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Recommended Citation

Dickinson, D. L., & McElroy, T. (2017). Sleep restriction and circadian effects on social decisions. *European Economic Review*, 97, 57–71. <https://doi.org/10.1016/j.euroecorev.2017.05.002>

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Elsevier

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PII: S0014-2921(17)30087-9
DOI: [10.1016/j.eurocorev.2017.05.002](https://doi.org/10.1016/j.eurocorev.2017.05.002)
Reference: EER 2993

To appear in: *European Economic Review*

Received date: 19 October 2016
Accepted date: 8 May 2017

Please cite this article as: David L Dickinson , Todd McElroy , “Sleep restriction and circadian effects on social decisions.”, *European Economic Review* (2017), doi: [10.1016/j.eurocorev.2017.05.002](https://doi.org/10.1016/j.eurocorev.2017.05.002)

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“Sleep restriction and circadian effects on social decisions.”

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Abstract: Our study examines how chronic sleep restriction and suboptimal times-of-day affect decisions in a classic set of social tasks. We experimentally manipulate and objectively measured sleep in 184 young-adult subjects, who were also randomly assigned an early morning or late evening experiment session during which decision tasks were administered. Sleep restriction and suboptimal time-of-day are both estimated to either directly or indirectly (via an impact on sleepiness) reduce altruism, trust, and trustworthiness. We conclude that commonly experienced adverse sleep states, most notably chronic sleep restriction, significantly reduce prosocial behaviors, and can therefore limit benefits from short-term social interactions.

Keywords: Trust, dictator, social behavior, sleep restriction, circadian

JEL codes: C92, C72, I19

1. Introduction

Prosocial behaviors help encourage positive interactions and promote economic institutions that require trust/trustworthiness. There are numerous factors that may influence the propensity to exhibit prosocial behaviors. The variable considered in this paper is one that receives little attention in this area of decision-making research: sleepiness. We examine the impact of commonly experienced sleep and circadian states on outcomes in three well-known simple social interaction tasks: the ultimatum, dictator, and trust games. Prosocial behaviors in these games, such as trust, are at least a simple indication of social capital of the decision maker (Putnam, 1993). Some researchers have even found that increases in survey-based measures of country-level trust promote desirable macroeconomic outcomes, such as increased economic growth (Knack and Keefer, 1997) or reduced government corruption (LaPorta *et al.*, 1997). As such, factors that influence the micro-level choice to behave prosocially—a type of individual investment in social capital (Glaeser *et al.*, 2002)—have multiplier effects in society and the economy, and so the importance of this decision domain should not be underestimated.

The limited research that exists on sleep loss and simple social decisions has utilized highly controlled total sleep loss protocols (Anderson and Dickinson, 2010; Ferrara *et al.*, 2015). Such protocols help establish the dose-response of behavior to extreme levels of sleep loss, but the external validity of such findings remains unclear. Observation or field data on sleep levels and choice are highly externally valid, but the limited or lack of experimental control in such data (or the potential bias in self-reports of one's sleep level) reduce one's ability to identify causal effects. In short, we believe that generating primary experimental data using an ecologically valid sleep setting is a valuable approach for studying this particular research question. Here, we utilized an at-home sleep manipulation protocol with objective sleep data acquisition, random treatment assignments, and within-subjects behavioral measures, which increase our ability to claim causal effects. Sleepiness is one of the more concerning health trends at present, and so sleepy decision-making is hardly a rare occurrence. Nevertheless, the weight of the research on sleep and decision making has focused on individual decision tasks

(e.g., Harrison and Horne, 2000; Killgore *et al.*, 2006), which leaves a gap in our understanding of how adverse sleep states affect choices in a critical decision domain.

A main contribution of our work is to experimentally manipulate sleep in ecologically valid ways that are highly applicable to the real world—the levels of sleep restriction we examine are pervasive in modern society and part of everyday life for nearly 30% of U.S. adults (Schoenborn and Adams, 2010). And, while the U.S. Centers for Disease Control and Prevention has labeled sleep deprivation a public health epidemic, little is known about how commonly experienced adverse sleep states impact social interactions. We hypothesize that increased sleepiness will reduce prosocial behaviors given the (limited) related research and given our understanding of deliberative thinking and social decisions (Anderson and Dickinson, 2010; Ferrara *et al.*, 2015; Rilling and Sanfey, 2011; Krajbich *et al.*, 2009; McCabe *et al.*, 2001; Chee and Chuah, 2008). This hypothesis suggests that sleepiness leads to inefficiencies or unrealized benefits in social exchanges.

