback remained identical to that of the intervention period, except that we removed mention of the financial reward, as displayed in Figure B.4. To examine the importance of maintaining the cue for habit persistence, we tested a variant of the Immediate Incentives treatment in which participants did not receive cues and feedback in the post-intervention period, *Immediate Incentives – No Post-Cue/Feedback* ($N = 112$, waves 5 and 7). Our primary analysis pools the two variants of Immediate Incentives. In waves in which the post-treatment period spanned Thanksgiving, we paused reminders and feedback during the week of Thanksgiving. Our primary analysis excludes holiday weeks (spring break and Thanksgiving).

Secondary treatments. Following our pre-analysis plan, in the first three waves of the study, we implemented secondary treatments to examine the importance of cues and feedback, and the timing of financial incentives for habit formation.

In the *Delayed Incentives* treatment ($N = 103$, waves 1-3), we provided participants with cues, feedback and rewards, as in the Immediate Incentives treatment. The only difference was that, although feedback about receiving the incentive was immediate, payment was not. Participants learned each day whether they met their sleep goal, but only received a single transfer with the total payment at the end of the study period, one to five weeks after the intervention ended. Figure B.3 displays the feedback screens for the Delayed Incentives treatment. In the post-intervention period, participants continued to receive the bedtime cue and morning feedback, as in the Immediate Incentives treatment. This treatment aimed to test whether providing repeated immediate rewards increases their effectiveness during treatment compared to delayed rewards, as has been shown with one-time rewards (Levitt et al., 2016); and whether reinforcing behavior with immediate rewards enhances persistence of behavior after the reward is removed, more so than delayed rewards.

In the *Delayed Incentives – No Cue/Feedback* treatment ($N = 97$, waves 1-3), we removed the bedtime reminders (i.e., the cue) and the daily feedback about whether participants met their sleep goal but retained the financial incentive. At the start of the intervention period, we informed participants that they would receive \$4.75 for every night they met the goal of sleeping at least seven hours by 9 am, with payment to be received via a Venmo transfer at the end of the semester. The participants did not receive reminders or feedback during the post-intervention period. This treatment is analogous to other work using financial incentives to create habits in the context of exercising (e.g., [Charness and Gneezy, 2009](#); [Royer et al., 2015](#)) and aimed to test the importance of pairing cues and feedback with rewards.¹²

To test whether financial rewards are critical for establishing habits, we also conducted an additional treatment, *Cue/Feedback* ($N = 101$, waves 1-3), where we removed the financial reward. Participants in this treatment received the same bedtime reminders as participants in the Immediate Incentives and Delayed Incentives treatments. They also received daily feedback via the app on whether they had achieved their sleep goal. Instead of providing participants with a reward, the feedback screen included a positively- or negatively-valenced emoji depending on whether participants had achieved their sleep goal – i.e., the same feedback that the Immediate and Delayed incentives groups received in the post-intervention period (see Figure B.4). The participants in the Cue/Feedback treatment continued to receive the same reminders and feedback throughout the post-intervention period.

2.4 Data

2.4.1 Sleep

Our primary pre-registered sleep outcome is the share of weeknights (Sunday - Thursday) participants sleep at least seven hours. Our secondary measures of sleep include sleep hours per night, sleeping seven hours per night and sleeping between seven and nine hours without restricting to weeknights, sleeping seven hours per night including naps, bedtime, wake-up time, sleep regularity, and sleep quality as measured by the Fitbit. In exploratory analyses, we also analyze sleeping between seven and nine hours on weeknights and sleeping at least six hours on weeknights in order to better compare these outcomes to our primary outcome measure. As a secondary measure, we also measured self-reported sleep and desired sleep in the intake survey.

We study sleep regularity in two ways. First, we use the sleep regularity index (SRI) (Fis-

¹²We did not test immediate incentives without providing cue/feedback because delivering immediate rewards inherently requires providing feedback on whether the goal was met. However, in the post-intervention period, we tested the effect of removing the cue/feedback for immediate incentives and, as we note, find little evidence that the post-cue/feedback had any significant impact (Appendix Table A.3 panel B, column 1).

cher et al., 2021; Phillips et al., 2017; Windred et al., 2024), which measures how similar a person’s sleep-wake cycles are from one day to the next using binary classifications of sleep and wake states at the minute-level. The SRI is a percentage score that ranges from 0 to 100, where higher values indicate more regular sleep patterns. It is calculated based on the presence or absence of sleep at each minute interval over a 24-hour period, comparing day-to-day variations. Second, we focus on a measure of variation or dispersion across the week, as measured by the within-person standard deviation in the outcome of interest (Fischer et al., 2021).

We measure sleep quality in terms of efficiency, Rapid Eye Movement (REM) sleep and deep sleep. Efficiency measures the percentage of time in bed that an individual is asleep. REM sleep is the stage of sleep in which individuals dream, which stimulates areas of the brain essential to learning. During REM sleep, heart rate and blood pressure rise. Studies suggest that REM sleep plays a key role in memory consolidation, emotional processing, and brain development (Marks et al., 1995; Boyce et al., 2016). Deep sleep is the most restorative form of sleep. During deep sleep the heart rate and breathing rate are at their lowest and our body repairs tissue. Deep sleep is important for regulating glucose metabolism and has also been linked to cognitive function and memory (Zhang and Gruber, 2019; Leproult and Van Cauter, 2010). We caution that while, as described above, there is growing evidence on the performance of recent Fitbit models in accurately measuring sleep duration (de Zambotti et al., 2018), the accuracy and reliability of these devices in capturing sleep stages needs further validation. In particular, sleep trackers have acceptable sensitivity but poor specificity when compared with sleep stages obtained using polysomnography (PSG). Given these limitations, sleep quality measures should be interpreted with caution. Furthermore, short naps (less than an hour) are not accurately recorded by Fitbit devices.

Sync rates throughout the study were relatively high. On average participants synced their devices for 86% of the days. As shown in Table A.2, there are higher sync rates during the intervention period among the Immediate Incentives group compared to the Control group (there are no significant differences in sync rates in the baseline and post-intervention periods). As noted above, 8 of the 1149 participants did not have any Fitbit data, including at baseline. Of the 1141 participants with baseline data, 24 did not report any data during treatment (2%) and 89 (8%) did not report any data in the post-intervention period.¹³ In all of our analyses, for the nights with missing data, we replace missing data with an individual’s baseline average following the approach of Bachiredy et al. (2019). We also conduct sensitivity analyses that do not replace missing data and report Lee (2009) Bounds

¹³Differences in syncing rates between the secondary treatments and the control group are reported in the Supplementary Online Appendix (Giuntella et al., 2024).

for treatment and post-intervention effects. Results do not meaningfully change (Table A.3).

One concern with using Fitbits is that participants might lend their Fitbit to someone else for several nights to manipulate the reward system. To mitigate this potential concern, we make use of the resting heart rate data collected by the Fitbit. Research indicates that although resting heart rate can vary greatly between different people, it usually remains fairly stable within the same individual over time (Quer et al., 2020). We do not find a significant link between unusual fluctuations in resting heart rate (deviating more than one standard deviations from baseline) and treatment assignment (p -value= 0.82). We also find no evidence of data manipulation, such as heaping, rounding in recorded sleep times or manual logging of sleep (see the Supplementary Online Appendix for further discussion on wearables and measurement error (Giuntella et al., 2024)).

2.4.2 Educational outcomes

Our primary educational outcome is term Grade Point Average (GPA), measured using administrative data obtained on September 14, 2023. The Registrar’s Office at the University of Pittsburgh supplied us with course data for participants enrolled in our experiment, covering each semester of their enrollment at the university from Fall 2018 through Spring 2023.¹⁴

This dataset provides comprehensive course information for our experimental participants across three key periods. In addition to data from the term during which our intervention took place (our primary outcome measure), it includes information from both before and after the intervention. Specifically, we have data for the term immediately preceding the intervention if participants were still enrolled at Pitt, as well as their high school GPA. We also have data for at least two terms following the intervention. Access to this post-intervention data allows us to conduct exploratory analyses on the intervention’s longer-term impact, provided the student enrolled in graded classes during subsequent semesters.

We calculate term GPA based on all courses in which a student received a letter grade, A+ through F, converted to a 4-point scale (see Table A.1 for the grading system). Term GPA is an average of the course grade points, weighted by the number of credits for each course. Our secondary outcome measures include course completion and credits earned; we also collected exploratory measures of withdrawals, course failures and course pass rates. Eighty-eight percent of the courses in our data receive a letter grade on the 4-point scale. Our main analysis excludes courses with grades outside this scale. We include these courses

¹⁴Because the administrative data was obtained after the intervention was concluded, we did not know the distribution of baseline GPA or GPA by course type in advance of the study. At the time of the pre-registration, we also did not know how many semesters of data and which secondary administrative data on educational outcomes would eventually be made available to us at study completion.

when examining the likelihood of having any grade, number of credits earned, as well as in the exploratory analysis examining course withdrawal, failure, and pass rates.

Of the 1,149 participants in our experiment, 1,128 have at least one course grade for the term of the intervention (98% of the sample). The 21 remaining participants had no available grades for the term of the intervention, but have academic records in other terms. Our analysis includes all available grade data for the relevant term. For 1,049 participants (91% of the sample) we have data on at least one course grade for one term after the intervention; for 968 participants (84% of the sample) we have at least one course grade for two terms after the intervention. From the Registrar data on participants' high school GPA, we could match 1,049 students (91% of the sample). For 74 of the 100 students with no high-school GPA, we have information on baseline GPA at the start of the term (cumulative GPA from all prior terms). This gives a total of 1,123 participants with baseline GPA (98% of our sample).

