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Unethical Decision Making and Sleep Restriction: Experimental Evidence

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Abstract: Recent examinations into the cognitive underpinnings of ethical decision making has focused on understanding whether honesty is more likely to result from deliberative or unconscious decision processes. We randomly assigned participants to a multi-night sleep manipulation, after which they completed 3 tasks of interest: imperfectly identifiable dishonesty (the Coin Flip task), identifiable dishonesty (the Matrix task), and anti-social allocation choices (the Money Burning game). We document the validity of the sleep protocol via significantly reduced nightly sleep levels (objectively measured using validated instrumentation) and significantly higher sleepiness ratings in the sleep-restricted (SR) group compared to the well-rested (WR) group. We report that money burning decisions are not statistically different between SR and WR participants. However, regarding honesty, we find significant and robust effects of SR on honesty. In total, given the connection between sleepiness and deliberation, these results add to the literature that has identified conditions under which deliberation impacts ethical choice. When dishonesty harms an abstract "other" person (e.g., the researcher's budget), reduced deliberation more likely increases dishonesty compared to when harm is done to someone at closer social distance (e.g., another subject).

JEL Codes: C91, D91, D63

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

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1. Introduction

Understanding the factors that influence unethical behaviors is crucial, and economists have been working on the determinants of unethical activities for decades. Indeed unethical activities constitute a major concern for firms and organizational research has shed light on the the importance and pervasiveness of such deviant behavior within firms.[1] Unethical activities generate large costs to the affected organizations. For instance, yearly losses due to theft are estimated at over $40 billion (Coffin, 2003), and the annual cost of absenteeism in the United States is estimated at approximately $30 billion (Steers and Rhodes, 1984). In the aggregate, deviant behavior costs organizations as much as $200 billion annually (Murphy, 1993). Moreover, employees'exposure to other employees' deviance can lead to low morale, damaged self-esteem, increased fear at work, create uncertainty, eroded trust among workers, impaired collaborations, and increased turnover (Giacalone, Riordan, & Rosenfeld, 1997).

Traditionally, it is assumed that cheating results from a comparison of the expected pecuniary costs and benefits associated with honest and dishonest behavior. According to the standard economic model of crime, an individual maximizes the expected material payoff when choosing between honest and dishonest behavior (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 (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).

Previous studies suggest heterogeneity across individuals regarding morality preferences. Some people incur high intrinsic costs of unethical behavior such that they may always behave honestly even when this means foregone material benefits (*homo moralis*)[2]. In contrast, others may always cheat unless extrinsic costs are present and outweight the material benefits of dishonesty (*homo economicus*). Between these two extremes cases, many individuals may be conditionally honest and cheat only if the benefits outweigh the intrinsic

[1] 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 Schwieren 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). Schwieren 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)

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

costs.[3] 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 (a)morality is 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 such as social influence, organizational features or choices in moral dilemmas as determinants of unethical activities (e.g., Fox, Spector, & Miles, 2001; Diekmann et al., 2015; Kroher and Wolbring, 2015; Rauhut, 2013; Fortin et al., 2007; Figuieres et al.2013; Kebede and Zizzo, 2015).[4] Other studies have examined how state-level emotions impact dishonesty (Gaudine and Thorne, 2001).

One particular domain that has not received as much attention in the midst of this research is the relationship between insufficient sleep and unethical decision making. In this current paper we use experimental methodology to investigate how sleep restriction may affect unethical behaviors. Precisely, we attempt to explore how a commonplace cognitive state (insufficient sleep) may contribute to unethical behaviors in simple and consequential decision environments. Given the prevalence of insufficient sleep in society, estimated to affect roughly 1/3 of adults in numerous countries (Hafner et al, 2017), this research is timely and has broad implications for understanding unethical behavior in the workplace as well as elsewhere.

We conjecture that individuals will be less likely to resist the temptation to engage in unethical behavior when they are sleep restricted, and we test this hypothesis using validated laboratory decision tasks. There is previous evidence supporting this conjecture (e.g., Barnes et al, 2011; Christian and Ellis, 2011; Barnes et al, 2015; Welsh et al, 2018). 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). In our experiment, we administrated a validated at-home sleep restriction protocol to over 200 participants who then

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

[4] Using a two-period lab experiment, Diekmann et al. (2015) found that confronting subjects with others' cheating in the first round increases cheating in the second round. In sharp contrast, Kroher and Wolbring (2015) did not find any significant effect of showing subjects the prevalence of cheating in other experiments. Rauhut (2013) found that, on average, cheating did not change over four rounds if participants received information on others' behavior in the previous round. However, when distinguishing subjects by their beliefs about others' dishonesty in the respective period, Rauhut (2013) showed that over-estimators reduced dishonesty when they are informed about others' behavior while under-estimators increasde dishonesty. In the context of a tax evasion game, Fortin et al. (2007) investigated whether cheating was influenced by the information about average tax evasion in the previous round. No evidence for dynamic social learning effects was found.

made decisions focused on honesty and antisocial choice. Our full-week sleep restriction protocol approximated the levels of insufficient sleep common in society such that findings are more likely to transfer to real world decision making.

A first contribution of our study is that we test the relationship between sleep restriction and unethical activities by reporting results from three different decision tasks: the money burning task (Zizzo and Oswald, 2001, the coin flip task (Bucciol and Piovesan, 2011; Houser et al, 2012), and the matrix task (Mazar et al, 2008). 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 honesty. The second contribution of our work is that we provide an original theoretical framework for decision making with moral concerns that may help identify key pathways through which sleep restriction may affect choices.