Results from our 3-week at-home sleep protocol study indicate that both sleep restriction and suboptimal time of day either directly or indirectly (through sleepiness) reduce simple behavioral measures of prosocial decisions. Estimated reductions in dictator giving are the most robust, though our multivariate estimation results show significant decreases for trust and trustworthiness in most specifications as well. Given that these simple games can be considered building blocks for more complex social interactions, such findings have important implications. A significant portion of adults in many countries have habitual sleep levels similar to those we study, and sleep restricted decision-making at suboptimal times of day is common in modern society as well. Our data draw attention to a typically overlooked behavioral “cost” of these modern sleep trends. At least in the area of social interactions, sleepiness may contribute to a type of dead-weight loss of potential benefits that has not been previously highlighted.

1.1 Background

Our behavioral hypothesis stems from the argument that prosocial behavior requires deliberative thinking and active suppression of myopic self-interest (Rilling and Sanfey., 2011;

McCabe *et al.*, 2001; Fehr and Camerer, 2007; Achtziger *et al.*, 2016). Interestingly, recent evidence suggests that reduced deliberation may promote altruism and cooperation in certain contexts (Rand, *et al.*, 2012; Rand *et al.*, 2014; Rand *et al.*, 2016), although other studies found no such relationship (Verkoeijen and Bouwmeester, 2014; Tinghög *et al.*, 2013; Krajbich *et al.*, 2015). It is important to note, however, that these studies typically employ standard cognitive load or time pressure manipulations that are not directly comparable to our manipulation.¹ Another distinct manipulation in the literature that is intended to reduce deliberation is referred to as “ego depletion”, which seems to impact motivational aspects of task performance more than cognition (Inzlicht and Schmeichel, 2012).² Notably, Ainsworth *et al.* (2012) reported reduced trust following ego depletion, and a study by Vohs *et al.* (2010) crossed ego-depletion with sleep deprivation and found that only ego-depletion influenced the expression of the negative social behavior of aggression. Thus, the existing research leads to the natural question of whether we believe our sleep manipulations will affect pro-social behavior by harming cognition or task motivation.

It has been argued that sleep loss may impact motivation as well as cognition, but the mixed evidence includes examples where no significant motivation decline is reported (e.g., Drummond *et al.*, 2005), while others report significant motivational decline following sleep deprivation (e.g., Almklov *et al.*, 2014). However, even when short, novel, or incentivized tasks are used to mitigate motivational decline caused by sleep loss (see Alhola and Polo-Kantola, 2007, and sources therein), performance decrements are still observed. This argues that cognitive effects likely dominate any motivation effects of our treatment manipulation—our tasks are incentivized, short, and relatively novel for the subjects. Another recent study concludes that self-regulation (the target of ego depletion manipulations) and fatigue are not overlapping constructs (Vohs *et al.*, 2010), which supports our view of dominant cognition effects over motivational effects in our design. Thus, compared to commonly used alternative protocols aimed at reducing deliberative processes, ours is unique and focused on cognitive

¹ Distinct from the other studies mentioned, Krajbich *et al.*, 2015, examined patients with prefrontal lesions.

² With an ego depletion manipulation, a subject is made to exert self-control of some sort at time t , and then administration of another task requiring self-control is administered at time $t+1$. Ego depletion is meant to reduce one's capacity to self-regulate in the time $t+1$ task.

effects of mild adverse sleep states. We also argue that our ecologically valid at-home sleep protocol implies that our results have more clear implications for real world decision makers.

For our research question, another relevant stream of literature has identified the importance of deliberative thinking for prosocial decisions (Rilling and Sanfey, 2011; Krajbich *et al.*, 2009; McCabe *et al.*, 2001; Fehr and Camerer, 2007). This is important because the prefrontal cortex (PFC) is particularly vulnerable to sleep deprivation. Some studies report negative sleep impacts on PFC function (Horne, 1993; Muzur *et al.*, 2002; Chee and Chua, 2008), and Horne (1993) reports a more general link between sleep deprivation and reduced PFC activation. This is consistent with recent neural and behavioral studies showing the negative impact of adverse sleep or circadian states on deliberative decisions (Yoo *et al.*, 2007; McElroy and Dickinson, 2010; Dickinson and McElroy, 2010). However, in other contexts researchers have reported increased PFC activation following sleep deprivation. For example, compensatory PFC activation on a verbal learning task is reported in Drummond *et al.* (2000) following total sleep loss. Regarding decision making studies, Venkatraman *et al.* (2011) documented increased ventromedial PFC (vmPFC) activation following sleep deprivation in a risk-taking task. The authors attributed the increase in vmPFC activation following sleep loss to an increased focus on monetary gains. However, rather than improving decision making, increased vmPFC activation appears to suggest an optimism bias triggered by sleep loss (see also Venkatraman *et al.*, 2007; 2009). Our interpretation of this literature is that sleep deprivation, while increasing some PFC activation on certain tasks, has not been found to improve deliberative processes useful for optimal decision making. In fact, others have noted the negative impact of sleep loss on decisions that rely particularly on the vmPFC (Killgore *et al.*, 2012).