The match rates are similar if we limit the data to our primary analysis comparing the Control and Immediate Incentives groups. Of these 848 participants, we match 833 to course grades in the term of the intervention (98%), 784 to high school GPA (92%) and an additional 41 to a prior term GPA. We construct a baseline GPA variable from either prior semester GPA or, when not available, high school GPA. A total of 825 participants have a baseline GPA (97%). As shown in Table A.2, we have a higher proportion of baseline grades for the Immediate Incentives group than for the Control group. In sensitivity analysis, we limit the sample to participants with baseline GPA and results are similar (Table A.6). There is no difference between the groups in the likelihood of having course grades, which is our primary outcome measure (Table A.2).

The course data allow us to classify courses by class type and start time. Class types include lectures, seminars, credit laboratories, practicums, workshops, independent studies, directed studies, internships, and laboratories. Lectures comprise 80% of the classes. As shown in Figure A.3, non-lecture classes have significantly higher grades and lower variance than lecture classes. The average GPA (and standard deviation) in lectures is 3.43 (0.81) compared to 3.74 (0.54) in other classes. In lecture classes, 47% of students receive the highest possible grade and the lowest quartile is a B. By comparison, in non-lecture classes, 67% of students receive the highest possible grade and the lowest quartile is an A-. This raises concerns that the grading system in non-lecture courses leaves little scope for treatment effects. In our analyses, we therefore report estimated treatment effects for all course types together, as pre-registered, and an exploratory analysis with lectures alone (we report the effects for non-lecture courses in Table A.6).

In exploratory analysis, we also classify each course as STEM or non-STEM using the

Department of Homeland Security 2023 list of STEM-designated CIP codes.¹⁵ The average GPA (and standard deviation) in STEM classes is 3.26 (0.89), while it is 3.69 (0.58) in non-STEM classes. In STEM classes, 39% of students receive the highest possible grade and the lowest quartile is a B. By comparison, in non-STEM classes, 61% of students receive the highest possible grade and the lowest quartile is an A-. Similar to non-lecture courses, the grading in non-STEM courses may limit the scope for treatment effects.

2.4.3 Additional outcomes

Time use. We implemented a time use survey once a week, rotating the weekday on which the survey was administered. Our time use measure follows the structure of American Time Use Survey (Abraham and Flood, 2009). From a drop-down menu, participants indicated how they allocated their time on the previous day. For each 30 minute interval over the course of 24 hours, participants could choose from a list of activities that included sleeping, grooming (self), watching TV/videos, surfing the internet, playing games, working, studying, preparing meals or snacks, eating or drinking, cleaning, laundry, grocery shopping, attending religious services, hanging out with friends, paying bills, exercising, commuting, or other activities. They could also indicate that they did not know or could not remember how they spent their time, or could refuse to respond. In our primary analysis, we examine “screen” time, which pools time spent watching TV/videos, surfing the internet and playing games and excludes screen time spent studying; we categorize time spent hanging out with friends as “social” time. We exclude from the analysis responses that report 24 hours of “other activities,” which may reflect inattention in filling out the time use survey.

Creativity and quantitative tasks. We collected secondary measures of performance through creativity and quantitative tasks. We drew the math questions from the math section of the Graduate Record Examination (GRE) test. We measured creativity using an adapted version of the task employed by Charness and Grieco (2019), where we provided participants with a list of 10 words and asked them to use some or all of the words to write an interesting sentence. On alternate weeks, the weekly time use survey included either one multiple choice math question or one creativity task. Both tasks were incentivized (see instructions in Appendix B). To assess the creativity task, we recruited raters from lab participants at the University of California San Diego and from Prolific ($N = 1,369$), and four undergraduate research assistants at the PEEL lab at the University of Pittsburgh. Raters received a random subset of the sentences produced by participants in the creativity task and rated them on a 1-5 scale. Each sentence was rated by a minimum of two raters; the

¹⁵<https://nces.ed.gov/ipeds/cipcode/Files/2023/Final-2023-CIP-STEM-List-Blog.pdf>

median number of ratings per sentence is thirteen.

Physical health. From the Fitbits, we collected data on resting heart rate and physical activity (daily steps and active minutes). Resting heart rate measures heart beats per minute (BPM) at rest (i.e., when sitting, lying down or relaxing). Faster resting heart rates are associated with shorter life expectancy (Cooney et al., 2010; Dyer et al., 1980). Daily steps are the number of steps over the course of a 24 hour-period. Active minutes are measured as minutes in which a person is non-sedentary for a least 10 continuous minutes, where non-sedentary is defined as activity that raises heart rate enough to burn at least 3 metabolic equivalents (METs).¹⁶

Well-being. We collected measures of mental health in the intake survey (conducted upon enrollment) and in the endline survey at the end of the semester. We assessed depression using the Center for Epidemiologic Studies Depression scale (CES-D, Radloff, 1977), which is a 20-item validated instrument designed to assess the frequency of depressive symptoms on a scale from 0 (“Rarely or none of the time”) to 3 (“Most or almost all the time”). An overall depression score is calculated by summing answers to all 20 items, with total scores ranging from 0 - 60. We also measured anxiety using the Generalized Anxiety Disorder scale (GAD-7, Williams, 2014), a 7-item scale designed to assess symptoms of Generalized Anxiety Disorder. The instrument assesses the frequency of anxiety-related symptoms using a scale ranging from 0 (“Not at all”) to 3 (“Nearly every day”), with total scores ranging from 0 - 21. To measure well-being, we collected exploratory measures of mood, stress and ability to cope with stress (resilience). For mood, we asked participants to indicate, on a 10-point Likert scale, how happy they felt in that moment. For stress and resilience, participants indicated, using a 5-point Likert scale, 1) the extent to which they faced stress in their life at the time of answering the survey and 2) the extent to which they felt able to deal with the stress they were facing. Every week, we alternated between the mood and the stress/resilience questions. These measures were collected via text message and, each week, participants were randomly assigned to receive the text message at different times of the day (11 am, 4 pm, 9 pm).

As shown in Table A.2, there is no difference in attrition rates between the Immediate Incentives group and the Control group for the additional outcomes discussed above.

¹⁶In practice, this measure sums the “lightly active”, “fairly active” and “very active” minutes collected by the Fitbit.

2.5 Randomization and baseline characteristics

Randomization occurred at the end of the baseline period, the weekend before the start of the intervention period. We employed a block randomized design, stratifying our participants by gender and the share of weeknights participants slept more than seven hours (above vs below median).¹⁷ In the initial waves of the study (Spring 2019 to Spring 2020) we randomized participants to one of five groups with equal probability (Control, Immediate Incentives, Delayed Incentives, Delayed Incentives No Cue/Feedback and Cue/Feedback). For the remaining waves, we randomly assigned participants to either the Control group or the Immediate Incentives treatment. In Waves 5 and 7 (Spring 2021 and 2022), we randomized participants in the Immediate Incentives treatment to either receive or not receive cue and feedback during the post-intervention period (Immediate Incentives - Post Cue/Feedback or Immediate Incentives - No Post Cue/Feedback).

Table 2 compares baseline characteristics in the Control group (column 1) to the Immediate Incentives group (column 2). We report demographic characteristics, baseline sleep behaviors and baseline academic characteristics (we discuss the baseline sleep in Section 3.1).

Students in the Control group are on average about 19 years old, with a large share of freshmen (52%). Sophomore, junior, and senior and above students make up 12%, 23%, and 12% of the Control group, respectively. Female and Asian students are over-represented compared to the full-time Pitt student population, and the U.S. college population in general. Approximately 55% (58%) of Pitt (U.S.) students are women, while women make up 72% of the Control group. Asian students make up 11% (7%) of the Pitt (U.S.) student population, while they represent 28% of the Control group. White students, which make up 55% of the Control group, are slightly under-represented compared to the Pitt student population (68%) and slightly over-represented compared to the U.S. college population (52%). The share of Black (8.8%) and Hispanic (4.0%) students is representative of the Pitt student population but lower than the U.S. college population, in which 13% of students are Black and 22% are Hispanic.¹⁸ A quarter of the students in the Control group report their parents did not receive a college degree (either some or no college); 29% report that at least one of their parents has a college degree; and 45% report that at least one of their parents has a post-graduate degree.

About 58% of participants in the Control group report to be in STEM majors. They

¹⁷We did not balance the randomization on baseline GPA because, as discussed above, GPA data was not available at the time of randomization.

¹⁸Demographics for the 2021-22 U.S. college population are available at: <https://www.statista.com/statistics/236360/undergraduate-enrollment-in-us-by-gender>, accessed on November 18 2023. Demographics for the Pitt student population in 2021-22 are available at: https://www.ir.pitt.edu/sites/default/files/assets/CDS_2021-2022_Pittsburgh%20Campus_2.pdf, accessed on November 18 2023.