To preview our findings, we observe that sleep restriction increases dishonesty but not antisocial choice. We discuss how these findings fit into the literature on ethical choice. Specifically, the social distance between the decision making and the individual impacted by the choice likely mediates the path connecting deliberation and dishonesty. Models of decision making that incorporate ethical concerns into one's utility function may wish to consider social influences, as well as factors that may dilute the disutility of immoral choice, within their frameworks.

The paper is organized as follows. Section 2 presents the existing literature. Section 3 describes our experimental design and procedures. Section 4 discusses the theoretical predictions and behavioral hypotheses that we propose for evaluation. Section 5 reports our findings. Finally, section 6 discusses our main findings and concludes this paper.

2. 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). Sleep deprived individuals tend to perform normally on standardized tests (Blagrove, et al. 1995; Harrison and Horne, 2000), and they may not suffer from poor decisions if the choice environement is not complex (McElroy and Dickinson, 2019). However, for more complex decision tasks requiring executive function, the lack of sleep likely harm 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).[5]

Research on how sleep affects moral choice or ethical decision making is relatively new. 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 observations and not manipulated. Another paper used a proper sleep manipulation and showed that sleep deprivation increased antisocial deviant behaviors, and this effect was mediated by hostility of the individual (Christian and Ellis, 2011). A focus area of the existing research on sleep and ethical behavior 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). Still others have connected lack of sleep and to reduced moral awareness (Barnes et al, 2015a) or suggested that morning versus evening times may impact morality (Kouchaki and Smith, 2014). The research on morality in the morning 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 and therefore at their highest in the morning. Some have suggested factors that either moderate (e.g, "contemplation", see Welsh et al., 2018) or strengthen this 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.

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,

[5] This region have 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, Baumeister, & Boone, 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).

dishonesty is often considered the more automatic behavior (see survey in Bereby-Meyer and Shalvi, 2015). Depletion of self-control resources, 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 automatic choices, increased honesty in a deception game. This result was supportive of the Social Heuristics Hypothesis (Rand et al, 2014) that argues honesty is more intuitive because it serves one better evolutionarily in everyday life. A recent meta-analysis that reviewed the literature on intuitive honesty versus intuitive dishonesty (Köbis et al, 2019) is perhaps helpful. 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 automatic or intuitive choice is promoted. Conversely, when dishonesty harms other subjects in the lab (i.e., those at reduced social distance), then being honest is more automatic.

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 feature of random assignment and objective measurement of sleep levels resulting from the protocol. First, an online sleep survey was administered to random samples of university students at one of the author's institution. 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.

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. For 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. 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 shall discuss degree of compliance and issues surrounding subject attrition within the one-week protocol. In short, the protocol was intended to produce subjects who have 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 may have been in the protocol 5, 6, or 7 nights at the time of decision making in that task.

The objective measurement of sleep levels means we have more than one way in which we can 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). Yet another option is to combine the nightly sleep average measures with the subject assessment of 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 dictated 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 cross 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 of interest: the money burning task (Zizzo and Oswald, 2001, the coin flip task (Bucciol 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). While such money burning, in general, can be viewed as anti-social given that resources are destroyed, choices to burn may be considered acts to reduce inequality or acts of pure nastiness depending on the allocation being considered (see Table 2). As such, the money burning task is not about honesty, but could be considered within the domain of ethical choice if one considers nastiness as unethical. Table 2 shows the stimulus of the money burning task. This task was administered in the lab session at the end of the sleep protocol and key details of the task were summarized in the stimulus itself. 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 at the end of the experiment (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.[6] This short task was not administered in the lab session but rather was offered as an optional addition experiment task to be taken online by the participant after the 5th night of the sleep protocol (but before the end of the study). Specifically, in the online task participants were asked to locate a coin, flip it 15 times, and report the number of HEADS flipped in total. The temptation for dishonesty was induced by indicating in the instructions that the participant would be paid $0.25 per HEAD flipped (see Appendix B). This payment

[6] 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 & Föllmi-Heusi, 2013) does this in a different way given the potential for multiple outcomes that allow varied degree of cheating.

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 sheet 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 15 \$1 bills. Task details were such that decisions and payments separated and ostensibly anonymous (no subject codes, names, and no payment receipts for this task), and completed matrix sheets were collected separately by the experimenter—any remaining bills left in the unmarked envelope at the lab station would only be retrieved after all participants had left the lab. However, while the participant's assigned sleep study subject code was not used in any materials for this task, the experimenter could match outcomes with payments at the individual level via the participant's lab station location and a secondary coding of the lab station number obfuscated within the experimental laboratory name printed in the footer of the backside of the decision sheet (see Appendix B). As such, honesty at the individual level was identifiable in the matrix task.

4. Theoretical predictions

Here, we aim to explain how sleep deprivation can be linked to self-control, which can in turn be linked to unethical behavior. 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. Morality is captured by $v(a - \hat{a})$, which subtracts from utility for actions that deviate from one's moral imperative, $\hat{a}$. In this framework, social influences may impact one's moral imperative such that reduced social distance between the decision maker and those affected by one's actions may be important. One practical extension of the model may be as follows:

$$U(a) = b(a) - c(a) - \Lambda v(a - \hat{a})$$

Here, consider that $\lambda \in [0,1]$ is a weight that can blunt the utility cost of deviation from one's moral target. In other words, λ decreases in sleep deprivation due to reduction prefrontal resources necessary for moral choice. In this way, the full disutility of deviation from one's moral target is only felt by someone fully alert (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, lack of sleep may produce a diminished ability to control impulsive and potentially deviant behaviors. Such a framework can also be used to explain how any temporal or environmental factor, no just sleep, may impact utility in a way that affect moral choice (e.g., Lu et al, 2018).