Somewhat related to the issue of PFC importance in social decisions, researchers have reported higher altruism and trust levels in older adolescents (Benenson *et al.*, 2007; Fehr *et al.*, 2008; Fehr *et al.*, 2013). Such findings also support the hypothesis that the PFC, which develops later than other brain regions, is important for prosocial choice. As a whole, we find that the weight of the existing evidence suggests PFC vulnerability to adverse sleep or circadian states, and PFC function is important for prosocial choice. While this is a somewhat overly

simplified characterization of the literature, it is useful in organizing our thinking and forming our hypothesis regarding the likely impact of our experiment manipulation in the domain of social choice. Our hypothesis can be succinctly stated as follows:

Hypothesis: Sleepy subjects—whether from sleep restriction or suboptimal time of day—will make less prosocial decisions.

1.2 Experimental Protocol

Our starting point was to first generate a database of young-adults (18-39 year olds) for whom we had specific sleep-related information—some of this was to address inclusion criteria for the main experimental study and some was in order to manage random session-time assignments. We first administered a large-scale online survey that included questions on basic demographics, recent sleep habits, validated anxiety and depressive disorder screener questions, and a validated morningness-eveningness questionnaire. Over multiple waves of the online survey we generated several thousand responses to our survey (mostly student responses).³ Criteria for viable subjects for the main study included: those below standard cutoffs for risk of major depressive or anxiety disorder, no diagnosed sleep disorder or self-reported insomnia, between 18-39 years of age. To assess diurnal preference, the survey administered a validated short form of the morningness-eveningness questionnaire, henceforth rMEQ (Adan and Almiral, 1991). The rMEQ classifies individuals on a continuous scale of 4-25, with morning-types having a rMEQ score from 18-25 and evening-types having a rMEQ score from 4-11. Though based on subject self-reports, the methodology has been validated against physiological data on oral temperatures (Horne and Östberg, 1976) and is a standard tool in circadian research. A validated measure of diurnal preference was necessary for the validity of our random session time assignments.

³ While some have argued that laboratory experiments may overstate social behaviors (Levitt and List, 2007), recent research suggests that the use of student subject pools may, if anything, *underestimate* the importance of social behaviors in the general population (Falk et al, 2013). What is important is to note that the qualitative direction of our results suggest damaging effects of relatively mild adverse sleep states on prosocial behaviors.

We identified morning-types (MT) and evening-types (ET) in the database and randomly assigned each potential subject, *ex ante*, to a morning (7:30 a.m.) or an evening (10:00 p.m.) experiment session prior to sending out recruitment emails. Due to the rarity of true MT subjects—less than 10% in young adult populations are morning-types (see Chelminski *et al*, 2000)—we extended our rMEQ cutoff for MT to include rMEQ scores of 16 and 17. To compensate, we only recruited the more extreme (and still abundant) ET subjects with rMEQ scores from 4-9.⁴ This approach helped ensure that the sample we recruited for the main study was comprised of individuals with true morningness or eveningness preferences.⁵

For the main study, subjects were recruited for a 3-week experiment protocol with multiple in-lab sessions each occurring at the same randomly assigned time-of-day for that subject. The 3-week protocol allowed for a within-subjects manipulation of at-home sleep levels, as discussed in the next paragraph. Another design option would have been to vary the time of day for the lab sessions such that each subject was administered decision tasks both at a more optimal (circadian matched) and less optimal (circadian mismatched, MM) time of day. That is, a within-subjects circadian mismatch protocol was an alternative design choice. However, one of our concerns was subject scheduling and the difficulties in recruiting and preserving participation of subjects with non-constant session times-of-day, and so we chose to proceed with the circadian portion of the protocol as a between-subjects design feature (see Dickinson *et al*, 2016, for a more complete discussion of subject attrition in this design). Other researchers may wish to examine optimal versus sub-optimal times-of-day in decision making as a within-subject design factor, but our design choice was to favor additional statistical power along the at-home sleep restriction dimension.

