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David L. Dickinson, David Masclet

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Unethical Decision Making and Sleep Restriction: Experimental Evidence

David L Dickinson (Appalachian State University; IZA; ESI)

David Masclet (University of Rennes 1; CREM; CIRANO)

Abstract: Insufficient sleep is costly to organizations (e.g., direct health costs, cognitive errors, accident risk, and lower labour productivity). In this current study, we examine another more hidden cost associated with insufficient sleep-- unethical behaviors. Using a hybrid field/lab experimental approach, participants were randomly assigned to a week of sleep-restriction or well-rested sleep levels in their at-home (naturalistic) environment prior to decision making. We found that sleep restricted participants cheated significantly more in two honesty tasks, while anti-social choices were, surprisingly, not affected. Because sleep restriction promotes reduced deliberation, these results contribute to our understanding of the cognitive underpinnings of decision making. Importantly, these findings have practical implications to managers who want to reduce dishonesty in the workplace. For example, our results suggest that workplace health promotion programs focused on good sleep hygiene would additionally benefit the company in terms of indirectly promoting ethical conduct in the workplace.

JEL Codes: C91, D91, D63

Key words: Ethical choice, dishonesty, antisocial behavior, sleep.

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1. Introduction

The Centers for Disease Control and Prevention (CDC) has declared insufficient sleep a “public health problem” in the United States, and it takes a significant toll on organizations.¹ Insufficient sleep is estimated to affect roughly one third of adults in numerous countries (Hafner et al., 2017), and so this research is timely and has broad implications not only for society, but also for managerial practice and the health of a corporation’s culture. Many studies have shown that insufficient sleep leads to poor health outcomes, but it also results in large economic costs for organizations in terms of lower labour productivity, increased workplace accidents and cognitive errors (Hafner et al. 2017; Barnes and Wagner, 2009; Nuckols et al., 2009; Caruso et al., 2006; Scott & Judge, 2006; Pack et al., 1995).² Other studies have investigated more indirect costs of insufficient sleep by measuring how sleep deprivation might decrease ethical choice and moral awareness (e.g., Barnes et al., 2011, 2015a, 2015b; Christian and Ellis, 2011; Welsh et al., 2018). As a practical application of such findings, consider the implementation of a Workplace Health Promotion Program (WHPP). Though WHPPs typically have focused on smoking cessation and weight loss (Robbins and Jean-Louis, 2018), a sleep-hygiene focused WHPP would likely have benefits for a company that can go beyond direct health cost savings or reduced absenteeism. In short, support for a sleep-focused WHPP that can *indirectly* promote moral awareness (Barnes et al., 2015a) and increased ethical conduct (Barnes and Watson, 2019) is good management practice.

In this current study, we contribute to the literature on how insufficient sleep impacts ethical behaviors, which is an area receiving growing attention and of relevance for business (Barnes and Watson, 2019). Though the literature has shown that insufficient sleep likely increases unethical conduct, very little of this evidence is based on research using ecologically valid levels of objectively measured (insufficient) sleep or incentivized decision tasks. Here, we use experimental methodology to investigate how sleep restriction may affect unethical or anti-social choice in a set of simple but consequential (non-hypothetical) incentivized tasks.

Our working hypothesis is that individuals will exhibit more dishonest choices and will engage in more unethical activities when they are sleep restricted (see, e.g., Barnes et al., 2011; Christian and Ellis, 2011; Usmani et al., 2013; Barnes et al., 2015; Welsh et al., 2018; Barnes and Watson, 2019; Gunia, 2022). We present a simple theoretical framework below that

¹ Initially, the CDC used the term « public health epidemic » (see <https://www.aaas.org/news/sleep-deprivation-described-serious-public-health-problem> and <https://www.sleepdr.com/the-sleep-blog/cdc-declares-sleep-disorders-a-public-health-epidemic/>), although “public health problem” seems to now be preferred (e.g., <http://medbox.iiab.me/modules/en-cdc/www.cdc.gov/features/dssleep/index.html>).

² See also results from the National Sleep Foundation’s *Sleep in America* 2008 poll on sleep in the workplace.

generates this prediction, which concurs with empirical findings in the literature, and we tested this hypothesis using an experimental protocol that combined a full week of at-home sleep manipulation, objective sleep level measurements, and validated laboratory decision tasks. Our data set is comprised of over 200 participants who made consequential decisions in validated tasks at the end of a week of randomly assigned sleep levels (i.e., well-rested or restricted). As such, our study provides unique data on how sleep levels affect ethical decisions, which has important implications for the workplace.

Understanding the factors that influence unethical behaviors is crucial, and the topic has interested economists for decades. Unethical conduct or so-called “deviant” behavior constitutes a major concern for companies, and organizational research has shed light on its pervasiveness within firms.³ Unethical activities generate large costs to organizations and so are of clear interest to managerial practice. For example, yearly losses due to theft and fraud are estimated at close to \$50 billion (Coffin, 2003), and the annual cost of absenteeism in the United States is estimated to be approximately \$30 billion (Steers and Rhodes, 1984). In the aggregate, unethical behaviors may cost organizations as much as \$200 billion annually (Murphy, 1993). Moreover, employees’ exposure to other employees’ deviance can affect corporate culture and lead to low morale, damaged self-esteem, increased fear and uncertainty at work, eroded trust among workers, impaired collaborations, and increased turnover (Giacalone et al., 1997). The rise in remote work arrangements also implies increased opportunities for worker dishonesty, shirking, or impression management due to decreased monitoring effectiveness and increased anonymity of choice (Corgnet et al., 2015; Dickinson and McEvoy, 2021). How to best manage workplace ethics may not be simple, because the implementation of formal institutions to promote ethical conduct (e.g., monitoring policies, ethical conduct seminars) may have the unintended consequence of crowding out intrinsic honesty (Galeotti et al., 2021).

³ Unethical behaviors in firms can be defined as behavior that violate significantly organizational norms and legal rules, and can therefore threaten performance and well-being of the organization and/or its members (Robinson and Bennett, 1995). Examples of unethical activities are many: theft (Coffins, 2003), sabotage (Lazear, 1989; Chen, 2003; Harbring and Irlenbusch, 2008; Harbring et al., 2007; Abbink and Hermann, 2011), false performance reports or doping (see Schwierien and Weichselbaumer, 2010; Charness et al., 2014), forgery (List et al., 2001; Enders and Hoover, 2004), excessive absenteeism, leaving early or arriving late to work (Robinson & Bennett, 1995), inter-personal rudeness (Robinson and Bennett, 1995), resource destruction (Abbink and Sadrieh, 2009; Abbink and Hermann, 2011; Zizzo and Oswald, 2001; Zizzo, 2010). Schwierien and Weichselbaumer (2010) show experimentally that a competitive environment encourages people to cheat to improve their own performance, and others have found that competition may increase sabotage (Lazear, 1989; Chen, 2003; Harbring and Irlenbusch, 2008; Harbring et al., 2007; Abbink and Hermann, 2011)

Traditionally, it is assumed that unethical choice results from a comparison of the expected pecuniary costs and benefits associated with the ethical versus unethical behavior. According to the standard economic model of crime, an individual maximizes the expected material payoff when choosing between behaviors (e.g., Becker, 1968). However standard models do a poor job in explaining unethical activities, and a large body of experimental research has found that individuals do not fully exploit opportunities to cheat or lie (e.g., Gneezy, 2005; Sutter, 2008; Dreber and Johannesson, 2008; Kajackaite and Gneezy, 2017; Gneezy et al., 2013; Gneezy et al., 2018; Mazar et al., 2008; Cohn et al. 2019; Balasubramanian et al., 2017). As such, ethical behavior seems driven by factors beyond simply pecuniary cost-benefit analysis.

Previous studies suggest there is preference heterogeneity across individuals regarding morality. Some people incur high intrinsic costs of unethical behavior such that they may always behave ethically or honestly even when this means foregone material benefits (*homo moralis*).⁴ In contrast, others may always choose dishonesty unless extrinsic costs are present and outweigh the material benefits of the dishonest behavior (*homo economicus*). Between these two extremes cases, many individuals may face intrinsic costs of unethical behavior and behave as if *conditionally* ethical (i.e., only cheat or lie if the extrinsic benefits outweigh the intrinsic costs). Another strand of literature suggests that unethical behavior may often result from non-rational processes (Bazerman, 2014; Bazerman and Tenbrunsel, 2011; Pittarello et al., 2015; Elfenbein, 2007). An on-going debate worth more attention is whether morality is a stable feature of behavior or whether it may be influenced by factors such as the environment or mood. A growing body of research has attempted to identify various contextual factors that affect ethical behaviors such as social influence, organizational features or the available choices in moral dilemmas (e.g., Fox et al., 2001; Diekmann et al., 2015; Kroher and Wolbring, 2015; Rauhut, 2013; Fortin et al., 2007; Figuières et al., 2013; Kebede and Zizzo, 2015).⁵ Still others have examined how state-level emotions impact dishonesty (Gaudine and Thorne, 2001).

Overall, our contributions are two-fold. First, we extend an original theoretical framework for decision making with moral concerns that may help identify key pathways through which sleep restriction may affect choices. Models of decision making that incorporate ethical concerns into one's utility function may wish to consider social influences, as well as

⁴ This is in line with Augustine (421) and Kant (1787) who advocated such a categorical approach to morality.

⁵ Some other studies have examined the role of individual differences or organizational characteristics as antecedents of unethical behavior (Kish-Gephart, Harrison, & Trevino, 2010), such as gender, age, education, work experience, personality, ethical climate, and culture.

factors that may dilute the disutility of immoral choice, within their frameworks. Second, we test the relationship between sleep restriction and unethical or deviant behavior across three distinct decision tasks: the money burning task (Zizzo and Oswald, 2001), the coin flip task (Buccioli and Piovesan, 2011; Houser et al., 2012), and the matrix task (Mazar et al., 2008).⁶ The money burning game is commonly used to study anti-social preferences (Zizzo and Oswald, 2001; Zizzo, 2004; Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011; Prediger et al., 2014; Dickinson and Masclet, 2019; Zhang et al., 2020), while the coin flip and matrix tasks involve objective honesty or cheating behavior.

A main finding we report is that sleep restriction robustly increases dishonesty, but it does not increase antisocial choice (in the money burning game). Our findings are of practical importance given that our randomly assigned sleep levels were designed to approximate either the levels of insufficient sleep common in society or the levels recommended by sleep health experts. As such, our findings are more likely to transfer to real world decision making differences between two focal levels of nightly sleep.