Table 2: Treatment-Control differences in baseline characteristics, Immediate Incentives

Variable	Control	Immediate Incentives	Difference
<i>Demographics</i>			
Female	0.721 (0.449)	0.726 (0.446)	-0.002 (0.031)
Age	19.463 (2.982)	19.344 (1.964)	-0.110 (0.170)
White	0.548 (0.498)	0.568 (0.496)	0.023 (0.034)
Asian	0.285 (0.452)	0.261 (0.439)	-0.017 (0.031)
Black	0.088 (0.283)	0.068 (0.253)	-0.021 (0.019)
Hispanic	0.040 (0.196)	0.053 (0.225)	0.010 (0.015)
Other	0.040 (0.196)	0.049 (0.216)	0.005 (0.014)
Highest parent educ:			
less than college	0.255 (0.437)	0.284 (0.452)	0.028 (0.031)
college	0.287 (0.453)	0.288 (0.454)	0.001 (0.032)
more than college	0.447 (0.498)	0.427 (0.495)	-0.018 (0.034)
<i>Baseline sleep outcomes</i>			
Sleep hours	6.625 (0.958)	6.659 (0.902)	0.012 (0.064)
Sleep ≥ 7 hours	0.438 (0.276)	0.426 (0.274)	-0.020 (0.019)
Sleep ≥ 6 hours	0.706 (0.258)	0.713 (0.258)	0.001 (0.018)
Bedtime	25.265 (1.313)	25.211 (1.297)	-0.073 (0.091)
Wake up time	7.956 (1.302)	7.935 (1.238)	-0.062 (0.086)
<i>Baseline academic characteristics</i>			
Freshman	0.521 (0.500)	0.530 (0.500)	-0.006 (0.031)
Sophomore	0.118 (0.324)	0.120 (0.325)	0.009 (0.022)
Junior	0.226 (0.419)	0.212 (0.409)	-0.012 (0.028)
Senior and above	0.124 (0.330)	0.139 (0.346)	0.020 (0.023)
STEM major	0.582 (0.494)	0.571 (0.496)	-0.004 (0.035)
Number of courses	5.167 (1.282)	5.158 (1.420)	-0.038 (0.095)
Number of early sessions	1.523 (1.562)	1.667 (1.545)	0.166 (0.109)
High-School GPA	4.140 (0.440)	4.131 (0.434)	-0.011 (0.032)
Baseline term GPA	3.429 (0.530)	3.457 (0.465)	0.016 (0.038)
<i>Joint p-value</i>			0.762
Observations	380	468	848

Notes: The sample is restricted to individuals in the Control and Immediate Incentives treatment groups. Early class sessions are classes starting at 10 am or earlier. All estimates in column 3 include wave fixed effects. Robust standard errors are in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

are enrolled in an average of 5.2 courses with an average of 1.5 class sessions per week beginning before 10 am (early classes). The average high school GPA in our sample of 4.14 is representative of the overall University of Pittsburgh student population: the interquartile range of students offered admission at Pitt in 2022 had a weighted average GPA ranging from 3.91 to 4.42.¹⁹

In column 3, we estimate the treatment-control difference for each baseline characteristic from a regression that includes an indicator for the Immediate Incentives group and wave fixed effects. We do not find any statistically significant differences between average baseline characteristics in the Control group and the Immediate Incentives group. We also estimate treatment-control differences for each treatment group separately in Giuntella et al. (2024) and find statistically significant differences at the expected rate (e.g., about five percent of tests are significant at the $p < 0.05$ level).

2.6 Analysis

For outcome measures that are observed repeatedly throughout the study (e.g., nightly sleep), our primary regression analysis estimates treatment effects during the intervention period and the post-intervention period relative to the Control group. Formally, we estimate the following OLS model, unless otherwise noted:

$$Y_{it} = \beta_1 D_i * T_t + \beta_2 D_i * P_t + X_i + \rho_t + w_t + \mu_t + d_t + \epsilon_{it} \quad (1)$$

where Y_{it} is the outcome measure of interest; D_i is an indicator equal to one if an individual was assigned to the treatment group of interest; T_t is an indicator equal to one for any observation during the four-week intervention period; P_t is an indicator equal to one for any observation in the post-intervention period; X_i includes an individual’s baseline value of the outcome variable, baseline sleep (percent of weeknights with at least seven hours of sleep)²⁰, baseline GPA, indicators for the number of classes starting before 10 am in a week (ranging from 0 - 5), and demographic controls for gender, age in years (dummies), race/ethnicity (Asian, Black, Hispanic, White, other), and indicators for parents’ highest education (less than college degree, which includes high school degree only or some college; college degree; or, more than a college degree). For all individual characteristics, we included a missing indicator for whether the variable is missing. The variables ρ_t , w_t , μ_t , d_t are fixed effects for the wave of the experiment, week of the experiment, month of the year, and day of the week, respectively. Standard errors are clustered at the individual level. For outcome measures

¹⁹Data available at: <https://admissions.pitt.edu/first-year-student/class-profile>, accessed on November 18 2023.

²⁰We exclude baseline sleep in regressions for sleep outcomes due to collinearity with the the baseline value of the outcome variable.

that are observed only once during the study (e.g., course grades), we estimate the following OLS model, unless otherwise noted:

$$Y_i = \beta_1 D_i + X_i + \epsilon_i \tag{2}$$

where the variables are as described above. In regressions on course grades, the level of observation is the course weighted by the number of credits. Standard errors are clustered at the individual level.

Our main analysis compares the Control group to the Immediate Incentives treatment. As pre-registered, we also present the analysis for the primary outcomes comparing the Control group to the pooled incentives treatments (see column 7 of Table A.3 and column 8 of Table A.6) column.

We report both unadjusted p -values and statistical significance adjusted for multiple hypothesis testing (MHT) within families of secondary measures (see Tables C.1 and C.2 in the Appendix for the families of outcomes). For that purpose, we use the method described in [Anderson \(2008\)](#), calculating Anderson False Discovery Rate (FDR) q -values and noting which estimates are robust to adjustment.²¹ In the Results section, we report unadjusted p -values and note which estimates are robust to adjustment.

In our main specifications, we include all participants who have outcome data. In the Appendix, we conduct sensitivity analyses that limit the sample to those who have both Fitbit and course grades data (Table A.3, column 3 and Table A.6, column 8).

3 Results

We first examine treatment effects on sleep hours. We then turn to the impact of our intervention on educational outcomes.

3.1 Sleep

Baseline. Data from the baseline (pre-intervention) period reveals three findings about our college student participants: (1) students are sleep deprived relative to recommended guidelines; (2) they overestimate their sleep; and (3) they would like to sleep more than they do.

First, as shown in Table 2, on weeknights, participants sleep an average of 6.6 hours, meeting the recommendation of sleeping at least seven hours on approximately 43% of weeknights

²¹Note that adjusted q -values can be both larger or smaller than unadjusted p -values. This is because, as noted by [Anderson \(2008\)](#), sharpened FDR q -values can be less than unadjusted p -values when many hypotheses are rejected.

and sleeping less than six hours on approximately 29% of weeknights. About half of the participants have an average bedtime after 1 am, and about a quarter go to bed after 2 am on average. These data indicate that sleep deprivation is prevalent in our sample, consistent with a recent report by the National Institutes of Health indicating that more than 70% of college students sleep less than eight hours per night (Hershner and Chervin, 2014). In our sample, participants sleep less than eight hours on approximately 83% of weeknights. As shown in Appendix Figure A.4 panel A, only 4% of the participants achieve the goal of sleeping at least seven hours on all five weeknights while only 10% meet the goal on four nights. About a quarter of participants meet the goal on only one night, and one-fifth meet the goal on no nights.

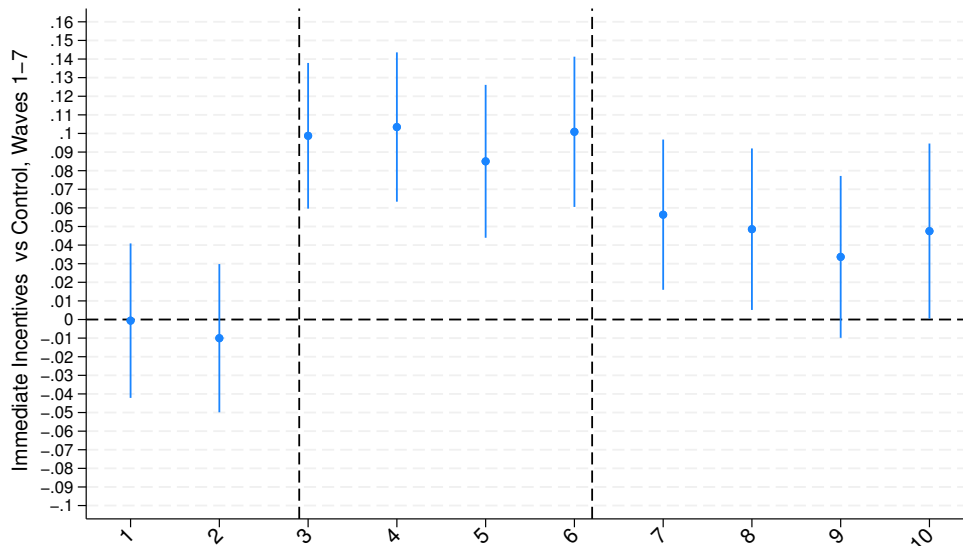
Second, while only about one-third of participants meet the seven hour sleep goal on at least three out of five weeknights (as measured by the Fitbit), over two-thirds report sleeping at least seven hours on a typical weeknight in the enrollment survey. This suggests that students systematically overestimate their sleep duration.

Third, participants would like to sleep more than they actually do—both in comparison to their Fitbit-measured sleep and their (over-estimated) self-reported sleep. On average, their stated optimal sleep time is an hour longer than their reported typical sleep duration. Comparing optimal to actual sleep, Appendix Figure A.4 panel B, shows that the distribution of optimal sleep is skewed to the right relative to actual sleep (as measured by the Fitbit), with 97% of the students reporting that their optimal weeknight sleep would be at least seven hours and the modal stated optimal sleep being eight hours. Appendix Figure A.4 panel C compares actual and optimal sleep hours at the individual level. Ninety-four percent of individuals fall above the 45-degree line, indicating that they sleep less than they believe they should. These results suggest potential scope for interventions that help individuals increase their sleep, as they state they would like to.

Intervention and post-intervention period. We first estimate treatment effects on the primary measure of sleep, which we incentivized: sleeping at least seven hours on weeknights (Sunday - Thursday). This outcome variable only includes nighttime sleep, and excludes weekends, holidays and naps (defined as episodes of sleep that start between 7 am and 8 pm). Figure 2 plots the estimated difference in the rate of sleeping at least seven hours between the treatment (Immediate Incentives) and Control groups, by week. The estimates are from regressions by week in which individual-nights are the level of observation and we include an indicator for the treatment group with no additional covariates (the Control group is the omitted group). Standard errors are adjusted for clustering at the individual level (the

bars in the figure indicate 95% confidence intervals).²² As shown in the figure, there are no differences at baseline (weeks 1-2). Treatment effects emerge in the first week of the intervention (week 3) and persist throughout the four-week treatment period (weeks 3-6). Treatment effects decline as soon as the intervention ends (week 7) but remain positive and fairly steady throughout the post-intervention period (weeks 7-10).