Though Masclet and Dickinson (2019) consider that social influences one's moral target via the parameter $\hat{a}$, here we can also consider social influences via the λ weight (see Gino et al, 2009, on social influences on unethical choice). A more anonymous decision environment, or one where decision impacts on others are more abstract and hard to identify, may reduce the disutility of deviations from one's moral target. This specification would then imply that more unethical choice may result from depletion of cognitive resources via λ, all else equal. Alternatively, there is evidence in the sleep literature that may indicate sleep deprivation blunts the impact of an action's costs in one's utility and/or magnifies the anticipation of potential gains from a particular choice (e.g., Venkatraman et al, 2007, 2011). Thus, it is also conceivable that our hypotheses derive from the prediction that sleep deprivation will enhance the expected marginal benefit, $b(a)$, of an action and/or reduce the expected marginal cost, $c(a)$, of that action. All three of the potential mechanisms, which we cannot disentangle in this paper would lead us to the same hypotheses we enumerate below. In addition, for a given level of sleepiness (or cognitive resource depletion) a more direct impact of unethical choices on others would sharpen the utility consequences of a deviation from one's moral target (i.e., increased λ, all else equal), which motivates a hypothesis focused on one's social distance to those impacted by a decision.

In formulating our hypotheses, we therefore take into account the prior literature identifying sleep and moral choice, as well as the qualitative predictions of ethical behavior differences that likely appear when decisions impact individuals at reduced social distance to the decision maker, such as considered in the framework above. In doing so, we are also mindful of the fact that antisocial choices in the Money Burning game are not equivalent to "dishonesty", but it is also the case that it is common knowledge that Money Burning will impact another subject in the experiment (i.e., reduced social distance of impact). In contrast, dishonesty in the Coin Flip or Matrix tasks impact the more abstract "experimenter budget", which may be seen as similar to affecting someone at greater social distance to the decision maker than another subject in the experiment.

Hypothesis 1a: Sleep Restriction (SR) will increase resource destruction in the Money Burning task

Hypothesis 1b: SR will not just increase money burning that reduces disadvantageous inequality but also money burning that is considered nasty.

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.

Hypothesis 4: SR will more strongly predict dishonesty in the Coin Flip and Matrix tasks than anti-social choice in the Money Burning game.

5. Results

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.

Assessment of the sleep protocol is an important first step in evaluating the validity of the sleep manipulation methodology. Figure 2 and Table 1 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 is greater than 375 min/night or WR-assigned with sleep levels of <405 min/night.[7]

[7] 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

In general, we will report results from both the full sample and restricted sample of "compliant" subjects. The full sample captures the benefits of random assignment without the same selection bias concerns that are present in considering the reasons behind noncompliance.

The protocol was successful at generating significant differences in nightly sleep levels and, as shown in Table 1, 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 estimated 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.

We first evaluate results from the Money Burning task in using panel methods of the choices over the 9 allocation scenarios shown in Table 2, 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). We considered demographic and allocation descriptors in all panel estimations in Table 2. Important descriptors of the allocation scenario involve the payoff difference between the two individual, 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 of the probability a subject choose to burn money in a given scenario are shown for 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. Scenarios 1-5 in Table 2

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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.

are rather different from scenarios 6-9 in the sense that the decision maker is at a payoff *advantage* in the Start Distribution. As such, a money burning choice would be considered somewhat “nasty” (see Zizzo and Oswald, 2001; Abbink and Sadrieh, 2009; Abbink and Hermann, 2011). We therefore conduct 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.

Marginal effects of the probability of burning money are reported in the Table 3, which are based on robust standard errors clustered at the individual subject level.[8] As can be seen in Table 3, 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 by our subjects, and this does not differ based on SR assignment.[9] This null result is robust with respect to a sample selection correction as well (see Appendix Table A2). Thus, our data fail to support *Hypotheses* 1a and 1b.

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 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

[8] Models without demographic controls yield the same qualitative results in terms of sign and significance 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.

[9] 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.

statistically significant coefficient estimates on the *SR* indicator variable across specifications in Table 4 support Hypothesis 2 that sleep restriction increases the number of HEADS reported.

Additional sensitivity analysis for Hypothesis 2 is shown in the coefficient plots of the Coin Flip task analysis. This Figure 3 shows the estimates of the key sleep effect results on coin flip outcomes using alternative measures to capture the sleep restriction effect. 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* (see Appendix Tables A3 and A4 for full estimation details of these additional models). While these two alternative approaches to controlling for sleep impacts in the data more fully exploit the continuous nature of the data we have from the actigraphy measures, 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, with *Personal SD* perhaps being open to some additional criticism due to the combination of the objective sleep and subjective sleep need in its construction

In contrast to our money burning estimates, which failed to support Hypotheses 1a and 1b, results in Table 4 and Figure 3 (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 available on request). Regarding independent variables used to control for sleep, Figure 3 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* and, to some extent, *Hypothesis* 4, 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 definitively claim cheating due to increased HEADS reported above the statistical expectation.

Finally, we turn to the results from the Matrix Task, which allows identification of dishonest overpayment at the individual participant level. Table 5 shows results of several specifications estimating the predictors of the number of matrices for which one paid oneself, *Matrix Pay.* Figure 4 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. We consider it an accidental overpayment if the subjects indicated a correctly solved matrix but was mistaken, and yet made a self-payment based on that mistaken number of correct matrices reported. To account for this, we include an additional control in the estimations for *Matrix Report*, which captures the total number of matrices the subject reportedly solved. Self-payments that are 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 statically different from 1 (Wald tests: $p > .10$ in each instance), which is consistent with subjects self-payment increasing by $1 for each additional matrix reportedly solved (whether that is correct or not).