The paper is organized as follows. Section 2 presents more background literature of relevance. Section 3 describes our experimental design and procedures. Section 4 discusses the theoretical predictions and behavioral hypotheses. Section 5 reports our findings. Finally, section 6 concludes and discusses how our findings fit into the literature on ethical choice and managerial practice, in addition to noting the limitations to our findings.

2. Background

2.1. Background on the effects of sleep deprivation

A large body of research has shed light on the importance of sleep for various outcomes, and the focus on how insufficient sleep affects decision making has been of significant interest in this literature (e.g., Pilcher and Huffcutt, 1996; Harrison and Horne, 2000; McKenna et al., 2007; Barnes and Hollenbeck, 2009; Dickinson and McElroy, 2017).⁷ Sleep deprived

⁶ While the cheating task we implemented was based on the Mazar et al. (2008) design, with which the authors and others have documented evidence of cheating, a main conclusion from their research was that moral reminders reduce the incidence of cheating. In fact, a comprehensive report of numerous direct replication attempts found that this result did *not* replicate—moral reminders did not reduce cheating in this task (Verschuere et al., 2018). Nevertheless, the task was useful given it allowed us to identify cheating at the individual level in the data.

⁷ Recent field experiment work has found no significant impact on decision making when sleep is increased by roughly 30 minutes in a noisy urban environment (Bessone et al., 2021). However, it must be noted that in their context, even after the increase in sleep their study participants would have been considered sleep deprived (i.e., their treatment manipulations increased sleep length from about 5.5 hrs/night to about 6 hrs/night). Their study is of particular interest in understanding the challenges to improving sleep in some important real-world contexts.

individuals tend to perform normally on standardized tests (Blagrove, et al., 1995; Harrison and Horne, 2000), and their decision making may not suffer if the choice environment is simple (McElroy and Dickinson, 2019).⁸ However, for more complex decision tasks requiring executive function, the lack of sleep likely harms decision quality (Harrison and Horne, 2000; McElroy and Dickinson, 2019; Dickinson and McElroy, 2019). In the neuroscientific literature, it has been shown that the effects of sleep deprivation on human behavior result from decreased brain functioning, particularly in the prefrontal cortex, a region that contains a critical set of neocortical structures related to executive function and self-control (Harrison and Horne, 2000; Jennings, et al., 2003; Durmer and Dinges, 2005).⁹

2.2. Background on how sleep affects ethical decisions

Research on how sleep affects moral choice or ethical decision making is growing (see reviews in Barnes and Watson, 2015; Gunia, 2022). Some previous studies have shown that sleep-deprived individuals often act impulsively, express irritability, hostility, anger and may engage in interpersonally inappropriate behaviors (e.g. Harrison and Horne, 2000; Reynolds and Schiffbauer, 2004; Zohar et al., 2005). Barnes et al. (2011) highlighted the positive relationship between sleep and self-control. Their study showed increased sleep reduced unethical choices in a laboratory choice task, although sleep levels were observational and not manipulated. Another paper used a proper sleep manipulation and showed that one night of total sleep deprivation increased dishonesty and antisocial behaviors, and this effect was mediated by hostility and self-control reductions stemming from sleep deprivation (Christian and Ellis, 2011).

⁸ Researcher have found mixed evidence regarding social influences on unethical behavior, with the weight of the evidence suggest a social influence effect. In the context of a tax evasion game, Fortin et al. (2007) found no evidence that information about average (laboratory) tax evasion behavior in the previous round affected current cheating. However, Diekmann et al. (2015) found that confronting subjects with others' cheating increased cheating in the subsequent round. Kroher and Wolbring (2015) reported that cheating increased in one's knowledge of others' unethical behavior. And, Rauhut (2013) showed that those over-estimating or underestimating others' unethical behaviors, when informed about other's actual unethical behavior, changed their behavior as if to be more consistent with the prevalence of dishonesty in other participants.

⁹ This region has been implicated in the ability to control emotions and inhibit behaviors (Damasio, 1994; Miller, 2000). Other authors have shown that prefrontal impairment leads to increased negative emotions and poor emotion regulation (Davidson et al., 2000). Furthermore, some studies have shown that individuals who have depleted self-control are less able to inhibit aggressive or destructive impulses (DeWall et al., 2007; Tangney et al., 2004). Other studies have shed light on the fact that when self-regulatory resources are depleted, self-control is reduced (DeWall et al., 2007; Gailliot et al., 2006). Self-control has also been linked to an increase in impulsive and risky decisions (Leith and Baumeister, 1996). Of relevance here, one study also found that self-control depleted individuals were more likely to take advantage of the opportunity to cheat (Mead et al., 2009).

A focus area of the existing research on sleep and ethical choice has been on workplace behaviors, where reduced sleep has been shown to increase cyberloafing (Wagner et al., 2012), increase workplace deviant behaviors (Christian and Ellis, 2011), and increase abusive supervisory behavior (Barnes et al., 2015b), among other workplace relevant findings (Barber and Budnick, 2016; Watkins et al., 2021). Still others have connected the lack of sleep to reduced moral awareness (Barnes et al., 2015a), or they have suggested that morning versus evening times may impact morality (Kouchaki and Smith, 2014).¹⁰ The research on morning-morality by Kouchaki and Smith (2014), however, is likely more focused on the idea that resources needed to control unethical urges are depleted over the course of the day. Some have suggested factors that either moderate (e.g., “contemplation”, see Welsh et al., 2018) or strengthen this sleep-morality effect (e.g., social influence, see Welsh et al., 2014).

As a whole, the state of this literature suggests that sleep deprivation or insufficient sleep likely leads to more unethical or deviant behaviors, although relatively little of this evidence uses direct and incentivized measures of dishonesty with experimentally manipulated sleep. The work of Christian and Ellis (2011) is an exception that reported increased “theft” using an incentivized task among sleep deprived subjects compared to well-rested controls. Our work is distinct from theirs regarding the sleep protocol: we focused on more commonly experienced partial but chronic sleep restriction over the course of a full week, and the sleep restriction was experienced in one’s naturalistic at-home setting. In contrast, Christian and Ellis (2011) considered one night of total sleep deprivation experienced outside of one’s typical setting (i.e., a sleep lab lounge). Another distinction is that we included two separate incentivized honesty tasks as well as an incentivized antisocial behavior task, while they examined antisocial behavior using a non-incentivized interpersonal deviance task. Table 1 compares and contrasts the research cited above in terms of sleep methodology, incentives, task(s) used, and basic result found.

Most of the aforementioned literature connecting sleep to ethical choice relies heavily on theories of self-control resource depletion (Baumeister et al., 2000). Within this literature, dishonesty is often considered the more automatic behavior (see survey in Bereby-Meyer and Shalvi, 2015), which would invoke a dual systems framework of decision making. Here, system 1 decision processes (the more automatic or quick-thinking) are assumed to favor dishonesty, while more deliberative system 2 processes favor honesty. Depletion of self-control resources,

¹⁰ Other research has shown impacts of insufficient sleep on interpersonal conflict resolution that, though not necessary a question of ethics or morality, would also be of interest in managerial practice (Dickinson et al., 2022).

as might be the case with sleep restriction, has been shown to increase impulsive cheating (Mead et al., 2009; Gino et al., 2011). However, not all have come to the same conclusion. Capraro (2017) found that time pressure, which presumably promotes more quick-thinking automatic choices, increased honesty in a deception game. Haidt (2001) argues that ethical choice is more intuitive, and this result supports the Social Heuristics Hypothesis (Rand et al., 2014), which argues that honesty is more intuitive because it serves one better evolutionarily in everyday life (see also Rand et al., 2012). Van't Veer et al. (2014) used a digit-memorization task to induce cognitive load and also found that participants were more honest under higher cognitive load. Thus, if dishonesty is more cognitively taxing than being honest, one might consider honesty to be the more automatic behavior.

Disparate findings in the literature complicate the question: is being ethical/honest (or prosocial) the more or less automatic choice? Self-control resource depletion typically heightens self-interest and increases dishonesty, but some unethical behaviors require more effortful thinking than others (Bereby-Meyers and Shalvi, 2015). Poor sleep would seem to deplete critical resources necessary to resist temptation, but less ethical choices may not result from a state of depleted resources (e.g., if the choice is a complicated web of deception). A dual systems framework may be useful to organize how to think about ethical decision making, but we must also consider that not all unethical choices look the same and they can differ in the amount of deliberation required of them. A consideration of social or moral norms is also a likely input to one's choice in the moral or social behaviors domain. The theoretical framework we outline in Section 4 is focused on moral target behavior, and how sleepiness may decrease one's sensitivity to deviations from that target.

A meta-analysis that reviewed the literature on intuitive honesty versus intuitive dishonesty (Köbis et al., 2019) is also of interest here. Interestingly, these authors concluded that when dishonesty harms abstract "others" or those at increased social distance (e.g., the researcher's budget), then dishonesty increases when using more automatic or intuitive processes. Conversely, when dishonesty would harm other subjects in the lab (i.e., those at reduced social distance), then being honest is more automatic (i.e., less deliberation required). Nevertheless, previous studies have reported that sleep deprivation also increased deception (lying) towards socially proximate other participants in the experiment when doing so would benefit oneself at the expense of another participant's payoff (Barnes et al., 2011; Welsh et al., 2014, 2018). Though not a primary focus of our paper, the importance of social distance between decision-maker and those impacted by dishonesty is still rather unclear and likely needs further investigation.

Our study provides a valuable contribution given that the literature suggests: a) insufficient sleep disproportionately harms executive function (and, therefore, deliberation); b) sleep loss has been shown to increase unethical choice in self-report behavioral measures with only a rare focus on consequential ethical choices; and c) deliberation has shown mixed results regarding its impact on honesty. Also, existing studies on this topic have used observational sleep levels, self-report sleep levels, or total sleep deprivation to examine choice in the ethical domain. Our study manipulates sleep in a way to mimic the difference between recommended sleep levels and those commonly experienced by many (i.e., chronic but insufficient sleep levels). Our findings are therefore more easily transferrable to real world decision making and thus increase the external validity of our study.

3. Methodology and experimental design

3.1 Sleep Protocol Methods

Our research method started with an at-home sleep protocol designed to be ecologically valid in its approach to sleep manipulation, while at the same time preserving experimental features of random assignment and objective measurement of sleep levels in participants. First, an online sleep survey was administered to random samples of university students at one of the author's institutions. This took place regularly (e.g., every semester) to establish a viable database from which to draw potential experiment participants. The sleep survey database included basic sleep measures, a validated morningness-eveningness questionnaire, and screeners for anxiety, depressive, and sleep disorders. Exclusion criteria for the main study were: age outside the 18-40 years of age range, extreme morning- or evening-type preference, significant risk of major depressive or anxiety disorder, and self-reported sleep disorder or insomnia. Those individuals in the database who passed the screening criteria were then randomly assigned, *ex ante*, to a restricted sleep (SR) or well-rested (WR) treatment week prior to being sent an email invitation to participate in the week-long study. This protocol is therefore a between-subjects version of the same basic protocol described in Dickinson et al., (2017).