Figure 2: Immediate incentives and sleep ≥ 7 hrs (weeknights), excluding naps



Notes: The sample is restricted to weeknights (Sunday-Thursday nights). On the horizontal axis we report week of the study: baseline (weeks 1-2), treatment (weeks 3-6), post-treatment (weeks 7-10). The coefficient reports the difference in the likelihood of sleeping at least 7hrs between individuals in the Immediate Incentives treatment and those in Control by week. Standard errors are clustered at the individual level. Bars indicate 95% confidence intervals.

In Table 3, we present regression estimates of the treatment and post-treatment impacts of Immediate Incentives on sleeping at least seven hours on weeknights (column 1) and on weeknight sleep hours (column 2), following the specification described in Equation 1. At baseline, participants meet the goal of sleeping at least seven hours on approximately 43.8% of weeknights.²³ During the intervention period, Immediate Incentives increase the rate of sleeping at least seven hours by an estimated 11.5 percentage points, a 26% increase ($p < 0.001$). The treatment effects persist into the post-intervention period but are about

²²We present the analogous figures for sleep hours in Figure A.5 and distributions of sleep hours in Figure A.6.

²³The baseline average reported in Table 3 is slightly different from that reported in Table 2 as we are pooling Immediate Incentives and Control. Furthermore, in Table 2 we calculate the baseline average at the individual level and in Table 3 we calculate it at the night level, and not all participants have the same number of nights in the baseline period due to rolling enrollment.

half the size: an estimated 5.2 percentage points, 12% higher than baseline ($p < 0.001$).²⁴ We estimate that total sleep hours increase by an estimated 19 minutes on average during the intervention period and by an estimated eight minutes during the post-intervention period. All estimates are significant ($p < 0.001$) and are robust to adjusting for multiple hypothesis testing (see Tables A.9). Our estimated effects on sleep duration are similar to those found for sleep medications, such as magnesium (Mah and Pitre, 2021), melatonin (Choi et al., 2022) and zolpidem (Xiang et al., 2021).²⁵

The effects at the mean reflect shifts throughout the distribution of sleep, as measured by sleep hours and share of nights sleeping at least seven hours (Figure A.6). In Table A.4, we estimate treatment effects by baseline quartile of sleep (share of nights sleep at least seven hours in panel A and sleep hours in panel B). We find similar effects across quartiles during the intervention period; and evidence of larger post-intervention effects among participants with lower levels of sleep at baseline. These results suggest that our intervention has the most persistent impact on those who are most sleep deprived. In exploratory analysis, we examine heterogeneity with respect to time preferences measured at baseline and find little evidence of differential effects on sleep (see the Supplementary Online Appendix (Giuntella et al., 2024)).

As shown in Table A.5 (panel A), we do not find any evidence of substitution between incentivized weeknight sleep and unincentivized sleep during the day, on weekends, or during holidays (spring break for the treatment period and Thanksgiving for the post-treatment period). If anything, we find small positive spillovers, with some evidence of an increase

²⁴We conduct the following sensitivity analyses in Table A.3. In panel A columns 2 - 6, respectively we: limit the covariates to wave fixed effects, gender, baseline sleep and baseline GPA; limit the sample to participants who have term GPA, exclude missing nights rather than replacing missing data with individual baseline means; exclude wave 3 (onset of COVID-19); and, reweight the sample with respect to gender to make it representative of the gender composition of U.S. college students. The results do not change. We estimate treatment effects of 11-12.7 percentage points and post-treatment effects of 5.2-6 percentage points. The estimated effects pooling incentives (column 7) are similar, but slightly lower: 10.5 percentage points during treatment and 4.9 percentage points in post-treatment. Lee (2009) bounds range between 9.6 and 15.5 percentage points for the treatment and between 3.4 and 6.1 percentage points for the post-treatment (panel B columns 2 - 3).

²⁵We do not find evidence that potential spillovers between the treatment and control groups are biasing our estimates. In waves 2 - 7, we asked subjects whether they knew someone else participating in the experiment. Out of 798 individuals, 232 reported knowing someone in the experiment (29% of our sample in waves 2-7); 70 (8.3%) reported knowing someone who either received reminders or rewards; and 66 (7.8%) reported knowing someone who received rewards. In Table A.3, we exclude each of the “spillover” groups from the analysis (panel B, columns 4 - 6) and also add controls for each of these categories (column 7). The point estimates decrease slightly when we exclude those who know anyone in the experiment and are almost identical when we exclude those who know someone in the treatment groups. Finally, we find no evidence that either treatment effects or baseline rates of sleeping at least seven hours increase across waves, further suggesting an absence of spillover effects over time (see the Supplementary Online Appendix (Giuntella et al., 2024)).

Table 3: Immediate Incentives and sleep

	(1)	(2)
	Sleep ≥ 7 hrs	Sleep hours
Treatment	0.115*** (0.013)	0.318*** (0.035)
Post-Treatment	0.052*** (0.015)	0.139*** (0.036)
Observations	39,035	39,035
Mean of Dep. Var.	0.438	6.653
Std. dev.	0.496	1.465

Notes: The sample is restricted to individuals in the Immediate Incentives treatment and individuals in the Control group. All estimates include day of the week, week of the experiment, wave, and month fixed effects, baseline value of the outcome variable, and demographic controls for gender, age (dummies), race and ethnicity (Asian, Black, Hispanic, White, other), indicators for the number of classes starting at 10 am or earlier, indicators for whether parents' highest academic title was less than college, college degree, more than a college degree, and quartile of baseline GPA (high school GPA if non-missing, prior term GPA if high school GPA is missing). For all demographic characteristics, we included a missing indicator for whether the variable is missing. Standard errors are clustered at the individual level. Mean of dep. var. is the mean of the dependent variable at baseline. Std. dev. is the standard deviation of the dependent variable at baseline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

in the likelihood of sleeping at least seven hours during weekends in the post-intervention period.²⁶

In panel B of Table A.5, we examine additional sleep outcomes. Similar to our main results, we find that our intervention significantly increases the share of nights participants sleep at least six hours and the share of nights they sleep seven to nine hours, with persistent but smaller impacts in the post-treatment period.²⁷

3.2 Educational outcomes

Next, we investigate the impact of our primary treatment on educational outcomes. Table 4 presents regression estimates of the impact of the intervention on semester GPA and

²⁶Including naps, holidays and weekends, we estimate the intervention increased the share of nights with at least seven hours of sleep by 8.1 percentage points during treatment and 3.9 percentage points during post-treatment; and, increased total sleep hours by an estimated 15 minutes in the treatment period and 5 minutes in the post-treatment period ($p < 0.01$ for all estimates). As noted above, Fitbits may not fully capture short naps.

²⁷Sleeping less than six hours is a common metric of sleep deprivation (Hafner et al., 2017). The recommendation of sleeping seven to nine hours draws on studies that link excessive sleep duration to detrimental effects on health (Hirshkowitz et al., 2015; Jike et al., 2018).

secondary educational outcomes.²⁸ The regressions follow the specification of Equation 2. In columns 1 - 2 we estimate treatment effects on our primary outcome, course grade, in the term the intervention took place. Column 1 includes all course types (lectures, seminars, labs, independent studies and other classes) whereas column 2 restricts the analysis to lectures (which account for approximately 80% of course types). Columns 3 - 4 report the same analysis for the term following the intervention. In columns 5 and 6, we examine the persistence of the effects two terms after the intervention.

Table 4: GPA, Immediate Incentives

	(1)	(2)	(3)	(4)	(5)	(6)
	Term of intervention		Term +1		Term +2	
	All classes	Lectures	All classes	Lectures	All classes	Lectures
Immediate Incentives	0.075** (0.037)	0.089** (0.042)	0.069* (0.038)	0.092** (0.042)	0.005 (0.042)	0.005 (0.046)
Observations	4,300	3,413	4,087	3,298	3,842	3,080
Mean of dep. var.	3.502	3.436	3.553	3.494	3.547	3.505
Std. dev.	0.763	0.805	0.756	0.795	0.774	0.806
Number of individuals	833	827	784	782	727	718

Notes: All estimates include demographic controls for gender, age (dummies), race and ethnicity (Asian, Black, Hispanic, White, other), baseline sleep, indicators for the number of classes starting at 10 am or earlier, indicators for whether parents' highest academic title was less than college, college degree, more than a college degree, and quartile of baseline GPA (high school GPA if non-missing, prior term GPA if high school GPA is missing). For all demographic characteristics, we included a missing indicator for whether the variable was missing. Observations are weighted by the number of credits taken in the semester. Standard errors are clustered at the individual level. Mean of dep. var is the mean of the dependent variable at baseline. Std. dev. is the standard deviation of the dependent variable at baseline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

As shown in columns 1 and 2, we estimate that Immediate Incentives improved average course performance by 0.075 grade points in all classes ($p = 0.044$) and 0.089 grade points in lecture classes ($p = 0.034$).²⁹ The estimated GPA impacts of 0.075 - 0.089 grade points during the intervention semester are equivalent to a 0.10 - 0.11 standard deviation (SD) increase in grades. These effect sizes are about 25-29% of the GPA gap (0.30 grade points) between students with above versus below median high school GPA. We estimate a treatment effect of similar magnitude on course performance in the semester following the intervention, providing suggestive evidence of persistent effects (column 3 ($p = 0.070$) and column 4

²⁸As discussed in Section 2.4, we are missing GPA for 1.9 percent of our participants. We examine differential attrition on the GPA measure in Table A.2 and find no evidence for differential attrition on term GPA.

²⁹As shown in panel C of Table A.6, we find no treatment effects in non-lecture classes (i.e., seminars, labs, internships, directed studies). As discussed in Section 2.4, over two-thirds of students in these classes receive an A, and the lowest quartile is A-, leaving little room to improve grades (see Figure A.3).