As can be seen from Table 5 and Figure 4 (see also Appendix Tables A5 and A6), we find robust evidence that, even after controlling for the number of matrices reportedly solved, SR increases *Matrix Pay* by about an extra $1. Given average value of *Matrix Report* of about 3.91, this represents an increase of approximately 25% in average dishonest payments, identifiable at the individual subject level. The left panel coefficient estimates derived from Appendix Table A5 using the binary assignment *SR* control show robust evidence level that sleep restriction increases the amount of payoff one takes in the Matrix task. The right and bottom panels of Figure 4 show sensitivity analysis of the same set of models that use the continuous *Avg Nightly Sleep* or *Personal SD* measure as the sleep control in place of the *SR* indicator, respectively. While estimation precision is reduced slightly when using these continuous sleep measures, in general, for the models estimated on compliant participants the statistical significance remains ($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 generate a measure of the severity of one's dishonsesty. To investigate this further, we also 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 are reported in Table 6. 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 6 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, and somewhat support Hypothesis 4 (given the lack of SR impact on Money Burning). These results from Table 6 estimations are robust to controlling for demographic and sample selection using the *IPW* correct, but they lose 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.

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 seven hours or more of nightly sleep (Ford et al, 2015; Hafner et al, 2017; Hirschkowitz et al, 2015; Jones, 2013; Watson et al, 2015). According to the National Sleep Foundation, the number of Americans who sleep fewer than six hours per night increased from 13 to 20 percent between 1999 and 2009 (NSF, 2009). Sleep research is also relevant to organizational scholars and economists, as organizations often contribute to a growing incidence on sleep deprivation. 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.

Sleep deprivation may have disastrous consequences in several domains (Bonnet and Arand,1995; Ferrara and De Gennaro, 2001) including 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. Beyond these direct negative consequences of sleep deprivation 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 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 anti-social choices, likely (though not provable) dishonesty, and individual-level identifiable and provable dishonesty.

Our results generally support our hypotheses that sleep restriction (SR) leads to increased dishonesty, at least in environments where other subjects are not harmed by the dishonest behavior. The one environment (Money Burning) where antisocial choices were measured, as opposed to dishonesty, was the one 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. Future research will have to address this key variable in a more systematic way.

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 have some preference for the model specifications presented in the main text that use the *SR* contro. 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. 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 areas where individuals feel others are not harmed (or, those harmed are at great social distance and therefore more “abstract”), then this implies that the current state of sleep-deprived societies has consequences that may extend beyond the individual and negatively impact society. This may be particularly true under current conditions of increased virtual interactions and social distancing due to pandemic response measures.

While relatively little research has been done in the area of sleep and ethical choice, this paper contributes in a valuable way to document evidence linking sleep restriction to dishonest behavior. Still, our research leaves open questions that will hopefully be addressed in the future. If the general viewpoint is that SR promotes less deliberative and more automatic 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 the likely victim's of dishonesty may be an important mediating factor. It is perhaps also the case 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. These 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 anti-social or unethical choices.

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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