The week-long protocol required the participant to visit the lab for each of two sessions exactly one week apart. Sessions were either on a Tuesday, Wednesday, or Thursday (to avoid weekend sleep effects) and between 10am and 6pm to avoid extreme times of day.¹¹ For

¹¹A previous version of the at-home sleep protocol (Dickinson et al., 2017) explicitly recruited more extreme diurnal preference types (i.e., morning and evening types, with intermediate types being excluded) in conjunction with early morning (7:30 am) or late evening (10:00 pm) session times in order to manipulate the time-of-day alignment with one's diurnal preference. In the present study, extreme morning- and evening-types were explicitly excluded from the study, and the session times during less extreme times of day. This design was therefore

example, a cohort of subjects would be scheduled for Tuesday 11am sessions, which meant the recruitment email described that they must come to the experiment lab for a 1.5 hour lab session on Tuesday from 11am-12:30 as well the following Tuesday from 11am-12:30. The email invitation explained to the potential subject the treatment week to which he/she had been randomly assigned, the time/day of the two experiment sessions, and the fact that she was expected to wear an actigraphy device (a.k.a., the sleep watch) that would objectively measure the subject's sleep levels throughout the entire week. Additionally, it was explained that she must keep a sleep diary during the week and email her bed/wake times to the experimenter each day (these complementary data were used as part of the standard actigraphy data scoring protocol). Subjects assigned to the SR treatment were asked to attempt sleep each night within the 5-6 hrs/night window, while WR treatment subjects were asked to attempt 8-9 hrs/night sleep. No restrictions were placed on bed or wake times but rather the subjects were only prescribed a total amount of attempted sleep in accordance with their treatment assignment. Subjects were cautioned against drinking alcohol if assigned to the SR treatment, but otherwise they were free to carry on their usual activities during the sleep treatment week.

The at-home nature of the protocol is what gives our design high ecological validity. Additionally, the protocol manages risk to the sleep subject by allowing all forms of compensatory strategies deemed useful by the subject (other than sleeping more). Below, we discuss participant compliance and issues surrounding attrition during the one-week protocol. In short, the protocol was intended to produce participants who had undergone a full week of SR or WR sleep levels prior to administration of decision tasks during Session 2 (the decision experiment lab session). One of the tasks reported below (the "coin flip" task) was administered as part of a voluntary additional online survey that participants could choose to complete after night 5 of the protocol for additional compensation—each was allowed to complete the survey at any point after night 5 but before Session 2, which implies the subject still experienced at least 5 nights of « treated » sleep prior decision making in the coin flip task.

The objective measurement obtained imply ways we may control for sleep restriction in the analysis. An alternative to a dichotomous indicator for SR assignment would be to use the continuous (and objective) measure of one's nightly sleep average (in minutes per night). Still another option is to combine the nightly sleep average measures with the subject assessment of

intended to focus on sleepiness caused by sleep restriction as opposed to circadian influences—the Dickinson et al. (2017) design was used in a study of social decisions in Dickinson and McElroy (2017) and the compound effect of being both circadian misaligned *and* sleep-restricted was statistically insignificant, and the sleep restriction manipulation was a more significant predictor of behavior compared to circadian misalignment.

one's personal sleep need, which was elicited at an earlier point in time during the online sleep survey. This measure, which we call *Personal SD*, is a hybrid of subjective and objective measures that may be useful in trying to assessing the level of sleep restriction in a more personalized way. Though these continuous measures allow for full exploitation of the continuously measured nightly sleep time, it is important to remember that only the binary assignment to $SR=0$ or 1 was exogenously assigned by the experimenters (i.e., variation in nightly sleep or *Personal SD*, within the sleep assignment groups may be due to factors beyond the experimenter control). As we will see, our results are largely robust across all potential sleep control measures, with perhaps some exceptions in the case of *Personal SD*.

3.2 Decision Task Methods

As noted above, our methods included the administration of 3 decision tasks: the money burning task (Zizzo and Oswald, 2001), the coin flip task (Buccioli and Piovesan, 2011; Houser et al., 2012), and the matrix task (Mazar et al., 2008). In our current study, the money burning task asks one to consider several possible allocations of payoffs between oneself and another participant, and it then gives one the ability to pay money to destroy even more resources of the other (and the other participant cannot retaliate). Others have reported that sleep restriction lowers prosocial choices (Anderson and Dickinson, 2010; Dickinson and McElroy, 2017), while money burning, in general, can be viewed as anti-social given that resources are destroyed. However, choices to burn may be considered acts to reduce inequality, which is not necessarily antisocial, or acts of pure nastiness depending on the allocation being considered (see Table 2). As such, the money burning task is not about objective honesty, but it can still identify a type of anti-social or deviant behavior. Table 2 shows the task, which was administered in the lab session at the end of the sleep protocol. The full instructions informed participants that allocations represented payoffs (in cents), that they must make an allocation choice for each of 9 different scenarios, and that random (anonymous) counterpart and role assignments, as well as the scenario randomly selected for real payoff, would only be determined after all decisions had been made (see Appendix B for full instructions).

Our version of the coin flip task measured honesty by asking participants to report the number of HEADS flipped out of 15 total coin flips when payoffs are known to increase in the number of HEADS reported.¹² This short task was administered online to participants after the

¹² This variant of the task in Houser et al. (2012) that asks for the outcome of multiple flips is another way to introduce additional richness to the outcome measure relative to a single coin flip report. The die-roll task (Fischbacher and Föllmi-Heusi, 2013) also allows varied degree of cheating.

5th night of the sleep protocol (but before the end of the study), and participants were not required to do this task as it was added to the study at a later point in time. In the online coin flip task participants were asked to locate a coin, flip it 15 times, and report the number of HEADS flipped. The temptation for dishonesty was induced by indicating that the participant would be paid \$0.25 per HEAD flipped (see Appendix B). This payment was given via Amazon gift-code and was in addition to a fixed payment received for the additional online survey offered to participants in this study—this online survey contained other unrelated tasks. Coin flips in the online task were obviously conducted in private and so certain outcomes (HEADS reports) may be statistically unlikely but not verifiably dishonest at the individual subject level.

The matrix task, like the money burning task, was conducted in the laboratory decision session after the sleep treatment week. For the matrix task, participants were presented a worksheet containing 15 distinct 3x3 matrices and told the goal with each matrix was to identify a pair of cells whose numbers summed to exactly 10.00. Participants were told they would be given exactly 4 minutes to solve as many matrix problems as possible. Participants were told they would earn \$1 for each matrix correctly solved, but they would pay themselves from a blank and unmarked envelope at their lab station that contained fifteen \$1 bills. The matrix task worksheet did not include the participant's code as was the case with the other tasks, there was no recording of names, no experimenter interaction or surveillance during the task or self-payment process, and special human subjects approval was obtained to not require any signed receipts for payments in this task. Completed matrix sheets were collected separately by the experimenter in non-systematic order where participants dropped their worksheet into a large box, and any remaining bills left in the unmarked envelope at the lab station would only be retrieved by the experimenter after all participants had left the lab. The experimenter could, however, match matrix task outcomes and self-payments to a specific participant code via the lab station location, which had been assigned by the experimenter prior to Session 1 to uniquely identify sleep watch assignments, and a secondary coding of the lab station number obfuscated within the footer on the backside of the decision sheet used by the participant (see Appendix B). As such, honesty at the individual participant level was identifiable in the matrix task.

4. Theoretical predictions

Here, we present a framework to help generate testable implications regarding sleep and moral choice. We do not claim this to be the only framework that may be useful in this regard, but what we present is based on the intuitive notion that insufficient sleep may desensitize one to

making a more immoral choice. A framework for decision making with moral concerns may help identify key pathways through which sleep restriction may affect choices.

Consider the framework in Masclet and Dickinson (2019). They define utility as: $U(a) = b(a) - c(a) - v(a - \hat{a})$, where a is an action that generates both benefits, b , and costs, c . Both benefits and costs are twice continuously differentiable: $b' > 0$, $c' > 0$, $b'' \leq 0$, $c'' \geq 0$.

The morality component of the utility function is captured by $v(a_i - \hat{a}_i)$, which subtracts from utility for actions that deviate from one's moral imperative, \hat{a} , in either direction— $v'_a > 0$ if $a > \hat{a}$, $v'_a < 0$ if $a < \hat{a}$, and $v'_a = 0$ if $a_i = \hat{a}_i$. Also, they assume $v''_{aa} > 0$ such that marginal disutility increases at an increasing rate as one's action gets further from the moral obligation. One practical extension of the model in our case may be as follows:

$$U(a) = b(a) - c(a) - \lambda(s).v(a - \hat{a}) \quad (1)$$

Where $\lambda(s) \in [0,1]$ is a function of the optimal sleep duration, $s \in [0,1]$, such that $\lambda(s)' > 0$. At one extreme, an optimally rested individual will fully weight deviations from her moral target ($\lambda = 1$). At the other extreme ($\lambda = 0$) an individual suffers significantly from sleep deprivation and is totally careless regarding concern over her action's deviation from the moral target. In short, the full disutility of deviation from one's moral target may only be felt by someone fully rested (or, without any cognitive resource depletion, in general). Given that the pre-frontal cortex plays a key role in executive functioning (Nilsson et al., 2005) or conscious decision making, the lack of sleep may produce a diminished ability to control impulsive and potentially deviant behaviors, such that deliberative thinking is what makes one fully sensitive to the cost of moral behavior that is inconsistent with one's moral target. Such a framework can also be generalized to explain how any temporal or environmental factor, not just sleep, may impact utility in a way that affects moral choice (e.g., Lu et al., 2018).¹³

In describing how this framework may produce testable implications regarding sleep and moral choice, consider the case where one's action, a , describes dishonesty that generates material benefits but the only costs are moral costs, $c(a) = 0$. Here, one maximizes $U(a) = b(a) - \lambda v(a - \hat{a})$ with respect to dishonesty. The first order conditions (FOC) are:

¹³ This utility function bears some resemblance to the peer pressure framework of Kandel and Lazear (1992). However, in our case one can incur disutility from deviation from one's moral target even in the absence of being observed by others. Thus, our framework is more about guilt than shame.

$$\text{FOC: } \frac{\partial U}{\partial a_i}: b'_{a_i}(a_i) - \lambda(s)v'_{a_i}(a_i - \hat{a}_i) = 0 \quad (2)$$