($p = 0.028$)).³⁰ However, we do not find treatment effects in the semester two terms after the intervention (columns 5 and 6).

We conduct the following sensitivity analyses in Table A.6: limit the covariates to wave fixed effects, gender, baseline sleep and baseline GPA; limit the sample to participants who have post-intervention term grades; limit the sample to participants who have baseline GPA (high school or baseline term GPA); limit the sample to participants who have sleep data at baseline; exclude wave 3 (onset of COVID-19); and reweight the sample with respect to gender to make it representative of the gender composition of U.S. college students. The estimated effects are slightly smaller when we limit the covariates, exclude participants missing grades data or reweight the sample: 0.061 - 0.075 grades points for all classes (p -values ranging between 0.024 and 0.085) and 0.075 - 0.089 for lectures (p -values ranging between 0.019 and 0.072). Interestingly, when we exclude the Spring 2020 semester (wave 3), when students were sent home mid-semester due to the onset of the COVID-19 pandemic, our estimated treatment effects increase slightly to an estimated 0.090 - 0.106 grade points. During this wave of the study, our participants experienced the abrupt closure of the university in the middle of the semester and disruptions in sleep and other lifestyle habits (Giuntella et al., 2021). When pooling all incentives treatments, the estimates are similar with slightly smaller average impacts, as shown in Table A.3 column 7 for sleep and Table A.6 column 8 for grades.

We also explore the effects of our intervention on other measures of course performance (Table A.8). This analysis includes the courses in our main analysis as well as courses that do not contribute to GPA because they do not have a grade on a four-point scale, such as pass, honors, and incomplete (see Table A.1 for the grading system). We find that students in the treatment group are less likely to receive any grade, which is primarily due to small increases in the likelihood of withdrawing from a class (column 2). At the same time, students in the treatment group are marginally less likely to fail a course (column 3). These results suggest that treated students are more likely to withdraw from classes they would otherwise fail. As a result, there are no significant differences in the likelihood of passing a class (column 4) nor in the number of credits completed in a term. We note that the effects on the likelihood of having any grade, withdrawing, or failing are not statistically significant after adjusting for multiple hypothesis testing (Table A.9).

Turning to heterogeneity, Table 5 shows that the results are not driven by performance in early-morning classes but rather the directionally largest effects are in late morning and early afternoon classes that occur between 10 am and 2 pm, followed by afternoon/evening

³⁰The effect one term after the intervention is not robust to multiple hypothesis testing (q -value=0.20, Table A.9).

Table 5: Immediate Incentives and GPA: Heterogeneity by schedule and class type

	(1) Course grade	(2) Class start: before 10am	(3) Class start: 10am-2pm	(4) Class start: after 2pm	(5) Class type: non-STEM	(6) Class type: STEM
Panel A: All classes						
Immediate Incentives	0.075** (0.037)	0.057 (0.064)	0.094** (0.044)	0.064 (0.055)	0.013 (0.034)	0.135** (0.057)
Observations	4,300	959	1,634	1,568	2,351	1,948
Mean of Dep. Var.	3.502	3.471	3.493	3.497	3.696	3.267
Std. dev.	0.763	0.810	0.744	0.773	0.574	0.888
Number of individuals	833	607	773	751	794	694
Panel B: Lectures						
Immediate Incentives	0.089** (0.042)	0.022 (0.071)	0.114** (0.048)	0.096 (0.065)	0.030 (0.040)	0.134** (0.059)
Observations	3,413	735	1,385	1,229	1,717	1,695
Mean of Dep. Var.	3.436	3.403	3.447	3.426	3.668	3.202
Std. dev.	0.805	0.859	0.774	0.815	0.598	0.912
Number of individuals	827	523	731	697	710	692

Notes: All estimates include demographic controls for gender, age (dummies), race and ethnicity (Asian, Black, Hispanic, White, other), baseline sleep, indicators for the number of classes starting before 10 am, indicators for whether parents' highest academic title was less than college, college degree, more than a college degree, and quartile of baseline GPA (high school GPA if non-missing, prior term GPA if high school GPA is missing). For all demographic characteristics, we included a missing indicator for whether the variable was missing. Observations are weighted by the number of credits taken in the semester. Standard errors are clustered at the individual level. Mean of dep. var. is the mean of the dependent variable at baseline. Std. dev. is the standard deviation of the dependent variable at baseline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

classes (after 2 pm). This is in line with the findings from [Carrell et al. \(2011\)](#) that early class start time affects performance in all classes, not just classes taking place early in the morning.

We additionally find evidence that our overall effects are driven by performance in STEM courses: on average our intervention leads to a 0.135 grade point increase in STEM courses, which corresponds to a 0.15 SD increase in STEM grades. By contrast, we estimate small increases of 0.013 grade points in non-STEM courses. This finding suggests that sleep may have a more significant impact on quantitative courses. However, given the large proportion of STEM majors in our sample (58%), another possibility is that improved sleep could enhance performance in courses important for a given major. The results may also partially reflect that there is more room to move grades in STEM courses, which have an average GPA of 3.27, compared to non-STEM courses with an average GPA of 3.70.

In exploratory analysis (Table A.7), we examine heterogeneous treatment effects on sleep and GPA by individual characteristics. We find larger effects among women, both on sleep and GPA. Effects on GPA are large among STEM majors, but effects on sleep are similar for

STEM and non-STEM majors. Further, first-term freshmen students exhibit substantially larger effects than other students. These findings are consistent with the idea that habits may be more malleable among freshman who have not fully developed their routines (Creswell et al., 2023).

4 Mechanisms for effects on educational outcomes

Our intervention targeted increased sleep hours as a primary mechanism for improving academic performance. In this section, we examine additional mechanisms through which our treatment effects could operate. First, we examine treatment impacts on sleep measures other than total hours, such as timing, regularity and quality, which could also affect educational outcomes. Second, we examine how increased time spent sleeping affects time spent on other activities in order to understand the opportunity costs of sleep and the extent to which shifts in non-sleep activities could drive academic performance impacts. Third, we examine treatment effects on creativity and quantitative tasks, physical health and mental well-being in order to understand the effect of sleep on these important outcomes, as well as their potential role as mechanisms for the impact of the intervention on academic performance. Finally, we explore the extent to which the immediate rewards themselves could be driving our results through direct effects of cash.

4.1 Sleep timing, regularity and quality

In the first two columns of Table 6 panel A, we estimate treatment effects on bedtime and wake-up time. During the intervention period, treated participants go to bed an estimated 19 minutes earlier than participants in the control group ($p < 0.001$) with directionally earlier wake up times ($p = 0.15$). This pattern does not persist in the post-treatment period when the incentives ended but the bedtime reminders continued. Instead, average bedtime largely reverts to baseline levels and treated participants wake up slightly later ($p = 0.08$). As shown in Figure A.5, both bedtime and wake-up time get progressively later over the course of the intervention period and stabilize during the post-intervention period.

We examine sleep regularity in two ways. First, in column 3, we examine effects on the Sleep Regularity Index (SRI), which captures day-by-day similarity in sleep and wake patterns (see Section 2.4 for details). In panel B, we examine the regularity of bedtime, wake-up time and sleep hours, which we measure by the within-individual standard deviation at the week level (e.g., how much a person’s nightly bedtime varies across the week where the level

Table 6: Sleep Timing and Regularity

	(1)	(2)	(3)
<i>Panel A: Daily data</i>	Bedtime	Wake up time	Sleep Regularity Index (SRI)
Treatment	-0.313*** (0.036)	-0.049 (0.034)	1.460*** (0.355)
Post-Treatment	-0.027 (0.037)	0.068* (0.038)	0.634 (0.392)
Observations	39,035	39,035	39,035
Mean of Dep. Var.	25.23	7.954	87.57
Std. dev.	1.560	1.557	9.760
Number of individuals	840	840	840
<i>Panel B: Weekly data - Regularity Within-individual weekly s.d.</i>	Bedtime	Wake up time	Sleep hours
Treatment	-0.049** (0.023)	-0.065** (0.032)	-0.102*** (0.033)
Post-Treatment	-0.055** (0.025)	-0.093*** (0.036)	-0.050 (0.035)
Observations	7,807	7,807	7,807
Mean of Dep. Var.	0.892	0.936	1.160
Std. dev.	0.481	0.592	0.664

Notes: The sample is restricted to individuals in the Immediate Incentive treatment and individuals in the Control group. All estimates include day of the week, week of the experiment, wave, and month fixed effects, baseline value of the outcome variable, and demographic controls for gender, age (dummies), race and ethnicity (Asian, Black, Hispanic, White, other), indicators for the number of classes starting at 10am or earlier, indicators for whether parents' highest academic title was less than college, college degree, more than a college degree, and quartile of baseline GPA (high school GPA if non-missing, prior term GPA if high school GPA is missing). For all demographic characteristics, we included a missing indicator for whether the variable is missing. Standard errors are clustered at the individual level. Mean of dep. var. is the mean of the dependent variable at baseline. Std. dev. is the standard deviation of the dependent variable at baseline. *** p<0.01, ** p<0.05, * p<0.1.

of observation is the individual-week).³¹ We find that the intervention significantly increases SRI by about 0.15 SD and within-individual variability in bedtime, wake-up time and sleep hours by about about 0.10 to 0.15 SD during the treatment period. The magnitude of the effects during the intervention and post-intervention periods are of similar size for bedtime and wake-up time (with smaller post-treatment effects on SRI and total sleep hours). These findings suggest that, while treated participants do not on average sustain earlier bedtimes after the intervention ends, they do establish more stable bedtime and wake-up time routines.