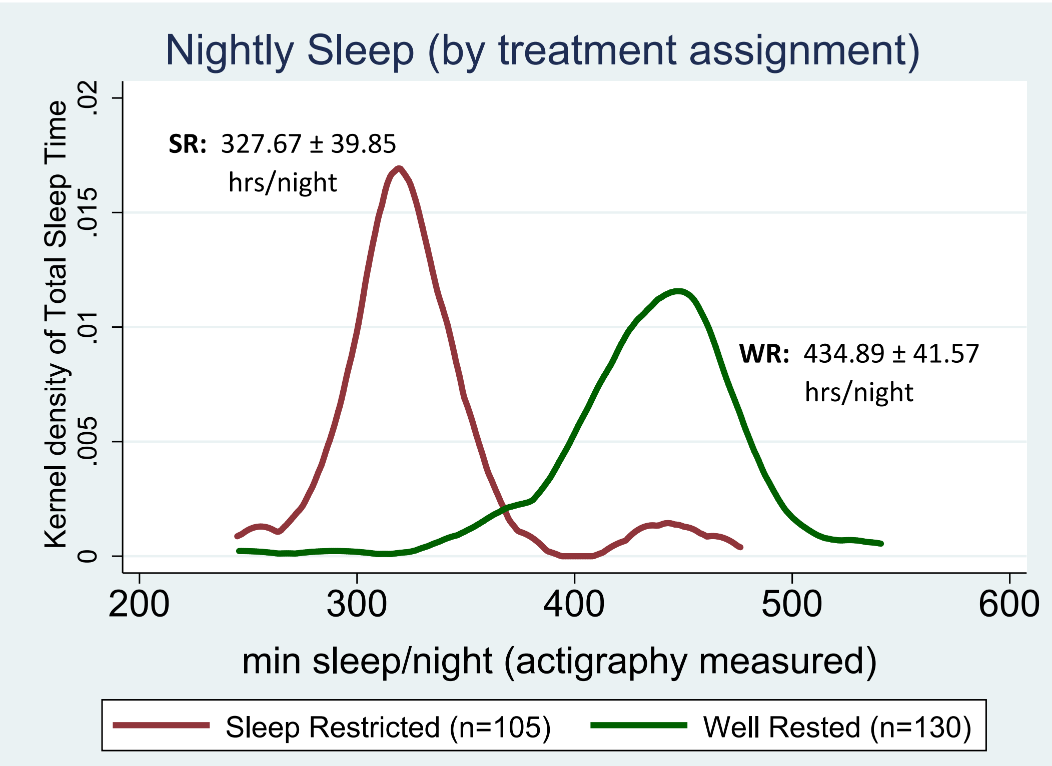
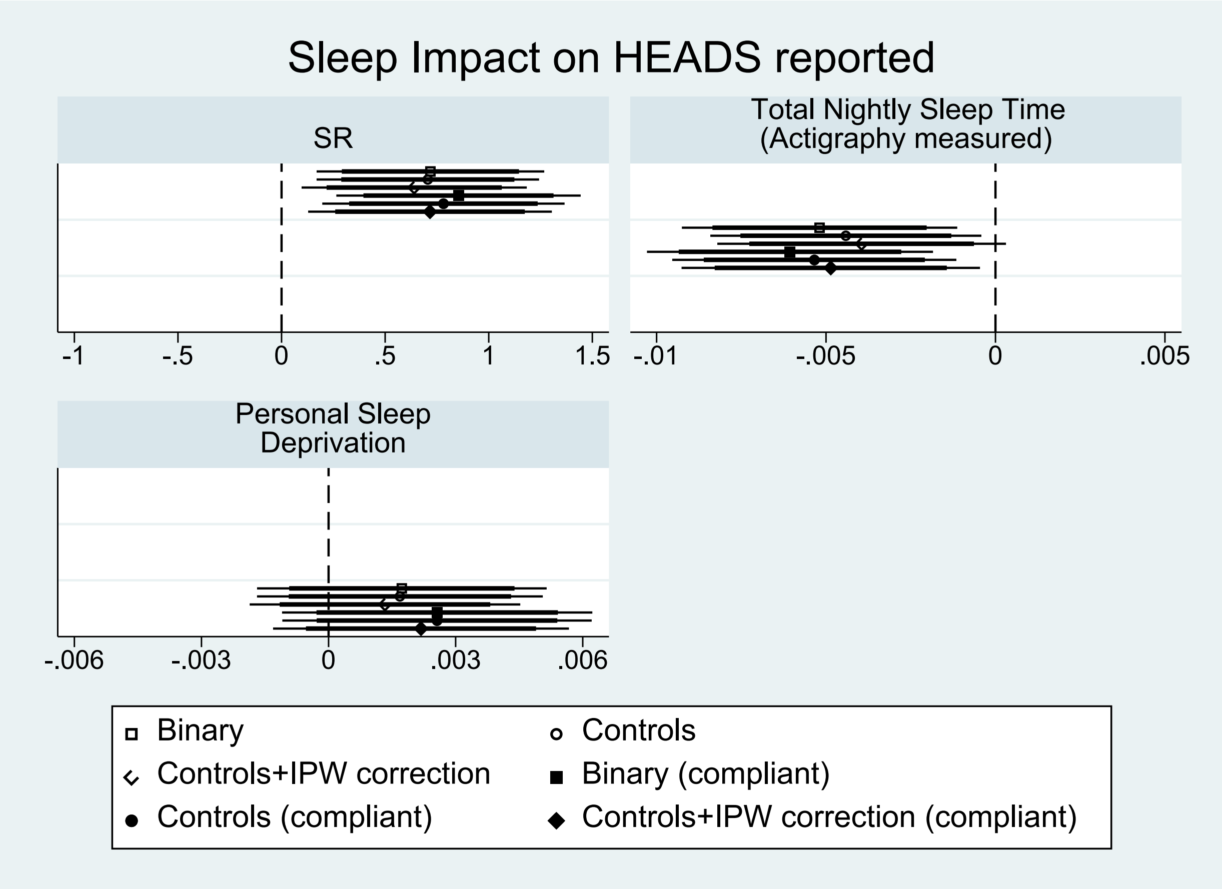
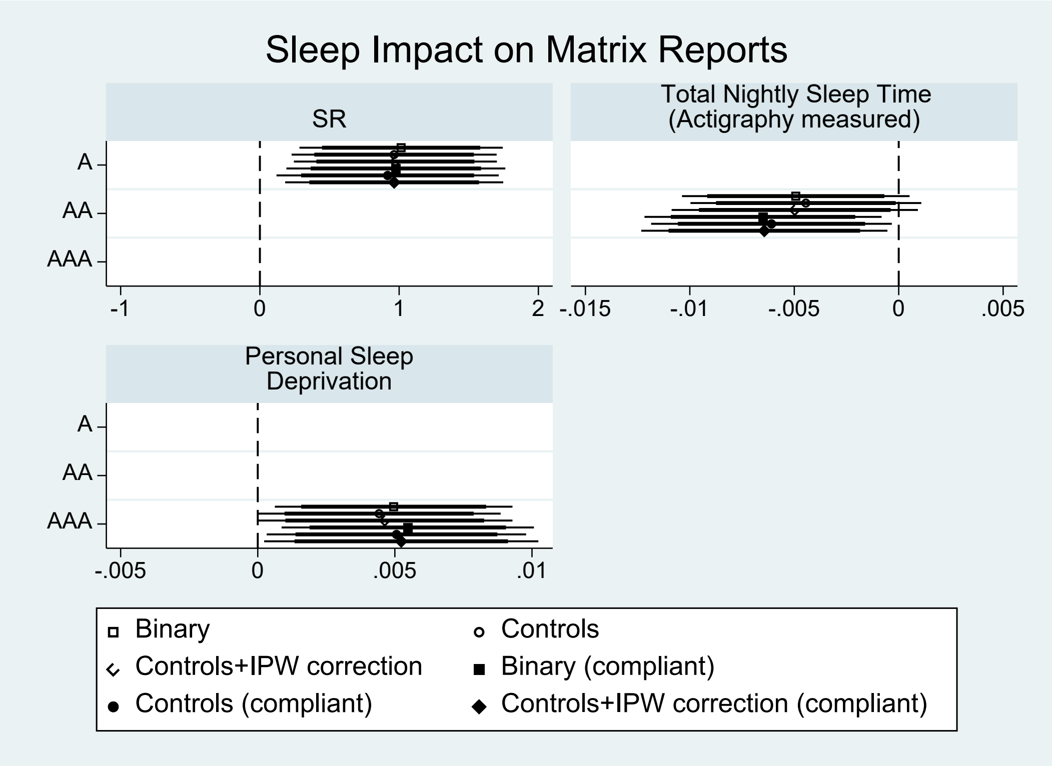