This FOC defines an optimal level of $a_i^* = a_i^*(\lambda(s))$ such that the FOC yields the identity:

$$b'_{a_i}(a_i^*(\lambda(s))) - \lambda(s)v'_{a_i}(a_i^*(\lambda(s)) - \hat{a}_i) \equiv 0 \quad (3)$$

The total differential of (3) can be written as (suppressing the participant subscript):

$$b''(a^*)da^* - \lambda(s)v''(a^*)da^* - d\lambda(s)v'(a^*) = 0 \quad (4)$$

This yields the implicit derivative:

$$\frac{\partial a^*}{\partial \lambda(s)} = \frac{v'(a^*)}{b''(a^*) - \lambda(s)v''(a^*)} < 0 \quad (5)$$

Since both $b(\cdot)$ and $v(\cdot)$ are convex functions, the denominator is unambiguously negative, and so the sign of $\frac{\partial a^*}{\partial \lambda(s)}$ is dictated by $v'(a^*)$. The result is that a reduction in λ (e.g., due to sleep restriction), is predicted to lead one to choose an action *further* from one's moral target—a lower a^* if one's action is below \hat{a} , and a higher a^* if one's action is above \hat{a} .¹⁴ Of course, in our specific task we assume the moral target is to not cheat ($\hat{a} = 0$), and so only cheating above the moral target of zero cheating is possible. Therefore, the prediction is that sleep restriction will lead to an increase in the optimal amount of cheating, a^* . This specification implies that more unethical choices may result from the depletion of cognitive resources via λ , all else equal.¹⁵ As such, we consider the framework outlined above as useful to organize our thinking and establishing the following hypotheses regarding how morality concerns may impact utility.

¹⁴ It is also the case that $v''_{aa} > 0$ by assumption, but one may debate this assumption that moral disutility increases at an increasing (rather than decreasing) rate. We consider this reasonable and similar to the typical assumption of increasing marginal costs, $c'' > 0$. The sign of (5) may still be negative with $v''_{aa} < 0$, but then the key result requires a relatively more negative b'' .

¹⁵ Alternatively, there is evidence in the sleep literature that may indicate sleep deprivation diminish one's sensitivity to the negative impact of an action (i.e., the costs) and/or magnify the anticipation of potential gains from a particular choice (e.g., Venkatraman et al., 2007, 2011). Thus, our hypotheses may also derive from the prediction that sleep deprivation will enhance the expected benefit, $b(a)$, of an action and/or reduce the expected cost, $c(a)$, of that action. All three of these potential mechanisms—reduced costs, enhanced benefits, or decreased moral disutility weight—cannot be disentangled in this paper, but they would lead us to similar hypotheses regarding increased unethical behaviors when sleep restricted.

Hypothesis 1: Sleep Restriction (SR) will increase anti-social (i.e., « nasty ») resource destruction in the Money Burning task.

Hypothesis 2: SR will increase the number of HEADS reported in the Coin Flip task.

Hypothesis 3: SR will increase dishonest payments in the Matrix task.

5. Results

5.1 Data set and protocol validity

A total of $n=237$ participants completed the protocol (though task data were incomplete on an additional 4 participants, depending on the task, and sleep watch data were incomplete or corrupted for another 2 participants). As such, the final data set is a total of $n=231$ for the Money Burning task, and $n=233$ for the Matrix task. The sample size is somewhat smaller ($n=197$) for Coin Flip task given that this was administered online after night 5 of the protocol but was a supplementary task that was not required of the participants. Thus, the 197 participants who completed the Coin Flip task represents a completion rate of 83%-84% for this task. Full sample and medians tests on the amount *over-paid* in the Matrix task (relative to one's reported outcome—our measures of cheating) showed no significant difference between the subsamples of data from those who had completed versus not completed the voluntary coin flip task ($p > .10$ in both Mann-Whitney and Kolmogorov-Smirnoff 2-sample tests).

Assessment of the sleep protocol is an important first step in evaluating the validity of the sleep manipulation methodology. Figure 2 and Table 2 summarize the protocol validity. Figure 2 shows the kernel density estimates of nightly sleep level distributions by treatment assignment. As can be seen, actigraphy measured sleep levels were approximately 117 min less per night for those in the SR group compared to the WR group. As discussed below, one may choose to examine all data as an intent-to-treat approach in the analysis, or an alternative is to score some subjects as being noncompliant with the assigned sleep prescription. Here, when coding the variable *Compliant* we consider a subject noncompliant if SR-assigned and sleep level > 375 min/night or WR-assigned with sleep levels of < 405 min/night.¹⁶ In general, we will report results from both the full (intent-to-treat) sample and the restricted sample of “compliant” subjects. The full sample captures the benefits of random assignment without the concerns present in considering the reasons behind noncompliance.

¹⁶ While this is a somewhat arbitrary cutoff, we consider it reasonable in the sense that it eliminates those with nightly sleep levels near average self-regulated levels (of around 6.5 hrs/night) in young adults. And, as can be seen in Figure 2, it removes those within the density function overlap that may be statistically harder to classify as belonging to one distribution or the other.

The protocol was successful at generating significant differences in nightly sleep levels and, as shown in Table 2, tests on self-report measures of sleepiness, irritability, and alertness present no significant differences on Day 1 of the 7-day protocol (prior to treatment) but significant differences in the expected directions on Day 7 (i.e., SR increases sleepiness, irritability, and reduces alertness). Additionally, because some participants were recruited for the one-week study but failed to complete the protocol, we considered the issue of sample selection in our estimations. Specifically, we estimated a probability of protocol completion equation from the entire set of participants who signed up for the study, whether or not the participant completed the protocol and appear in our decision task sample. This estimation model is shown in Appendix Table A1. From these completion probabilities we constructed the inverse probability weight (*IPW*) to estimate a weighted regression for all model specifications that corrects for selection into the final sample. As we will see, both our null and non-null results are similar whether or not we correct for sample selection.

5.2 Money Burning task results

We first evaluate results from the Money Burning task in using panel methods of the choices over the 9 allocation scenarios shown in Table 3, which were administered via the strategy method. In other words, subjects made decisions in all 9 scenarios *prior* to being randomly matched with another participant in the cohort, prior to random assignment of the roles of Player A and B, and prior to random selection of one scenario to play out for payoff (payoff values represented cents, such that a payoff of 400 was \$4.00, for example). Figure 3 offers a summary of the mean tendencies to burn money in the different scenarios shown in Table 3. Contrary to our hypotheses, we do not observe tendencies of SR participants to be more likely to burn resources in Scenarios 1-4—here, money burning would constitute a more anti-social “nastiness” (see Zizzo and Oswald, 2001; Abbink and Sadrieh, 2009; Abbink and Hermann, 2011). Results from two sample proportions tests on each binary comparison show only a significant difference in Scenario 6, where a marginally larger proportion of SR participants burned money. This Scenario 6 presented the smallest disadvantageous inequality gap to Player A, but the tendency is not general across all Scenarios where the decision maker starts with disadvantageous inequality. We next analyze these data using panel methods on the data set that has 9 observations per participant in this task.

We considered demographic and allocation descriptors in all panel estimations in Table 4, which are similar to the probit specifications used Dickinson and Masclet (2019) to model the probability of burning money. Important descriptors of the allocation scenario involve the

payoff difference between the two individuals, whether the start distribution income is equal between the participants, the cost of burning money relative to the Player A payoff. Demographic characteristics included age, gender, minority status, the treatment assignment ($SR = 0$ or 1), and a chronic daytime sleepiness score relating to the previous two weeks called the *Epworth* sleepiness scale ($=0-24$) that may represent an adverse sleep indication unrelated to the treatment assignment. Panel estimations were conducted on both the full intent-to-treat sample of 231 subjects, as well as the subsample of 203 subjects deemed compliant with the sleep treatment prescription. We conducted separate estimations of scenarios 1-5 and 5-9 data (with a dummy variable for the payoff equal start distributions scenario 5) to more clearly identify money burning that likely derives from a disadvantageous inequality aversion (Fehr and Schmidt, 1999) versus a preference for nastiness. Results are based on robust standard errors clustered at the individual subject level.

From our estimation results we report marginal effects on the probability of burning money in Table 4.¹⁷ As can be seen in Table 4, the sleep treatment assignment is not a significant predictor of money burning choices in our data. In fact, the only robust predictor of money burning is the equal start distribution allocation scenario 5. In this scenario, compared to scenarios 1-4 where one is at a payoff disadvantage, subjects are about 19 percentage points less likely to burn resources. This is consistent with an overall preference for payoff equality, as opposed to antisocial preferences, and this does not differ based on SR assignment.¹⁸ This null result is robust with respect to a sample selection correction as well (see Appendix Table A2). Thus, our data fail to support *Hypothesis 1*.

5.3 Coin Flip task results

We next examine results from the Coin Flip experiment. Mann-Whitney tests of the median number of HEADS reported in the SR versus WR groups shows more HEADS reported in the SR group, which is consistent with our Hypothesis 2. However, the result of the appropriate one-tailed test is only marginally significant for the full sample ($p = .061$) for the

¹⁷ Models without demographic controls yield the same qualitative results in terms of sign and significance of the sleep and allocation descriptor variables in all models. Also, results remain unchanged if using a continuous sleep quantity variable or the *Personal SD* measure in place of the dichotomous treatment assignment variable, *SR*. Our preference is to use the dichotomous *SR* indicator given it defines the random treatment assignment, whereas level of sleep (or, *degree of compliance*) may vary due to factors unrelated to the treatment assignment itself.

¹⁸ If one includes an interaction term *Equal Income*SR*, it is a statistically insignificant predictor ($p > .10$) of money burning, and its inclusion does not impact the sign or significance of the other coefficient estimates in the probit estimations. In general, these results are consistent with simple two-sample proportions tests that show no significant impact of *SR* on the proportion of money burning choices either in all scenarios, or in the set of S1-S4 scenarios where money burning would be considered anti-social ($p > .10$ in both instances using two-sample proportions tests).

full sample. The precision of the test increases when examining the restricted sample of compliant ($n=174$) participants ($p = .039$). Table 4 shows results from the multivariate analysis of Coin Flip determinants, which includes specifications both with and without demographic controls and with a correction for sample selection using the inverse probability weights (*IPW*) from the selection equation the predicts completing this task condition on enrollment in the 1-week study (Appendix Table A1). For this set of estimations, we estimated separate regression models on the intent-to-treat (all subjects) and compliant-only data sets. The negative and statistically significant coefficient estimates on the *SR* indicator variable across specifications in Table 5 support Hypothesis 2 that sleep restriction increases the number of HEADS reported.