On our measures of sleep quality, we find small positive increases in minutes of REM sleep, no significant impact on minutes of deep sleep, and small, marginally significant impacts on

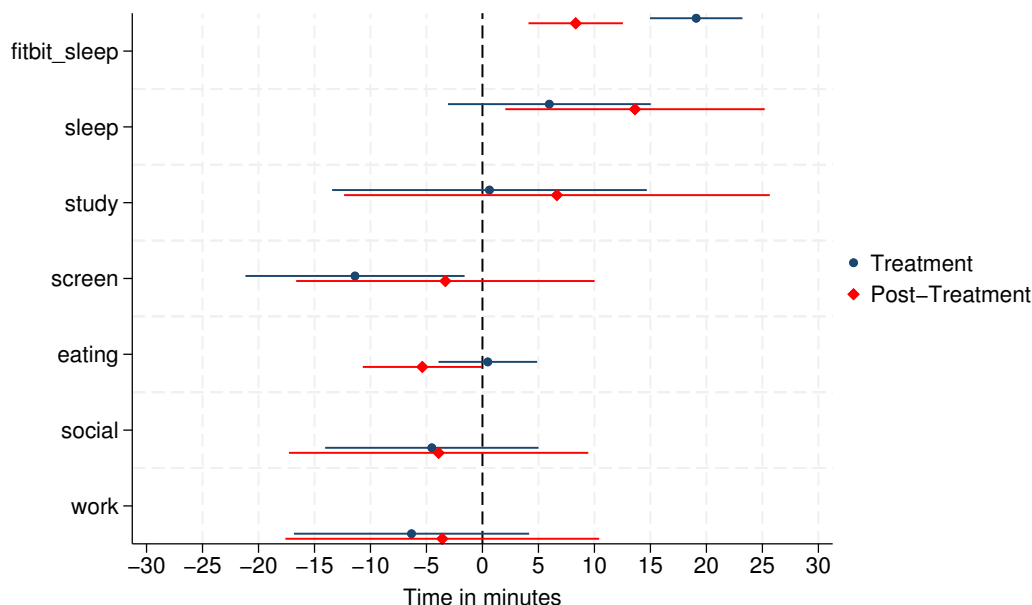
³¹The regressions follow the specification of Equation 1, except we exclude day of the week fixed effects given the analysis is at the weekly level.

sleep efficiency (Appendix Table A>5, panel B). We note that, in our sample, baseline efficiency is high: participants are asleep an estimated 93.5% of the time they are in bed. By comparison, [Bessone et al. \(2021\)](#) estimate an efficiency of 70% among their experimental participants in India. As discussed in Section 2.4 and in [Giuntella et al. \(2024\)](#), the accuracy and reliability of Fitbits in capturing sleep stages and assessing sleep quality remain limited.

4.2 Time use

We next focus on our survey measures of time use that we asked weekly throughout the study (see Section 2.4 for details). Figure 3 shows estimated treatment and post-treatment

Figure 3: Incentives to sleep and time use (minutes)

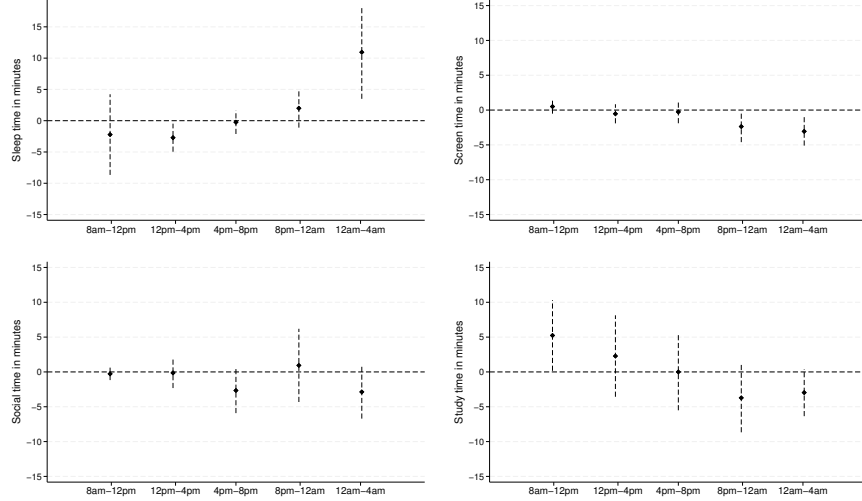


Notes: The figure reports differences between the Immediate Incentives treatment and Control group in time-use during the intervention (in navy) and in the post-intervention period (in red). All the coefficients are obtained from regressions including wave, month, and day of the week fixed effects, baseline value of the outcome variable, and demographic controls for gender, age (dummies), race and ethnicity (Asian, Black, Hispanic, White, other), indicators for the number of classes starting at 10 am or earlier, indicators for whether parents' highest academic title was less than college, college degree, more than a college degree, and quartile of baseline GPA (high school GPA if non-missing, prior term GPA if high school GPA is missing). For all demographic characteristics, we included a missing indicator for whether the variable was missing. Standard errors are clustered at the individual level. Bars indicate 95% confidence intervals.

effects on time spent (in minutes) for the top six time use categories, using the regression specification in Equation 1. In the first row of the figure, we show the estimated effects from the Fitbit data that we report in column 2 of Table 3. As discussed earlier, our intervention increased sleep by 19 minutes during treatment and eight minutes after the

removal of the incentives. Immediate Incentives directionally increase self-reported sleep in both the intervention and post-intervention periods by about 6 - 14 minutes on average per day (Table A.10). We also find that subjects were 7 (6) percentage points more likely to report at least seven hours of sleep during the intervention (post-intervention) period.

Figure 4: Immediate Incentives to sleep and time use over the day: Intervention period



Notes: The figure reports differences between participants in the Immediate Incentives treatment and Control group in the minutes allocated to different time-use activities during the intervention throughout the day. All the coefficients are obtained from regressions including wave and day of the week fixed effects, baseline value of the outcome variable, and demographic controls for gender, age (dummies), race and ethnicity (Asian, Black, Hispanic, White, other), indicators for the number of classes starting at 10 am or earlier, indicators for whether parents' highest academic title was less than college, college degree, more than a college degree, and quartile of baseline GPA (high school GPA if non-missing, prior term GPA if high school GPA is missing). For all demographic characteristics, we included a missing indicator for whether the variable was missing. Standard errors are clustered at the individual level. Bars indicate 95% confidence intervals.

During the intervention period, incentives to sleep significantly decrease average screen time, which includes internet browsing, TV/videos and games, excluding screen time for studying, by an estimated 11.5 minutes per day ($p < 0.01$). We estimate smaller and not statistically significant treatment effects on screen time during the post-intervention period. We do not find evidence of meaningful changes in time spent studying, socializing, eating or working during the intervention.³²

In Figure 4, we report treatment effects on sleep, screen time, social time and study time over the course of the day during the intervention period. The effects on sleep and

³²The effect on screen time is marginal once adjusting for multiple hypothesis testing ($q = 0.13$). We estimate treatment effects separately for internet, TV/videos and games in Table A.10 and the overall impact is largely driven by decreases in TV/video time. The table also reports effects on other time use categories. At baseline, we estimate the following average minutes per day for each category: sleep (493 minutes), study time (325 minutes), screen time (163 minutes), eating and preparing food (95 minutes), social time (99 minutes), and work time (97 minutes).

screen time are concentrated at night (8 pm - 4 am). Interestingly, while total study time does not increase, we observe a reallocation of study time from the evening/night (8 pm - 4 am) to the morning (8 am - 12 pm), although not precisely estimated. These results suggest that incentives to sleep led participants to develop sleep habits characterized by earlier screen disengagement at night and more focus on study time during the day. We find no significant changes in time spent working or participating in social activities. We also estimate treatment effects during the post-intervention period and find similar, but weaker, patterns (Figure A.7).

4.3 Task performance, physical health and well-being

To assess performance, we collected measures of quantitative reasoning and creative output on alternating weeks throughout the study (see Section 2.4 for details). We do not find any impact of the intervention on these measures (see Table A.11, columns 1 and 2), which could be due to the intervention not affecting math or creativity or to our coarse measures not being able to capture the impact of our intervention on performance. In addition, sleep could have influenced other measures of performance and cognition through channels like attention or memory consolidation, which were not captured by our measures (Diekelmann and Born, 2010).

Our final outcomes of interest are well-being and physical health. Previous work suggests that there is a positive relationship between sleep and both mental well-being and physical health (Giuntella and Mazzonna, 2019; Giuntella et al., 2017; Jin and Ziebarth, 2020). As discussed in Section 2, we use the Fitbit to measure participants' heart rate, daily steps and physical activity. We present estimates of treatment and post-treatment effects in Table A.11. We find no evidence of treatment effects on any of the physical health measures (columns 3, 4, and 5).

To investigate the impact of the intervention on well-being, we sent participants weekly text messages to collect data on mood, stress, and resilience to stress. Additionally, we utilize the Generalized Anxiety Disorder (GAD-7) scale to assess anxiety levels and the Center for Epidemiologic Studies Depression (CES-D) scale to gauge depression levels. These scales were administered at baseline and endline only, so we are only able to estimate treatment effects on post-intervention end-of-semester anxiety and depression. Table 7 shows that the intervention does not have a significant impact on mood or stress levels (columns 1 and 2). However, it led to a statistically significant increase in resilience—participants' self-reported ability to cope with stress—by approximately 0.15 standard deviations (column 3), which is significant at the 10% level after adjusting for multiple hypothesis testing. The intervention

Table 7: Immediate Incentives and well-being

	(1) Happiness	(2) Stress	(3) Resilience	(4) CES-D	(5) GAD-7
Treatment	-0.090 (0.108)	0.061 (0.059)	0.153*** (0.055)		
Post-Treatment	-0.084 (0.111)	-0.036 (0.068)	0.073 (0.063)	0.380 (0.846)	0.117 (0.384)
Observations	4,221	3,678	3,605	1,500	1,500
Mean of Dep. Var.	6.388	3.114	2.988	15.96	6.892
Std. dev.	1.649	1.116	0.995	10.28	4.883
Number of individuals	807	811	805	838	838

Notes: Estimates in columns 1 - 3 include day of the week, wave, and month fixed effects, and demographic controls for gender, age (dummies), race and ethnicity (Asian, Black, Hispanic, White, other), indicators for the number of classes starting at 10 am or earlier, indicators for whether parents' highest academic title was less than college, college degree, more than a college degree, and quartile of baseline GPA (high school GPA if non-missing, prior term GPA if high school GPA is missing). For all demographic characteristics, we included a missing indicator for whether the variable was missing. For mood, participants indicated, on a 10-point Likert scale, how happy they felt in that moment. For stress and resilience, participants indicated, using a 5-point Likert scale, 1) the extent to which participants faced stress in their life at the time of answering the survey and 2) the extent to which they felt able to deal with the stress they were facing. For columns 4 and 5, outcomes are measured at endline, and estimates include all of the controls listed above, except for day of week, week of the experiment, and month fixed effects. CES-D is the Center for Epidemiologic Studies Depression Scale. GAD-7 is the General Anxiety Disorder-7. Standard errors are clustered at the individual level. Mean of dep. var. is the mean of the dependent variable at baseline. Std. dev. is the standard deviation of the dependent variable at baseline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

did not show any significant effects on post-treatment measures of depression and anxiety (columns 4 and 5), with point estimates small in magnitude and not statistically significant.