FIGURE 3: HEADS outcomes (sensitivity analysis)



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 4: 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: Protocol validity tests

Measure	Nightly Sleep Time	Perceived Treatment Effect	**Session 1 Measure (before treatment week)** Karolinska Sleepiness	Irritability	Alertness	**Session 2 Measure (after treatment week)** 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

Note: 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 2: 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 *after* 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 Independent Variable	All Subjects		Compliant Subjects	
	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 Hypotheses 1a and 1b) is unchanged if using alternative sleep measure controls of *Avg Nightly Sleep* or *Personal SD* (results available on request).

TABLE 4: Coin Flip task regressions—Binary SR indicator
(see 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)
IPW correction for sample selection	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 efforts 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 hers 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 5: Matrix Task regressions—Binary SR Indicator
See 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)
IPW correction for sample selection	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 efforts shown for models using the inverse-probability weight (*IPW*) correction for selection.

TABLE 6: Probability of Matrix task dishonesty

Dependent Variable = Indicator variable of various levels of dishonest

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).

APPENDIX A: Additional estimation details

TABLE A1: Sample Selection and Attrition Analysis (used for IPW estimations)

Probit Estimation **Variable**	**Dep Var =** **Finished Protocol (=1)** (*Conditional on being recruited*) Coefficient (SE)
SR (=1)	-.568 (.192)***
Female (=1)	-.156 (.203)
Minority (=1)	.096 (.248)
Age	-.047 (.030)
Optimal Sleep	-.117 (.095)
Anxiety Risk	-.035 (.039)
Depression Risk	.138 (.131)
Epworth	.062 (.030)**
Reduced-MEQ	.039 (.031)
Observations	N=279
Log Likelihood	-115.01616

Notes: Full recruited sample of n=279 participants, n=258 started the protocol (i.e., showed up for Session 1) and n=237 finished the protocol (a small number lacked complete sleep data or failed to complete a task, as reflected in sample sizes for individual tasks). *p<.10, **p<.05, ***p<.01 for the 2-tailed test. Predicted likelihood of protocol completion for each participant used to determine weights for selection correction based on inverse probability weighting (IPW) in individual outcomes analysis.

TABLE A2: Probability of Burning Money
Inverse Probability Weight correction for sample selection

Marginal Effect (SE) displayed **Independent Variable**	**All Subjects**		**Compliant Subjects**	
	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)	-0.191 (0.026)***	-0.038 (0.039)	-0.194 (0.028)***	-0.042 (0.036)
Relative Cost (of burning)	---	0.463 (0.747)	---	0.547 (0.829)
SR	0.043 (0.037)	-0.013 (0.019)	0.053 (0.038)	-0.002 (0.019)
Epworth	.010 (0.005)*	-0.003 (0.002)	0.007 (0.005)	-0.001 (0.020)
Age	0.007 (0.006)	-0.002 (0.002)	0.010 (0.006)	-0.0003 (0.002)
Female	0.043 (0.037)	0.016 (0.018)	0.040 (0.038)	0.025 (0.018)
Minority	0.055 (0.043)	0.040 (0.025)*	0.070 (0.045)	0.025 (0.024)
Observations	**1155**	**1155**	**1015**	**1015**
# subjects	**231**	**231**	**203**	**203**
Log Pseudo-Likelihood	**-527.87**	**-201.59**	**-457.44**	**-165.85**

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. Number of subjects reflects reduction due to 4 subjects for which we lacked complete data from the task. Results are all robust to use of a continuous average nightly sleep time (minutes per night) or a personal sleep deprivation variable to control for sleep condition rather than the dichotomous SR indicator. Results available on request.

TABLE A3: Coin Flip task regressions—Continuous Aveage Nightly Sleep Time measure
see 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	11.166 (0.967)***	13.039 (1.521)***	13.012 (1.465)***	11.401 (1.002)***	13.268 (1.585)***	13.247 (1.517)***
Avg Sleep Time (min/night)	-0.005 (0.002)**	-0.004 (0.002)**	-0.004 (0.003)*	-0.006 (0.003)***	-0.005 (0.003)**	-0.005 (0.003)**
Epworth	---	0.040 (0.046)	0.044 (0.051)	---	0.031 (0.049)	0.036 (0.053)
Age	---	-0.100 (0.058)*	-0.112 (0.046)**	---	-0.095 (0.060)	-0.108 (0.047)**
Female	---	-1.098 (0.338)***	-1.040 (0.361)***	---	-1.007 (0.371)***	-0.040 (0.405)**
Minority	---	0.411 (0.384)	0.401 (0.412)	---	0.031 (0.049)	0.298 (0.053)
IPW correction for sample selection	No	No	Yes	No	No	Yes
Observations	**196**	**196**	**192**	**173**	**173**	**169**
R-squared	**.023**	**.086**	**.085**	**.032**	**.084**	**.082**