Given results in support of Hypothesis 2, we conducted additional sensitivity analysis to confirm the result is robust. Results of the sensitivity analysis are summarized using coefficient plots in Figure 4 with the full estimation results shown in Appendix Tables A3 and A4. In Figure 4, the coefficient plots show the key effect of sleep on coin flip outcomes using alternative measures to capture the sleep restriction. In addition to using the dichotomous indicator for sleep restriction, *SR*, we also estimated models that included the continuous actigraphy-measured *Average Nightly Sleep Time* and a hybrid variable where average nightly sleep time is subtracted from one's self-reported sleep need to create a variable that describes how personally sleep deprived the individual may be, *Personal SD*. While these two alternative approaches to controlling for sleep impacts in the data more fully exploit the continuous nature of the actigraphy data, it is worth noting again that variations in the degree of sleep restriction or well-restedness may be due to factors not under experimenter control. Only the *SR* indicator reflects the random assignment of sleep condition for the participant. Thus, our preferred specification uses the binary *SR* indicator. Furthermore, *Personal SD* may perhaps be open to some additional criticism due to the combination of objective sleep level and subjective sleep need used in its construction.

In contrast to our money burning estimates, which failed to support Hypothesis 1, results in Table 5 and Figure 4 (see also Appendix Tables A3 and A4) indicate robust evidence that assignment to the *SR* treatment predicts significantly higher number of HEADS reported in the only coin flip experiment. It should be noted that both *WR* and *SR* individuals reported, on average, significantly more HEADS than what is statistically expected ($p < .01$ in both instances, based on simple one-sample z-tests). However, the fact that *SR*-assignment predicts almost an additional HEAD reported (over a total of 15 flips reported) is suggestive of an even higher level of dishonesty in the *SR* group (or those with lower levels of total sleep time). The *Female* indicator is also a highlight significant predictor of *fewer* HEADS reported, although if

one includes an interaction term *Female*SR*, the interaction is not statistically significant (this additional estimation result is available on request). Regarding independent variables used to control for sleep, Figure 4 shows that this support for Hypothesis 2 is robust to the use of the objective and continuous measure of *Avg Nightly Sleep*, but the finding is not precisely estimated when using *Personal SD* to control for participant sleep. Overall, these results support *Hypothesis 2*, although they fall short of providing definitive evidence of increased dishonesty when sleep restricted because of the nature of the coin flip task—the experimenter cannot conclusively claim cheating due to a HEADS report above the statistical expectation.

Additional evidence, however, lends support to the notion of using the coin flip task to identify dishonesty. For example, we collected response time (RT) data on participants in the coin flip task and find that those who report more than 8 (out of 15) HEADS have significantly faster RTs than those reporting 7 or fewer HEADS (median RTs of 108.61 and 151.92 seconds, respectively: $p < .001$: Mann-Whitney test on RTs). Because there should be no systematic relationship between the RT and the outcome of the task, this more likely suggests that those reporting more than an average number of HEADS are less likely to be actually flipping a coin. In and of itself, this is a finding with interesting implications for online or remote settings. Suppose, for example, that one wished to simply save time on a task and he/she reports a statistically average outcome without performing the requested task. In a workplace setting, that might be analogous to someone cutting corners on a task in a way that might put the company at risk (e.g., skipping steps in performing regular audits because, on average, no breaches are found).¹⁹

5.4 Matrix Task results

Finally, we turn to the results from the Matrix Task, which allows identification of dishonest overpayment at the individual participant level. Table 6 shows results of several specifications estimating the predictors of the number of matrices for which one paid oneself, *Matrix Pay*. Figure 5 summarizes the results from these are even more specifications using alternative controls for sleep (see Appendix Tables A5 and A6 for their full estimation results). It is worth highlighting that the matrix task involves a cognitive component that may be impacted by SR and that may also lead to some *accidental* overpayments. This is supported by evidence of a marginally lower number of matrices correctly solved (not Matrices *reportedly*

¹⁹ This was also noted in Dickinson and McEvoy (2021), where they found an increase in the likelihood that individuals were not actually flipping a coin in an online version of the coin flip task, when compared to the same task played in-person in the lab where the act of coin flipping (or not) was observable.

solved, which does not differ by SR assignment) by those assigned to the SR treatment ($p < .10$ on the SR indicator coefficient in regressions with controls. Results available on request). We consider it an accidental overpayment if the participant indicated a correctly solved matrix but was mistaken, and yet made a self-payment based on that mistaken number of correct matrices reported.²⁰ We account for this, we include a control variable in the estimations for *Matrix Report*, which captures the total number of matrices the subject reportedly solved. Self-payments not tied to differences in *Matrix Report* are an indication of dishonesty that we can link directly to the individual subject. Coefficient estimates on *Matrix Report* are all significantly different from zero and not statistically different from 1 (Wald tests: $p > .10$ in each instance), which is consistent with an extra \$1 of self-payment for each additional matrix reportedly solved (whether that is correct or not).

As can be seen from Table 6 and Figure 5, we find robust evidence that, even after controlling for the number of matrices reportedly solved, SR increases *Matrix Pay* by about an extra \$1. Given average value of *Matrix Report* of about 3.91, this represents an increase of approximately 25% in average dishonest payments, identifiable at the individual subject level. The left panel coefficient estimates derived from Appendix Table A5 using the binary assignment SR control show robust evidence level that sleep restriction increases the amount of payoff one takes in the Matrix task. The right and bottom panels of Figure 5 show sensitivity analysis of the same set of models that use the continuous *Avg Nightly Sleep* or *Personal SD* measure as the sleep control in place of the SR indicator, respectively. While estimation precision is reduced slightly when using these continuous sleep measures, in general, for the models estimated on compliant participants the statistical significance remains ($p < .05$). For the models estimated on the full sample, statistical significance remains at least marginal for the specifications using *Avg Nightly Sleep* or *Personal SD* ($p < .10$ or better).

It is likely the case that SR assignment does not impact all subjects the same, and the Matrix Task can be used to measure the severity of one's dishonesty. To investigate this further, we coded separate variables to arbitrarily separate *MODERATE CHEATERS* who self-paid themselves *at least* \$5 more than what they themselves reported, versus the *MILD CHEATERS* who self-paid between \$1 and \$4 more than what they should have. Results of these estimations

²⁰ This is technically a more accurate measure of dishonesty than used in Mazar et al. (2008), because in their study the experimenters paid participants according to their self-reported outcomes. If a participant self-reports having solved 13 matrices, for example, but due to carelessness or a math error only actually solved 11 or 12, then the self-report is less dishonesty than carelessness. In our approach, we consider dishonesty to be an instance of an individual actually self-paying him/herself more than what was reported, whether or not the report itself is accurate or reflects a math error.

are reported in Table 7. Here, we find that SR assignment does not impact the probability of being a Matrix task cheater, in general. However, if one conducts separate estimations of the SR impact on mild versus moderate cheating, we find that SR significantly predicts moderate but not mild cheating. The far-right column of Table 7 also examines the impact of SR on being a *SUPER CHEATER*, which we define as one who self-paid at least \$10 more than what he/she reported as solved. It is clear from these estimations that our data show support for *Hypothesis 3*. These results from Table 7 estimations are robust to controlling for demographic and sample selection using the *IPW* correct, but they lose some significance when using the *Avg Nightly Sleep* or *Personal SD* measure in place of the *SR* indicator (additional results available on request). Taken as a whole, our data show robust evidence of increased dishonesty in the Matrix task among SR-assigned subjects, and the effect is significant in magnitude and, overall, robust across model specifications.

One final item of note here is with respect to how Matrix task outcomes may inform our view of the coinflip outcomes, which suffer from the weakness of not being able to conclusively identify individual-level dishonesty. We can estimate model specifications where the probability of cheating (i.e., *Matrix Pay* > *Matrix Report*) is a function of whether the individual reported more than the expected average number of HEADS in the coin flip task (here, we defined *ABOVE AVERAGE HEADS* as *HEADS* > 8). Because the matrix task occurred *after* one has completed the online coin flip, there is no concern for reverse causation in such a specification. In all instances, we see that those participants who flipped more than the average number of heads in the online coin flip task predicted an increased probability of cheating in the in-lab Matrix task, which took place the next day ($p \leq .05$ in most instances). This supports a view that anonymous coin flip outcomes may help identify those more likely to display unethical conduct (see Appendix A Figure A1 for a coefficient plot summary of these results).²¹

6. Discussion and Conclusion

Insufficient sleep is an important concern in many modern societies. Several studies have shown across the globe that one-third or more of adults likely do not get the recommended minimum seven hours of nightly sleep (Ford et al., 2015; Hafner et al., 2017; Hirschkowitz et al., 2015;

²¹ Additionally, we capture response time (RT) in the coin flip task and can estimate a simple regression to examine the relationship between coinflip RT and coinflip outcomes. Here, we find that coinflip RT negatively predicts the likelihood of reporting more than HEADS = 8 ($p = .012$). That is, lower RT predicted a statistically significant increase in the likelihood one reports HEADS > 8, which should obviously not occur unless RT reflects behavior in some other way (i.e., actually flipping a coin or not, or taking more time to think about one's choice even if not actually flipping a coin). This implies that one may add precision to viewing coin flip outcomes as indicators of dishonesty if considering both RT as well as coin flip outcomes (see Dickinson and McEvoy, 2021).

Jones, 2013; Watson et al., 2015). According to the National Sleep Foundation, the average American adult does not sleep seven hours per night and the number of Americans who sleep fewer than six hours per night increased from 13 to 20 percent between 2001 and 2009 (NSF, 2005 & 2009). Sleep research is also relevant to organizational scholars and economists, because organizations often contribute to a growing incidence of insufficient sleep. For instance, according to the National Institute for Occupational Safety and Health [NIOSH] (2004) the number of hours worked annually in the United States has increased steadily over the past several decades. Management practice can therefore be part of the problem or part of the solution when it comes to encouraging a well-rested workforce. Though short-run needs may occasionally require extra work and compromised sleep schedules in any company, the research suggests that a culture of minimizing workers' sleep health is unwise. As the saying goes, "the bill always comes due". And, the bill associated with "sleep sacrifice" appears to come with an assortment of more or less hidden charges.