4.4 Effects of immediate cash rewards

Finally, we examine whether receiving immediate cash rewards could have had direct effects on academic performance. Students in the Immediate Incentives group received an average of \$52 in immediate rewards over the four-week intervention period (\$4.75 on 55% of the 20 treatment days).³³ If rewards of this size alleviate financial constraints in the middle of the semester that hinder academic achievement, they are most likely to do so among students who are expected to face cash shortages. To examine whether this occurs, in Appendix Table A.12, we examine treatment effects on GPA by demographic subgroups that are more likely to experience an impact of direct cash: students who report receiving financial aid vs. not (columns 1-2), first generation college students vs. not (columns 3-4), and students who report working at baseline vs. not (columns 5-6). We find no evidence that disadvantaged students have larger treatment effects – point estimates are always smaller for the lower socioeconomic status subgroup compared to the more advantaged subgroup.

³³As discussed above, students in all conditions received \$6 at the start of the study a few weeks into the semester. Students in Control condition received an additional \$24 at the end of the study just before final exams.

Second, we asked participants about their consumption of caffeinated beverages, which could improve alertness and ability to focus (we asked about caffeine consumption in every other weekly time use survey). As shown in Appendix Table A.11 columns 6-7, we find no evidence that participants in the immediate incentives group increased their consumption of caffeinated beverages compared to those in control.

Third, in waves 2-7, we asked participants who received incentives what they spent the rewards on. In Appendix Table A.12, we estimate treatment effects excluding treatment group participants who report spending the incentives on food, drinks or school related expenses (column 7); and, treatment effects including only those treatment group participants who report spending on those categories (column 8). Note that we include the full control group in all analyses. We find no evidence that treatment effects are driven by participants who use the rewards to purchase food, beverages or school supplies. The estimated effects are directionally larger when these participants are excluded and directionally smaller when the analysis includes only these participants in the treatment group.

Finally, as discussed in more detail in Section 6, there is little evidence from prior work that the size of the rewards in our study would have a meaningful impact on academic performance. As shown in Figure 5, programs that provide grants of similar size find no impact on GPA (e.g., [Evans et al., 2020^A](#)).

Taken together, we do not find evidence for the direct effects of the cash rewards as a mechanism for our treatment effects. Instead, our results point to earlier bedtime and increases in sleep regularity, decreases in screen time, reallocation of study time from evening to morning hours and improved capability to deal with stress as potential mechanisms. Beyond total sleep hours, sleep timing and regularity may be important for cognition and performance. Prior work suggests that irregular sleep among college students is associated with delayed circadian rhythms and lower academic performance ([Phillips et al., 2017](#); [Windred et al., 2024](#); [Smarr, 2015](#)). Similarly, shifting study time to the morning, when students may be more alert and better able to concentrate, could enhance learning outcomes ([Pope, 2016](#)). Finally, reductions in screen time, particularly at night, could contribute to the impact of our intervention on academic performance. Prior experimental and quasi-experimental work does not find significant effects of social media on grades ([Braghieri et al., 2022](#); [Collis and Eggers, 2022](#)). However, [Braghieri et al. \(2022\)](#) do find that social media worsens college students' mental health and increases the likelihood they report adverse impacts of mental health on their academic performance. Our findings that Immediate Incentives to sleep both reduce screen time and enhance resilience provides suggestive evidence that sleep, social media use, and mental health may interact in important ways that affect educational outcomes, highlighting the need for future research to directly examine these mechanisms.

5 Mechanisms for habit formation in sleep

We designed the Immediate Incentives treatment with the aim of creating persistent bedtime habits by pairing bedtime cues with immediate rewards for meeting sleep goals. As discussed above, the intervention leads to earlier bedtimes, which was the intention of the bedtime reminder. However, these effects are not sustained after the incentives end, even though bedtime reminders remain in place. Instead, we find evidence of persistent treatment effects on sleep duration and sleep regularity for about four weeks after the intervention period ends.

To better understand the mechanisms for the effects of Immediate Incentives on sleep behaviors, we now turn to an examination of our secondary treatments. As discussed in Section 2 and summarized in Table 1, we designed the secondary treatments to vary elements of our primary Immediate Incentives treatment in order to investigate which of the features of Immediate Incentives drive habit formation and persistence. These include: (1) Delayed Incentives, which is identical to Immediate Incentives except that the rewards are distributed at the end of the study about a month after treatment; (2) Delayed Incentives No Cue/Feedback, which is identical to Delayed Incentives except that participants do not receive cues or feedback; and, (3) Cue/Feedback which only provides cues (bedtime reminders) and feedback with no rewards. Table 8 estimates the effects of our primary and secondary treatments on sleep hours and sleeping at least seven hours on weeknights. We restrict the analysis to waves 1 - 3 of the experiment when the secondary treatments were conducted.

We first examine the importance of immediate rewards compared to delayed or no rewards. As shown in columns 1 - 2, the effects of Immediate Incentives during treatment are about 48% to 73% percent higher than the effect of Delayed Incentives (with or without cues and feedback); and about three to four times higher than the effects of Cue/Feedback alone. The differences between the estimated impact of Immediate Incentives and each of the secondary treatments are all significant at the 10% level.³⁴ During the post-intervention period, the estimated effects of Immediate Incentives are generally larger in magnitude than the secondary treatments, particularly for sleep hours. However, the effects are not statistically indistinguishable.

We next examine the importance of bedtime cues and feedback. Comparing the two Delayed Incentives treatments, we find that the effects of Delayed Incentives are similar with or without cues and feedback. If anything, the variant with no cue/feedback has directionally larger effects in the post-intervention period. In exploratory analysis reported in

³⁴All differences remain significant at the 10% level after adjusting for multiple hypothesis testing, except for the difference between the treatment effect on sleeping at least seven hours of Immediate Incentives vs. Delayed Incentives (Table A.13).

Table 8: Secondary treatments

	(1) Sleep ≥ 7	(2) Sleep hours
Treatment		
Immediate incentives	0.126*** (0.020)	0.304*** (0.052)
Delayed incentives	0.085*** (0.021)	0.177*** (0.056)
Delayed incentives, no feedback	0.081*** (0.021)	0.177*** (0.057)
Cue/Feedback only	0.030 (0.019)	0.107** (0.051)
Post-Treatment		
Post: Immediate incentives	0.032 (0.024)	0.182*** (0.065)
Post: Delayed incentives	0.003 (0.025)	0.042 (0.070)
Post: Delayed incentives, no feedback	0.049* (0.027)	0.091 (0.074)
Post: Cue/Feedback only	0.008 (0.026)	0.067 (0.070)
Observations	27,130	27,130
Mean of Dep. Var.	0.429	6.677
Std. dev.	0.495	1.529
Number of individuals	589	589
Difference between treatments (p -value)		
Immediate vs delayed incentives (p -value)	0.0937	0.0491
Immediate vs delayed incentives no feedback (p -value)	0.0578	0.0492
Immediate vs only feedback (p -value)	1.76e-05	0.00101
Post: Immediate vs delayed incentives (p -value)	0.273	0.0568
Post: Immediate vs delayed incentives no feedback (p -value)	0.548	0.217
Post: Immediate vs feedback only (p -value)	0.367	0.115

Notes: The sample is restricted to waves 1 - 3. All estimates include day of the week, week of the experiment, wave, and month fixed effects, baseline value of the outcome variable, and demographic controls for gender, age (dummies), race and ethnicity (Asian, Black, Hispanic, White, other), indicators for the number of classes starting at 10 am or earlier, indicators for whether parents' highest academic title was less than college, college degree, more than a college degree, and quartile of baseline GPA (high school GPA if non-missing, prior term GPA if high school GPA is missing). For all demographic characteristics, we included a missing indicator for whether the variable is missing. Standard errors are clustered at the individual level. Mean of dep. var. is the mean of the dependent variable at baseline. Std. dev. is the standard deviation of the dependent variable at baseline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A.3, panel B column 1, we estimate the post-treatment effects of the Immediate Incentives intervention separately for the subgroup of participants who continued to receive reminders and feedback in the post-treatment period (Immediate Incentives Post Cue/Feedback) and for the subgroup of participants who stopped receiving them at the end of the intervention period (Immediate Incentives No Post Cue/Feedback).³⁵ Our estimates reveal no significant differences between these two subgroups, suggesting that receiving bedtime cues after incentives stopped did not help sustain the sleep patterns developed during the intervention period.

Collectively, the results in this section do not provide strong evidence that individuals built automatic habits in response to our external cue. Early bedtimes do not persist in the post-intervention period when cues (but not rewards) remained in place. And, treatment variants with cues do not outperform variants without them, in either the intervention or post-intervention periods. However, the intervention did lead to persistent improvements in sleep duration and sleep regularity, as evidenced by reduced variability in bedtime and wake-up time. This suggests that participants may have identified personal bedtimes and wake-up times that they could sustain independently, even without continued external reinforcement.