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 and one additional participant with corrupted actigraphy data. Robust standard efforts 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 A4: Coin Flip task regressions—Continuous *Personal Sleep Deprivation* measure
see 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	8.978 (0.259)***	11.740 (1.308)***	11.451 (1.071)***	8.802 (0.277)***	11.573 (1.370)***	11.274 (1.130)***
Personal SD (min/night)	.002 (0.002)	.002 (.002)	.001 (.002)	.003 (.002)	.003 (.002)	.002 (.002)
Epworth	---	.036 (0.047)	.043 (0.051)	---	.028 (0.050)	.036 (0.054)
Age	---	-0.127 (0.062)**	-.116 (0.046)**	---	-.125 (0.064)*	-.113 (0.047)**
Female	---	-1.129 (0.344)***	-1.082 (0.360)***	---	-1.050 (0.379)***	-1.002 (0.406)**
Minority	---	.430 (0.401)	.432 (0.3413)	---	.349 (0.435)	.353 (0.447)
IPW correction for sample selection	No	No	Yes	No	No	Yes
Observations	**192**	**192**	**192**	**174**	**174**	**170**
R-squared	**.0037**	**.0765**	**.0739**	**.0080**	**.0704**	**.0665**

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, 1 participant whose actigraphy data were corrupted, and 4 participants for whom we did not have the self-perceived sleep need measure (needed to construction the *Personal SD* variable—these participants were also those dropped from the *IPW* estimations when using the *SR* indicator or *Avg Sleep Time* measure). Robust standard efforts shown for models using the inverse-probability weight (*IPW*) correction for selection.

TABLE A5: Matrix Task regressions— Continuous Average Nightly Sleep Time measure
See 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	3.638 (1.331)***	5.725 (2.107)***	6.020 (2.240)***	4.243 (1.389)***	6.393 (2.223)***	6.672 (2.369)***
Matrix Report	0.915 (0.071)***	0.904 (0.073)***	0.897 (0.079)***	0.925 (0.075)***	0.913 (0.077)***	0.902 (0.070)***
Avg Sleep Time (min/night)	-0.005 (0.003)*	-0.004 (0.003)*	-0.005 (0.004)*	-0.006 (0.003)**	-0.006 (0.003)**	-0.006 (0.004)**
Epworth	---	0.026 (0.062)	0.015 (0.074)	---	0.011 (0.066)	0.001 (0.078)
Age	---	-0.115 (0.078)	-0.115 (0.037)***	---	-0.111 (0.083)	-0.113 (0.039)***
Female	---	-0.260 (0.469)	-0.141 (0.520)	---	-0.220 (0.506)	-0.106 (0.548)
Minority	---	-0.235 (0.541)	-0.342 (0.492)	---	-0.246 (0.576)	-0.363 (0.534)
IPW correction for sample selection	No	No	Yes	No	No	Yes
Observations	**232**	**232**	**232**	**204**	**204**	**204**
R-squared	**.423**	**.430**	**.418**	**.439**	**.445**	**.430**

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. One participant completing the task had corrupted sleep watch data and had missing *Total Sleep Time* data (but could still be used for estimation based on binary *SR* assignment. Robust standard efforts shown for models using the inverse-probability weight (*IPW*) correction for selection.

TABLE A6: Matrix Task regressions— Continuous *Personal Sleep Deprivation* measure
See 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	1.251 (0.434)***	3.519 (1.791)*	3.607 (1.283)***	1.201 (0.459)***	3.522 (1.904)*	3.673 (1.369)***
Matrix Report	0.918 (0.071)***	.906 (0.073)***	.899 (0.069)***	.929 (0.075)***	.916 (0.077)***	.905 (0.069)***
Personal SD (min/night)	.005 (.003)**	.004 (.002)**	.005 (.003)*	.005 (0.003)**	.005 (0.003)**	.005 (.003)**
Epworth	---	.023 (0.062)	.012 (0.075)	---	.011 (0.066)	.001 (0.078)
Age	---	-.111 (0.078)	-.112 (0.036)***	---	-.111 (0.083)	-.114 (.039)***
Female	---	-272 (0.465)	-.166 (0.507)	---	-.244 (.504)	-.138 (0.541)
Minority	---	-.115 (0.545)	-.218 (0.472)	---	-.093 (0.583)	-.209 (0.510)
IPW correction for sample selection	No	No	Yes	No	No	Yes
Observations	**232**	**232**	**232**	**204**	**204**	**204**
R-squared	**.4263**	**.4325**	**.4201**	**.4398**	**.4454**	**.4293**

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. One participant completing the task had corrupted sleep watch data and had missing *Total Sleep Time* data such that the *Personal SD* variable could not be constructed (but could still be used for estimation based on binary *SR* assignment. Robust standard efforts shown for models using the inverse-probability weight (*IPW*) correction for selection.

Appendix B : Experiment Instructions

THE MONEY BURNING TASK:

Instructions

In this task you will be randomly assigned with another subject in the room today. Your counterpart will remain anonymous to you and you will remain anonymous to your counterpart. There are two roles in this task: Player A and Player B. There are also several decision scenarios (S1-S9) where you are asked to make a decision, as seen in the table below. For each of these 9 scenarios, you are asked to choose between either the "Start Distribution" of payoffs or the "End Distribution" of payoffs. Payoffs (in cents) are listed in parenthesis, and the payoff amount you would receive is listed **first,** while the payoff listed second would be the payoff received by your counterpart for that distribution of payoffs. For example, if you choose a payoff distribution of (Y , Z), then your payoff would be Y cents, and your counterpart would be Z cents. The difference between the "Start Distribution" and the "End Distribution" is that the "End Distribution" subtracts 100 cents off of Player B's payoff, and 20 cents off of Player A's payoff. So, if you choose the "End Distribution" in a particular decision scenario, then you as Player A are choosing to "burn" 100 cents of the counterpart's payoff (i.e., the "damage") at a cost to you (Player A) of 20 cents (i.e., the "burning costs"). It is completely up to you as Player A to choose the "Start" or "End" Distribution for none, some, or all of the decision scenarios shown below.