Sleep deprivation is known to have negative consequences in several domains (Bonnet and Arand, 1995; Ferrara and De Gennaro, 2001), which include effects on alertness (Thomas et al., 2000), impaired decision-making capacity (Harrison and Horne, 2000), reduced occupational safety (Barnes and Wagner, 2009), increased abusive supervision (Barnes et al., 2015), increased workplace accidents (Barnes and Wagner, 2009; Caruso et al., 2006; Scott & Judge, 2006), and worker well-being (NIOSH, 2004). According to National Center on Sleep Disorders Research (2003) sleep deprivation costs approximately \$150 billion annually in terms of accidents and lost productivity for the U.S. economy. These negative consequences of insufficient sleep clearly impact workplace outcomes in a way that should be a concern to managers. Beyond the more direct negative consequences of poor sleep health in term of workplace accidents and lost productivity, a few recent studies have tried to further our understanding of how sleep deprivation affects moral choice and dishonesty (e.g., Barnes et al., 2011; Christian and Ellis, 2011; Barnes et al., 2015; Welsh et al., 2018).

Though the literature has suggested that insufficient sleep likely increases unethical behavior, very little of this evidence uses direct measures, real world levels of insufficient sleep, or incentivized tasks in coming to these conclusions. We help fill a void in the literature by presenting new evidence from experimentally sleep restricted participants who completed incentivized tasks that can identify dishonesty and possible anti-social choices. Additionally, we modified a theoretical framework based on the idea that deviations from a moral target generate disutility. Here, we can formalize the notion that sleep reduces moral awareness

(Barnes et al., 2015a) and show that this reduced sensitivity to moral concerns when sleep restricted generates the prediction that unethical conduct will increase.

Our results generally support the hypothesis that sleep restriction (SR) leads to increased dishonesty, at least in contexts we examined (the coin flip and the matrix tasks). The one task (Money Burning) where we explored antisocial/deviant choices unrelated to dishonesty was the environment where SR did not affect choices. From our particular experimental design, we are not able to identify whether this represents a fundamental difference in anti-social versus dishonesty choice domains, or whether the harm to other participants that would have resulted from money burning choices is the key factor in these differences. In other words, it may be that our honesty tasks generated a conflict between one's monetary payoff and ethical choices, whereas money burning reduces one's payoff such that there is no conflict between the more pro-social choice (i.e., do not burn money) and one's monetary payoff.²² Or, anti-social versus honesty tasks also differed in who would be harmed as a result of one's dishonest/anti-social choice (the experimenter or another participant)—this was key point made in the review of Köbis et al. (2019), though others found sleep deprivation increased unethical choice when the affected party was another participant in the experiment (e.g., Barnes et al., 2011; Welsh et al., 2014, 2018). Furthermore, our design also used a strategy-method to elicit money burning choices, which differs from a direct choice elicitation approach used in the other two tasks in our study. Specifically, the ex-post random role assignment approach used with strategy-method elicitation requires one to consider outcomes in both roles, which may reduce money burning over direct elicitation. Nevertheless, others have used a similar strategy method approach and observed significant anti-social money burning (Dickinson and Masclet, 2019). Our data also show evidence of some antisocial money burning (i.e., money burning in Scenarios 1-4) as can be seen in Figure 3, but we did not observe a significant difference in this by SR assignment. As such, future research is needed to more fully understanding what may be driving our null findings in the money burning game, especially in light of previous research document an increase in deviant behaviors or decreases in prosocial behaviors when sleep deprived.

In most instances, but not all, we showed that our results are robust with respect to whether we control for sleep using the binary *SR* assignment indicator, an objectively measured (via actigraphy devices) *Average Nightly Sleep* measure, or a hybrid measure we called *Personal SD*. In general, because the binary *SR* assignment was randomized in our design, we

²² We thank an anonymous reviewer for drawing out attention to this point.

have some preference for the model specifications presented in the main text that use the *SR* control. The presentation of results using alternative measures available from our methodology is meant to help convince the reader that our key results with respect to the Coin Flip and Matrix Tasks are fairly robust to alternative measures of the key sleep control variable.

Overall, we feel this research is an important step in our understanding of how insufficient sleep affects specific and consequential areas of decision making. Often, a sole focus on the physical health implications of insufficient sleep has led to a lesser appreciation of its “behavioral health” implications. In operational settings, sleep has not been a significant organizational focus (see Barnes, 2011; Giurge, 2017; Barnes and Watson, 2019), but our results suggest that efforts to extend WHPPs to include sleep hygiene would be wise managerial practice. For example, fatigue management or sleep awareness training, family-friendly policies, sleep disorder referrals, workplace environment characteristics (e.g., lighting), minimizing shift rotations, or reductions in long hours scheduling are all examples that have been suggested by others as ways to improve sleep health in the workplace (Christian and Ellis, 2011; Barnes, 2011; Redeker et al., 2019), and manager have recognized the value of WHPPs in generating indirect benefits to the company (Downey and Sharp 2007). Importantly, because corporate efforts to improve sleep are unlikely to be seen as focused on workplace ethics concerns, a sleep-focused WHPP would not be predicted to crowd out intrinsic moral motivations, which might be the case with a more overt corporate ethics policy (i.e., a more formal institution designed to increase ethical conduct, as noted in Galeotti et al., 2021).

Previous research has consistently supported the hypothesis that sleepy individuals make decisions using less deliberative thought processes. If reduced deliberation leads to increased dishonesty in certain predictable contexts, then this implies that the current state of sleep-deprived societies has consequences that may extend beyond the individual and negatively impact society. Others have found that dishonesty likely increases in the anonymity of the participant (Dickinson and McEvoy, 2021), and so our findings connecting dishonesty to insufficient sleep may be of particular interest under conditions of increased virtual interactions, social distancing, or remote work arrangements, such as those recently experienced due to pandemic response measures. Our study was not designed with this potentially interesting interaction effect in mind, but this may be another area for future research to explore.

This paper contributes in a valuable way to document evidence linking sleep restriction to ethical behavior. Still, our research leaves open questions that will hopefully be addressed going forward. If the general viewpoint is that *SR* promotes less deliberative decision processes, then our results may be taken to suggest that dishonesty is more automatic and it

takes deliberation to overcome the temptation to be dishonest. This may be the case, but the social distance one has from victims of one's dishonesty may yet be an important moderating factor that the present design cannot fully explore due to inherent task differences. A design that could directly test this, for example, would be one where the matrix task, for example, is modified so that in one treatment dishonesty lowers the payoff of another participant in the study (or perhaps even a friend), as opposed to merely affecting the experimenter budget.

Regarding whether the temptation to be dishonest must be overcome with deliberative thought processes, the domain of individual-choice may also be viewed differently than the domain of more socially interactive honesty. Additional research is needed to help identify settings in which honesty is more automatic/natural, because a more general understanding of how sleep loss may impact ethical choice likely depends on such factors. Our participants were college students, and it may also be true that the culture of a college student population is, unfortunately, more desensitized towards what is considered low-consequence dishonesty such that the real ethical dilemma only surfaces when much more is at stake. Such limitations to our findings should help identify future areas where this research may be extended to help improve our understanding of both the decision impacts of insufficient sleep and the cognitive underpinnings of ethical choices. The implications are considerable both for the workplace and beyond.

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FIGURE 1: The Matrix Game task

2.10	1.97	4.27
7.22	3.80	0.99
5.74	4.13	3.75
1.52	6.32	3.56

Got it ☐

5.76	8.31	8.09
6.41	6.89	9.45
7.43	1.26	5.47
3.59	5.79	4.40

Got it ☐

Notes : Example of 2 of the matrix task items. Participants were asked to find and circle two cells in the matrix whose numbers summed to exactly 10.00. The matrix on the left has no solution, while the matrix on the right has a solution. Out of the total of 15 matrices given (all at once on a sheet of paper, such that participants could work on matrices in any order), 11 of them had solutions.