Although habit formation is often conceptualized as an automatic response to an external cue, our findings suggest that sustained behavior change may have instead resulted from participants developing self-sustaining sleep routines, possibly reinforced by self-identified cues. Alternatively, sustained behavior change may have occurred through other mechanisms, such as acquiring a greater preference for sleep, recognizing its benefits more clearly, or restructuring personal schedules in ways that reduced the costs required to maintain regular sleep (Volpp and Loewenstein, 2020). Future research could explore these alternative pathways to sustained behavior change and examine when external cues can effectively promote habit formation.

6 Benchmarking our results

We benchmark our results in two ways. First, we compare our effects to casual estimates of the relationship between sleep and academic performance from naturally occurring data. As discussed above, prior work examines the effect of shifts in sunset and school start times on sleep, grades and test scores. For example, Carrell et al. (2011) estimates that shifting the start time of college students' first class by an hour from 7:00 am to 8:05 am improves overall

³⁵We restrict the analysis to waves 5 and 7 in which we ran both variants. The interaction term "Immediate Incentives*No Cue in Post-Treatment" estimates the difference in sleep in the post-intervention period between Immediate Incentives No Post Cue/Feedback and Immediate Incentives with Post Cue/Feedback.

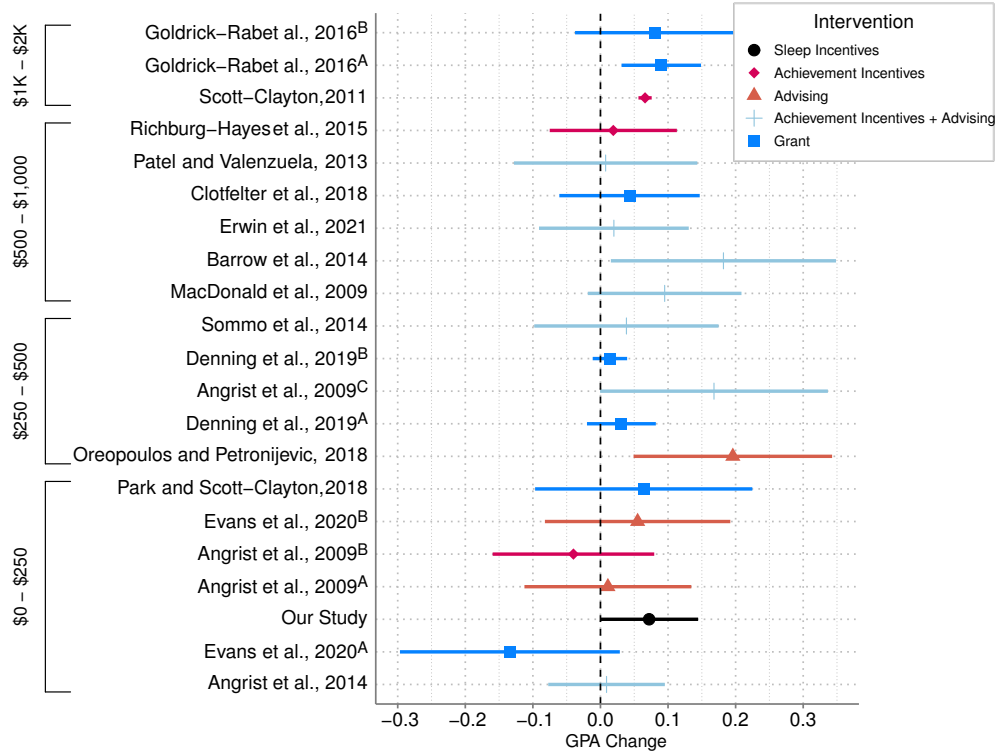
academic performance by 0.12 - 0.14 SD. The study does not directly measure students' sleep. Other studies using self-reported sleep estimate that a one-hour later school start time increases sleep by 36 minutes among American children, with a 0.16 SD improvement in reading and no change in math (Groen and Pabilonia, 2019). Related studies find that the the sun rising one hour later increases average sleep among American children by an estimated six minutes, with a 0.082 SD increase in math scores and a 0.057 SD improvement in reading scores (Heissel and Norris, 2018). Taken together, these studies suggest that a one hour shift increases sleep by 6 - 35 minutes and has either a null effect on academic performance or improves grades and test scores by 0.06 - 0.16 SD.³⁶ Our impacts of a 19 minute average increase in weeknight sleep during treatment, an eight minute average increase in post-treatment, and a 0.10 - 0.11 SD improvement in grades, falls within the range of the estimates in prior work on the causal relationship between shifts in sleep and changes in academic performance.

Second, we compare the cost effectiveness of our intervention to prior work examining policies aimed at improving college students' outcomes, including those that condition rewards on academic performance. Angrist et al. (2014) summarize the work on performance-based incentives, including their own, and conclude that, "the picture that emerges. . . is one of mostly modest effects. . . [And there are] similarly discouraging results from studies of state-based merit aid programs. A few studies report positive effects, most notably Scott-Clayton (2011)'s evaluation of West Virginia PROMISE," which conditions free tuition on meeting a minimum GPA. Scott-Clayton (2011) finds similar-sized GPA effects to our study, 0.066 grade point improvements, but at over ten times the cost, an estimated \$1,250 per student per semester. By comparison, we estimate that incentives to sleep increase semester GPA by 0.075 - 0.089 grade points and cost approximately \$112 per participant for the semester and would cost about \$160 per participant per year. This includes \$60 for the cost of the Fitbit and an estimated average of \$52 per participant per semester for the incentives (participants in the Immediate Incentives group received the incentives of \$4.75 per night on 55% of the 20 nights we offered it).

In Figure 5, we report the estimated effects on GPA from our study, alongside prior work examining the impact of achievement incentives, advising, and grants, ordered from most

³⁶Outside the U.S., Lusher et al. (2019) estimates that shifting class start times by an hour increases average sleep by about four minutes among Vietnamese University students with no effect on performance (Lusher et al., 2019). Jagnani (2021) estimates that the sun setting one hour earlier increases sleep by an average of 30 minutes among Indian children and that the sun setting 10 minutes earlier improves test scores by 0.10 SD and leads to 0.14 more years of schooling.

Figure 5: Cost-Effectiveness



Notes: The figure compares our Immediate Incentives effect on GPA to estimates from previous interventions aimed at improving college academic performance. Studies are grouped on the vertical axis based on their cost per subject per semester. Bars represent 95% confidence intervals. Superscripts above paper names denote different treatment arms or treatment groups. For Goldrick-Rab et al. (2016), superscript A is an estimate for the first cohort studied and B is their pooled estimate for the second and third cohort. For Denning et al. (2019), A and B are estimates for first-year and returning students, respectively. For Angrist et al. (2009), A is an estimate for an advising and peer-support treatment arm, B is for a financial incentives arm, and C for an arm combining A and B. For Evans et al. (2020), A estimates a grant treatment arm, and B estimates combined grant aid with academic advising. For Oreopoulos and Petronijevic (2018), course grades on a 0-100 scale have been divided by 25 for comparability to 4.0 scale GPA effects. Effect sizes for Richburg-Hayes et al. (2015), Patel and Valenzuela (2013), Erwin et al. (2021), MacDonald et al. (2009), and Sommo et al. (2014) are from Lintner (2024).

to least costly.³⁷ As depicted in the figure, our intervention is characterized by relatively low costs, while the estimated effects are equal to or greater than those observed in most previous studies. Only a handful of interventions surpass ours in terms of impact, but they come with a two to five-fold higher cost per participant. In particular, our intervention compares favorably to achievement incentives that condition rewards on GPA. As noted above, in a recent meta-analysis, Lintner (2024) estimates that performance-based financial

³⁷ Achievement incentives include performance-based incentives and merit aid. Advising includes advising and support services. We report authors' OLS estimates of effects on non-cumulative GPA, either at the semester or year-level. Costs are per program participant per semester. We note that for some of these programs the primary outcome may have been enrollment, persistence or graduation and GPA may have been a secondary outcome. We do not adjust program costs for inflation (note this overestimates the cost of our intervention relative to others). See Table A.14 for more information on each study.

incentives improve college students' GPA by an average of 0.041 grade points with average costs of \$457 per student per semester (or \$462 if weighted by study sample size). Our intervention finds effect sizes of almost twice the magnitude (0.075 - 0.089 grade points) at less than one-quarter the cost (about \$112).

7 Conclusion

In this paper, we show that an intervention targeting sleep habits improves academic performance. Our results suggest that targeting sleep may be a more cost-effective way to improve student performance than incentivizing performance directly. This could be because incentives based on sleep can be immediate whereas incentives based on grades are typically offered with a delay (e.g., at the end of the term). It could also be the case that students do not fully understand the production function for grades – and in particular, may not recognize the role of sleep.

Future research could explore how beliefs and information about sleep shape individual behavior and educational outcomes. Our students self-report wanting to sleep more than they do, but we do not know students' beliefs about the causal relationship between sleep and academic performance. Our study also highlights that the mechanisms for the impact of sleep on performance could have important interactions with behaviors that have largely been examined separately, including social media use and mental health. A better understanding of these interactions can help inform which behaviors to target to maximize downstream positive impacts on both performance and well-being ([Lindquist and Sadoff, 2023](#)).

Policies building on our results could focus on both the individual and structural level. Interventions that leverage a better understanding of beliefs could help bridge the gap between students' sleep intentions and their actual behavior. The timing of such interventions may also matter – for example, targeting first-term freshman and the middle of the semester may be particularly effective in shifting student behavior ([Creswell et al., 2023](#); [Liu et al., 2022](#)). Institutions could also test structural-level interventions, such as adjusting course start times or creating sleep-friendly campus environments. Identifying the most effective and scalable interventions is critical to providing actionable insights for educators and policymakers seeking to improve student outcomes through better sleep.

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