You will notice that the counterpart has no decision to make in this task and is simply a passive recipient of your decision. *However, all subjects in the room today will make decisions as if he/she may be assigned as Player A.* **Only after all decisions are made will we randomly match you with a counterpart**, **then we will randomly assign one of you as Player A (the other is Player B), and we will also then randomly select one of the nine scenarios, S1-S9, to count for both you and your counterpart's payoff in this task.** In other words, every subject is equally likely to be a Player A or a Player B in this task, and you will not know your assigned role until all decisions are made. Therefore, you should carefully make your decisions as Player A as if each one may be the one that determines your payoff (because it might!), but it is also possible that you will be assigned as the Player B in your pair such that your payoff will be determined by your counterpart's decision for the randomly selected payoff scenario. Remember, you will not know your assignment as Player A or B until after all decisions are made and after a single payoff Scenario is randomly drawn. Also remember that neither you or your counterpart will know of the other's decision before you must make your own decision, and you will never know the identity of your randomly matched counterpart (and vice versa).

Do you have any questions before you start?

NOTE: These payoffs are in "cents" (not "tokens"). So, 100 cents=$1.00 payoff, for example.

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 *after* 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

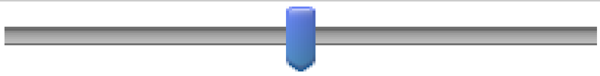
THE COIN FLIP TASK :

(highlights shown were included in survey text)

This question offers you the chance for additional compensation, so please read carefully.

Please find a coin (any coin with Heads and Tails sides to it). **Flip the coin 15 times** and record the number of HEADS you flip. Please only perform the 15 coin flips once. **Your payoff for this task will be $0.25 (i.e., 25 cents) for each HEADS outcome from these coin flips**, and this payment will be added to your Amazon gift code compensation for this survey. For example, if you flip HEADS 3 times, your additional compensation (on top of the flat $5 for completing the survey) will be another $.75. Similarly, if you flip HEADS 6 times you will receive an additional $1.50, if you flip HEADS 11 times you will receive an additional $2.75, etc.

Please tell us below how many times you flipped HEADS out of the 15 coin flips, and we will add 25 cents for each HEADs flipped to your survey payoff when you receive your Amazon gift code.

	0 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15
Number of HEADS outcomes from flipping coin 15 times	

THE MATRIX TASK:

INSTRUCTIONS:

Thank you for agreeing to take part in this experiment. Please read along as the instructions are read aloud. A clear understanding of the instructions will help you make better decisions and increase your earnings. Your earnings will be determined only by your own decisions in this task. **Do not communicate with the other participants.** If you have any questions, please ask us. Although there are many people participating in today's experiment, everyone is working independently. This means that your earnings in this task are based entirely on your decisions and what others decide has no effect on you.

All decisions that you make today are recorded only by an anonymous subject number (*not* your sleep study code) and will only be used for research purposes. Your decisions will remain completely anonymous.

Please read the following instructions on the task specifics before we start.

In the large envelope at your station, you will find a sheet with 15 matrices like the one below (front and back side of sheet). Do not open the envelope until we start the experiment.

Example

3.91	0.82	3.75
1.11	1.69	7.94
3.28	2.52	6.25
9.81	6.09	2.46

In each matrix, you should look for a **unique pair of numbers that sum up exactly to 10**. In some matrices there may not be a solution.

When you find a pair, circle the numbers, and mark the corresponding "Got It" box, as in the following example:

Example

(3.91)	0.82	3.75
1.11	1.69	7.94
3.28	2.52	6.25
9.81	(6.09)	2.46

Got It ☒

For each correct matrix solution, you will receive $1.00.

You will have 4 minutes to complete this task. After the 4 minutes is up, you will need to do the following:

- Count the number of correctly solved matrices and indicate that number on the back side of the matrix sheet. This will be your earnings.
- Also inside the large envelope is a smaller envelope containing 15 one-dollar bills. Now pay yourself from this money (stick your earnings in your pocket, wallet, purse, whatever). Leave the extra one-dollar bills in the envelope, seal the envelope, and leave it at your computer station. You will **not** have to sign any receipt for your earnings on this task. That envelope with the remaining

one-dollar bills will remain sealed until after all participants have left the lab, and will be separated from your matrix task sheet.

- Put your matrix task sheet and these instructions (i.e., everything **except your earnings and the sealed small envelope with the extra $1 bills)** in the large envelope and seal the large envelope. It will remain sealed until after all participants have left the lab.
  - A box will be brought around to each station. Drop the large envelope in the box (shuffle its location in the box….we do not care). These large envelopes containing the matrix task outcomes will not be opened until after all participants have left the lab, and you will note they have been separated from the small envelope.

Matrix Task indentification method:

Example of the footers on front and back side of duplexed decision sheet (15 matrices on sheet were split across front and back side of sheet, with space on back side to report total number of matrices solved). AppEEL is the name of the experimental economics laboratory used.

Footer on front side of page:

Appalachian State University		
Economics & AppEEL		
Center for Economic Research and Policy Analysis		

Footer on back side of page (with station #7 indicated between "Economics" and "AppEEL"):

Appalachian State University		
Economics 7 AppEEL		
Center for Economic Research and Policy Analysis		