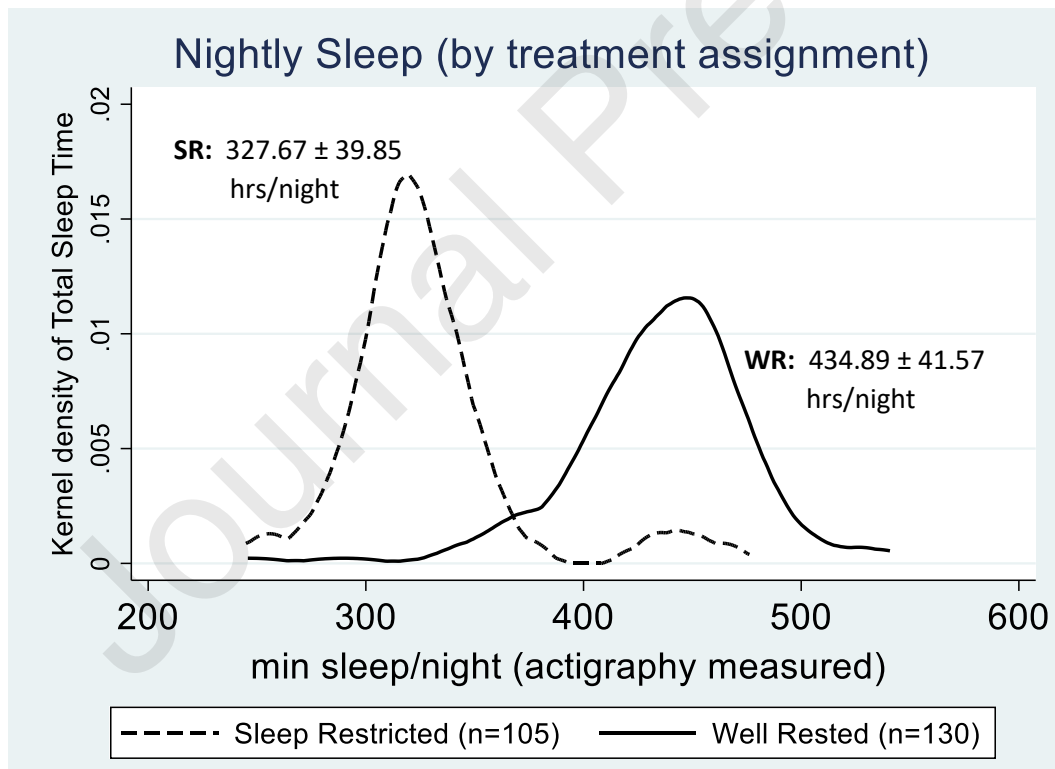
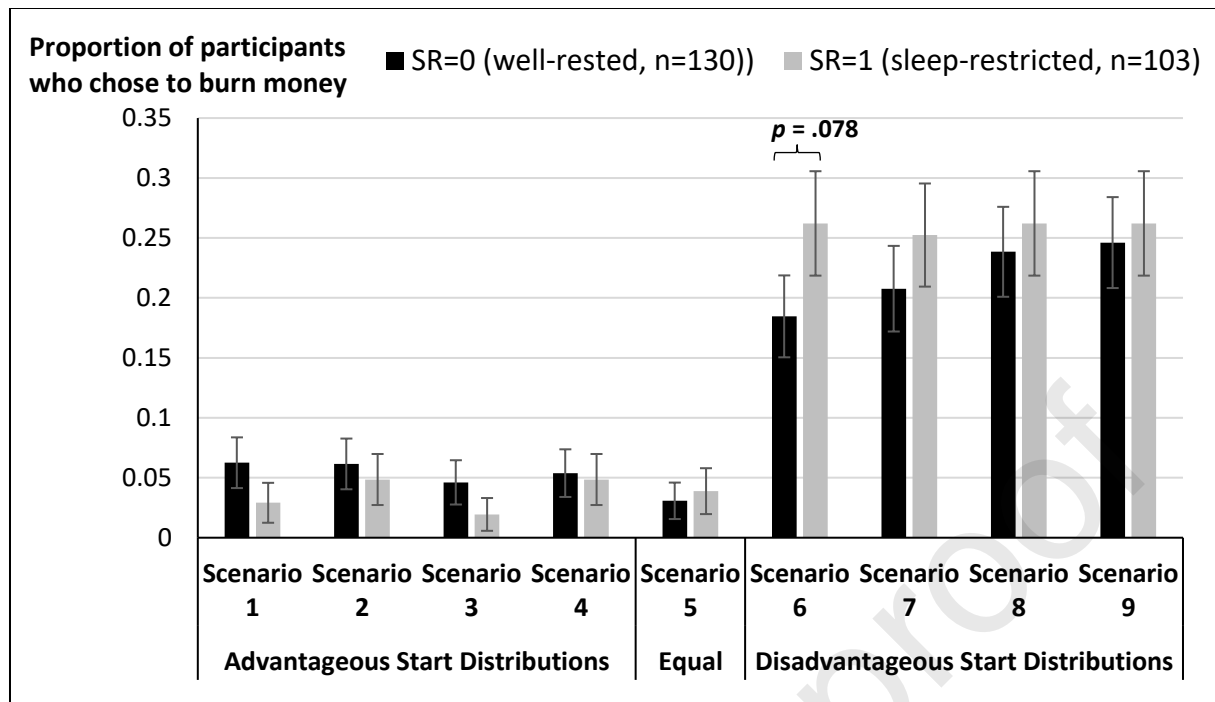
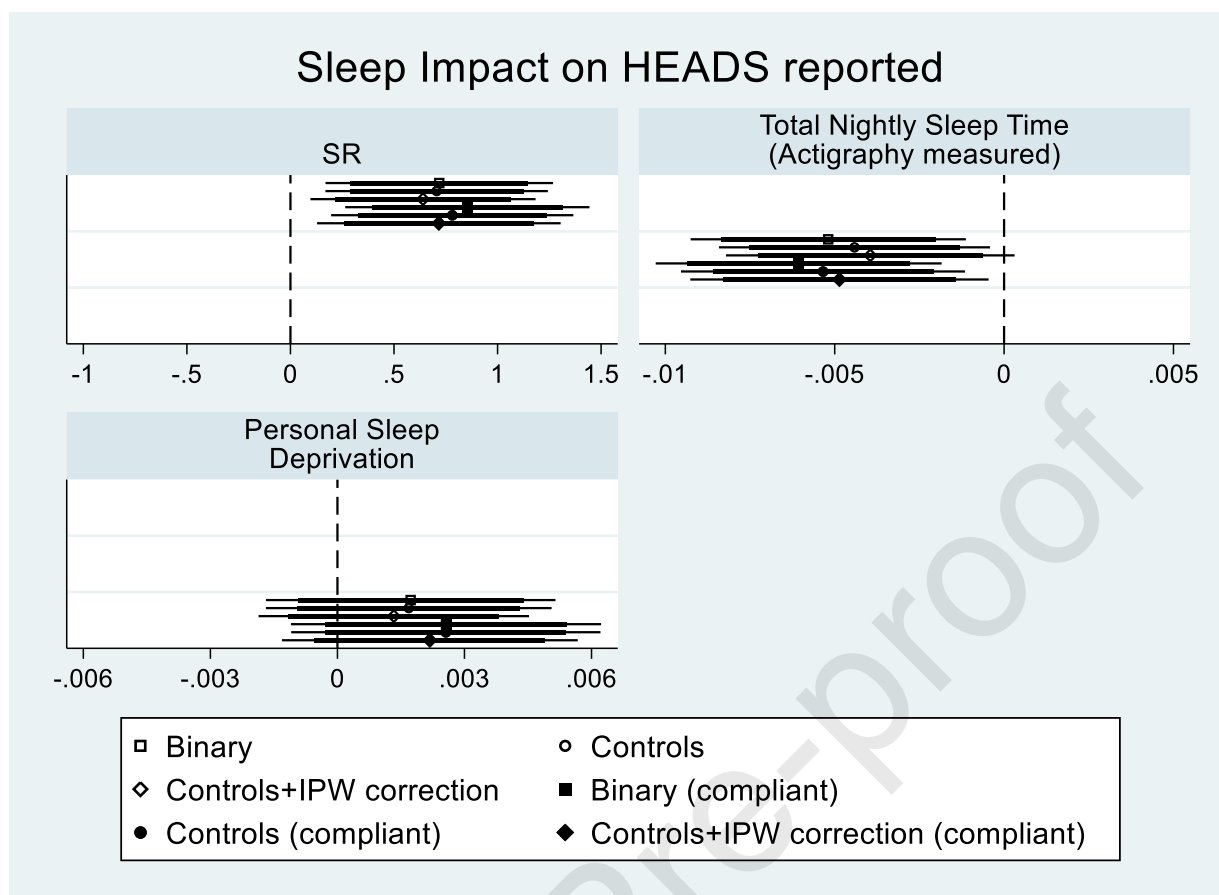
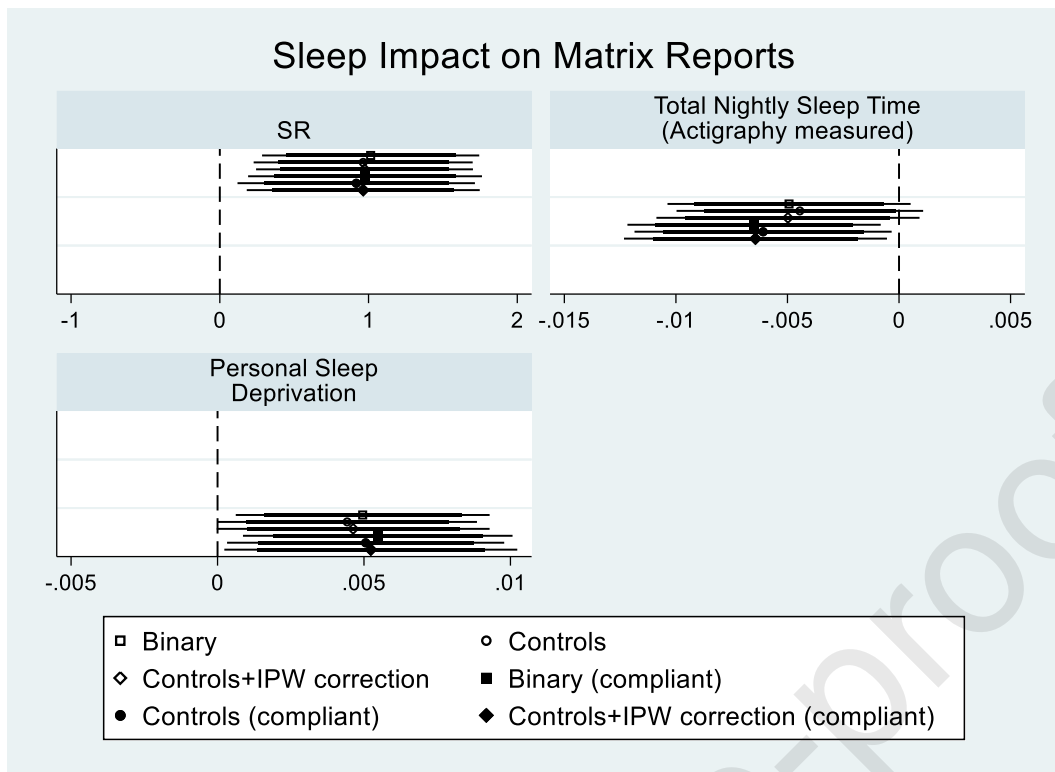
FIGURE 2: Nightly sleep levels by treatment

FIGURE 3: Resource destruction in the money burning game

Notes: Bars represent standard errors of the means. Each binary comparison (well-rested versus sleep restricted) was tested using a two sample proportions test. In all cases except Scenario 6, the tests indicate no statistical difference between the proportion of participants who chose to burn money or not ($p > .10$). For Scenario 6, the direction of the difference implies sleep-restricted participants were marginally *more* likely to burn money. Note that Scenario 6 has the smallest level of disadvantageous inequality in the start distribution of payoffs (see Table 3).

FIGURE 4: HEADS outcomes (sensitivity analysis)

Notes: Thick (thin) lines represent the 90% (95%) confidence intervals for the 1-tailed test of the ex ante hypothesis. IPW (inverse probability weight) regression correction for dropout (attrition) from recruitment to final sample (i.e., completing the protocol). These weights are derived from selection equation using sample of all participants recruited into the study (using demographics and sleep characteristics from the online screening response database, along with treatment assignment, to predict likelihood of being in the final sample).

FIGURE 5: Matrix reports (sensitivity analysis)

Notes: Thick (thin) lines represent the 90% (95%) confidence intervals for the 1-tailed test of the ex ante hypothesis. IPW (inverse probability weight) regression correction for dropout (attrition) from recruitment to final sample (i.e., completing the protocol). These weights are derived from selection equation using sample of all participants recruited into the study (using demographics and sleep characteristics from the online screening response database, along with treatment assignment, to predict likelihood of being in the final sample).

TABLE 1: Methodological comparisons

Study	Sleep Methods (within-subjects, between-subjects, or mixed approach)	Incentives Variable = Variable choice-based \$\$ pay Fixed = flat \$\$ pay	Task(s) (domain)	Key Finding
Reynolds and Schiffbauer (2004)	In-lab TSD (within)	Variable	Experiential discounting (impulsivity)	Delay discounting ↑ with TSD
Zohar et al. (2005)	Observational (within)	None	Disruptive events and affective states survey (emotion response)	Sleep loss ↑ negative affect impact of disruptive event
Barnes et al. (2011)	Self-report (mixed)	Course credit	Unethical work behavior survey (Study 3 and 4) (unethical choice)	Sleep loss ↑ unethical behavior
Christian and Ellis (2011)	Observational (between) + in-lab TSD (between)	Fixed + Variable	Workplace deviance survey (anti-social)	Sleep loss ↑ workplace deviance
Wagner et al. (2012)	Observational (between)	Course credit	Web searching (cyberloafing)	Sleep loss ↑ cyberloafing
Barnes et al. (2015a)	Self-report (mixed)	Fixed + Course Credit	Moral awareness (morals)	Sleep loss ↓ moral awareness
Barnes et al. (2015b)	Observational (mixed)	Study feedback (to company) as incentive	Abusive supervisor behavior (antisocial-behavior) Subordinate engagement	Poor sleep <i>quality</i> of supervisor ↑ abusive behaviors and ↓ work unit engagement
Kouchaki and Smith (2014)	Time-of-day manipulation (between)	Variable	Visual perception + messaging/reporting (dishonesty)	Morning ↑ moral choices compared to afternoon
Welsh et al. (2014)	In-lab TSD (between)	Variable + Course credit	Sender/receiver (deception)	Social influence ↑ but caffeine ↓ the TSD tendency to lie
Welsch et al. (2018)	In-lab TSD + observational (between)	Variable + Course credit + fixed	Sender/receiver (deception)	Control motivation ↓ the TSD tendency to lie
Watkins et al., (2021)	Observational (within)	None	Cyber incivility survey (anti-social)	Sleep loss ↑ cyber incivility
Present Study (n=231)	At-home SR (7 nights, between)	Variable	Coin flip + matrix (dishonesty) + Money burning (anti-social)	SR ↑ dishonesty; no SR effect on money burning

Notes: Review articles or article not providing original data not considered here. Articles reporting results from multiple studies may have more than one method for examining sleep or for task compensation, with distinct methods noted in the relevant cell above. Abbreviations: SR = sleep restriction, TSD = total sleep deprivation. Details regarding sleep measurement (i.e., self-report versus actigraphy) are omitted, but studies varied along this dimension.

TABLE 2: Protocol validity tests

			Session 1 Measure (before treatment week)			Session 2 Measure (after treatment week)		
Measure	Nightly Sleep Time	Perceived Treatment Effect	Karolinska Sleepiness	Irritability	Alertness	Karolinska Sleepiness	Irritability	Alertness
SR-WR difference (Z-stat)	12.699	11.993	1.211	0.212	0.103	10.299	6.740	9.043
p-value	< .001	< .001	= .226	= .832	= .918	< .001	< .001	< .001

Notes: Test are non-parametric Mann Whitney tests of medians. Session 2 measures reported are at the beginning of Session 2. Sleepiness and mood ratings were also assessed at the end of Session 2. Statistically significant difference in Sleepiness, irritability, and alertness between SR and WR groups remain at the end of Session 2, though the effect sizes are reduced.

TABLE 3: Money Burning task decision sheet

Please make your decision as Player A for each of the following scenarios: S1-S9 (recall, one of these will be randomly selected for real payoff)						
Payoffs are listed at (Player A payoff , Player B payoff) You are randomly assigned to counterpart and role only <i>after</i> decisions are made					Which Distribution do you choose?	
Scenario	Start Distribution	Damage (to Player B payoff)	Burning Costs (paid by Player A)	End Distribution	Circle your choice (for each Scenario)	
S1	(500 , 100)	100	20	(480 , 0)	Start	End
S2	(400 , 100)	100	20	(380 , 0)	Start	End
S3	(300 , 100)	100	20	(280 , 0)	Start	End
S4	(200 , 100)	100	20	(180 , 0)	Start	End
S5	(100 , 100)	100	20	(80 , 0)	Start	End
S6	(100 , 200)	100	20	(80 , 100)	Start	End
S7	(100 , 300)	100	20	(80 , 200)	Start	End
S8	(100 , 400)	100	20	(80 , 300)	Start	End
S9	(100 , 500)	100	20	(80 , 400)	Start	End

TABLE 4: Probability of Burning Money

Marginal Effect (SE) displayed	All Subjects		Compliant Subjects	
Independent Variable	Income ≤ Other's	Income ≥ Other's	Income ≤ Other's	Income ≥ Other's
Diff Income	0.0001 (0.0001)	0.0001 (0.0001)	0.0001 (0.0001)	0.0001 (0.0002)
Equal Income (x = y)	-0.189 (0.026)***	-0.043 (0.037)	-0.191 (0.027)***	-0.048 (0.034)
Relative Cost (of burning)	---	0.554 (0.7629)	---	0.649 (0.843)
SR	0.042 (0.037)	-0.013 (0.019)	0.056 (0.037)	-0.001 (0.019)
Epworth	0.009 (0.005)*	-0.003 (0.002)	0.007 (0.005)	-0.002 (0.002)
Age	0.006 (0.006)	-0.002 (0.003)	0.009 (0.006)	0.0004 (0.002)
Female	0.039 (0.037)	0.019 (0.019)	0.038 (0.038)	0.025 (0.018)
Minority	0.060 (0.043)	0.037 (0.024)*	-.072 (0.045)*	0.023 (0.023)
Observations	1155	1155	1015	1015
# subjects	231	231	203	203
Log Pseudo-Likelihood	-529.23	-205.34	-456.96	-167.07

Notes: *.10, **.05, ***.01 for the 1-tailed test on the hypothesized sleep effect (other tests are 2-tailed). Standard Errors clustered at the individual subject level. The null result (lack of support for Hypothesis 1) is unchanged if using alternative sleep measure controls of *Avg Nightly Sleep* or *Personal SD* (results available on request).

TABLE 5: Coin Flip task regressions—Binary SR indicator
(see also Figure 3 coefficient plots)

Dependent Variable = # Reported Heads flipped (out of 15)

Independent Variable	All Subjects			Compliant-Only Subjects		
	(1) Coef (SE)	(2) Coef (SE)	(3) Coef (SE)	(4) Coef (SE)	(5) Coef (SE)	(6) Coef (SE)
Constant	8.804 (0.225)***	10.981 (1.234)***	11.179 (1.039)***	8.622 (0.248)***	10.745 (1.300)***	10.971 (1.087)***
SR	0.718 (0.332)**	0.706 (0.325)**	0.640 (.329)**	0.854 (0.357)***	0.781 (0.354)**	0.716 (0.356)**
Epworth	---	0.042 (0.046)	0.045 (.050)	---	0.032 (0.049)	0.037 (0.053)
Age	---	-0.099 (0.058)*	-0.112 (.045)**	---	-0.092 (0.060)	-0.107 (0.045)**
Female	---	-1.147 (0.335)***	-1.092 (.354)	---	-1.029 (0.369)***	-0.971 (0.399)**
Minority	---	0.465 (0.383)	0.457 (.409)	---	0.371 (0.414)	0.356 (0.442)
<i>IPW correction for sample selection</i>	No	No	Yes	No	No	Yes
Observations	197	197	193	174	174	170
R-squared	.023	.093	.091	.032	.086	.083

Notes: *.10, **.05, ***.01 for the 1-tailed test on the hypothesized sleep effect (other tests are 2-tailed). Sample size reduced by those who chose not to complete this additional (voluntary) online task for extra compensation. Robust standard errors shown for models using the inverse-probability weight (*IPW*) correction for selection. These *IPW*-correction models have sample size reduced by 4 observations in models (3) and (6) due to uncertainties regarding inclusion of these participants in the selection equation estimation missing data on selection equation regressors (e.g., one withdrew at less than 24 hrs from completion due to military orders, another preferred to withdraw but was asked to continue so that we would have an even number of participants for a paired task not reported here).

TABLE 6: Matrix Task regressions—Binary SR Indicator

See also Figure 4 coefficient plots

Dependent Variable = *Matrix Pay* (= \$ amount self-paid in the matrix task)

Independent Variable	All Subjects			Compliant-Only Subjects		
	(1) Coef (SE)	(2) Coef (SE)	(3) Coef (SE)	(4) Coef (SE)	(5) Coef (SE)	(6) Coef (SE)
Constant	1.304 (0.407)***	3.567 (1.770)**	3.673 (1.251)***	1.248 (0.44)***	3.547 (1.893)*	3.685 (1.345)***
Matrix Report	0.914 (0.071)***	0.901 (0.073)***	0.895 (0.068)***	0.931 (0.075)***	0.918 (0.077)***	0.907 (0.069)***
SR	1.014 (0.442)**	0.964 (0.446)**	0.973 (0.441)**	0.976 (0.476)**	0.917 (0.483)**	0.964 (0.474)**
Epworth	---	0.026 (0.062)	0.016 (0.073)	---	0.015 (0.066)	0.005 (0.077)
Age	---	-0.112 (0.078)	-0.114 (0.037)***	---	-0.110 (0.083)	-0.114 (0.039)***
Female	---	-0.298 (0.461)	-0.206 (.500)	---	-0.249 (0.503)	-0.140 (0.548)
Minority	---	-0.177 (0.540)	-0.270 (0.481)	---	-0.179 (0.576)	-0.286 (0.519)
<i>IPW correction for sample selection</i>	No	No	Yes	No	No	Yes
Observations	233	233	233	204	204	204
R-squared	.427	.433	.421	.441	.447	.431

Notes: *.10, **.05, ***.01 for the 1-tailed test on the hypothesized sleep effect (other tests are 2-tailed). *Matrix Report* measures the number of matrices the subject reported correctly completing. Two subjects failed to complete the matrix task though we had complete sleep data on the participant. Robust standard errors shown for models using the inverse-probability weight (*IPW*) correction for selection.

TABLE 7: Probability of Matrix task dishonesty**Dependent Variable = Indicator variable of various levels of dishonesty**

Dep Var→ Independent Variable	Cheat=1 if Over-pay > 0 (1)	Mild-Cheat=1 if 0 < Over-pay < \$5 (2)	Moderate-Cheat=1 if Over-pay > \$5 (3)	Super-Cheat=1 if Over-pay > \$10 (4)
SR	0.064 (0.056)	-0.014 (0.043)	0.078 (0.043)**	0.059 (0.034)**
Observations	233	233	233	233
Log Likelihood	-126.679	-87.490	-79.736	-54.013

Notes: *,.10, **,05, ***,01 for the 1-tailed test on the hypothesized sleep effect (other tests are 2-tailed). **Marginal Effects** (st. error) displayed. As defined, there are 55 (of 233) *Cheat=1* subjects, 29 *Mild-Cheat=1*, 26 *Moderate-Cheat=1*, and 15 *Super-Cheat=1* subjects. Results are similar (sign and significance of coefficient on *SR* indicator) if adding controls, estimating a linear probability model version, or using the *IPW* correction with a weighted linear probability regression (all available on request).

Declaration of Interests: None

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