Subjects were recruited in groups (average size was about 13 subjects) and, while each experimental group was either designated a “morning session” or “evening session” group,

⁴ In this way, our sample was still drawn from the tails of the rMEQ distribution and eliminated the same amount of support from the non-tail portion of the rMEQ distribution compared to if we had used the traditional MT cutoff (rMEQ=18) but included non-extreme ET (rMEQ=10-11) in our sample.

⁵ Other studies have assessed diurnal preferences only *after* recruitment, and then used a median split to classify subjects as morning-types or evening-types (e.g., Bodenhausen, 1990; Kruglanski and Pierro, 2008). Given the distribution of diurnal preferences among young adult subjects (see Chelminski *et al*, 2000), the approach will incorrectly classify many intermediate or indeterminate type subjects as morning-types.

there was a mix of morning-type and evening-type subjects in each group. Figure 1 shows the timeline of the protocol and when the decision experiment sessions took place. As can be seen, each subject was prescribed a well-rested (WR: 8-9 hrs/night in bed) and a sleep-restricted (SR: 5-6 hrs/night in bed) week with an ad-lib sleep week in between. A small number ($n=30$) of control subjects were recruited as well to assess pure repeat administration effects. Control subjects were intermediate diurnal preference individuals, attended experiment sessions at mid-day (neither early nor late), and they were assigned WR sleep weeks in both Week 1 and Week 3 of the protocol.

The set of bargaining tasks was administered at the end of weeks 1 and 3 (i.e., at the end of each sleep treatment week). Because the session time-of-day remains constant for a given subject, this is a mixed design with between-subjects (circadian match/mismatch) and within-subjects (SR/WR) components. Subject sleep levels are objectively monitored during the entire three weeks with actigraphy devices that allow for raw data output (see Appendix for model of devices, which are commonly used in sleep and clinical research and are well-validated). Scoring the sleep data involved complementary sleep diary input as well as the objective actigraphy data, and subjects were paid a separate fixed payment (\$80) for providing the 3 weeks of sleep data. Subjects were deemed compliant if average nightly sleep during the WR week was at least 60 minutes more than during the SR week. The Appendix discusses the legitimacy and data-driven approach to this standard for compliance in detail (see also Dickinson *et al.*, 2016, for more extensive details and analysis of the sleep protocol).

1.3 Decision Tasks

We administered the ultimatum (Güth *et al.*, 1982), Dictator game (Forsythe, *et al.*, 1994), and Trust games (Berg *et al.*, 1995) twice for each subject—once following SR and once following WR. For the Ultimatum and Dictator games we used a \$10 starting sum. In the Ultimatum game, the first-mover proposes a division of the \$10 and the second-mover may either accept or reject the offer. Both players receive a zero payoff if the offer is rejected. The Dictator game removes the second-mover decision from the game, such that the dictator simply decides how the \$10 will be divided between the two players. For the \$10 Trust game

we did not endow the responder with any money, (and so, a subject attempting to equalize first-mover/second-mover earnings will send back a different amount than if both first and second-mover were endowed with \$10). The first mover may choose to pass (trust) some, all, or none of the \$10 to the second-mover. Whatever is trusted to the second mover is then tripled by the experimenter, and the second-mover can then choose to pass back some, all, or none of the tripled amount. The amount passed back is generally viewed as a measure of trustworthiness. We also administered a “risk” version of the Trust game where the second-mover is a pass-back algorithm rather than another subject in the group. In this way, we can identify the importance of the social interaction, as opposed to the pure risk of passing money, in making the interpersonal decision (see Sanfey *et al.*, 2003; Kosfeld *et al.*, 2005).

The games were administered via the strategy method whereby all subjects made decisions as first-mover and second-mover prior to knowing to which role he/she was randomly assigned. This procedure requires subjects to make decisions as a second-mover in the Ultimatum and Trust games for all possible contingencies of what the first mover might do. It was common knowledge that we would make random role assignments (first- or second-mover), anonymous counterpart assignments, and randomly select one of games for payoff for that decision session only after all decisions were made for all tasks. Outcomes for the randomly selected payoff game were only revealed at the end of the decision session, when payoffs for these and other decision tasks were given out in cash.⁶ During the second decision session (Session 3 in Figure 1) subjects were informed that the roles, counterparts, and the payoff game were re-randomized (i.e., not the same as in the first decision session).

In total, we recruited 184 young adults (30 control: 154 treatment: 18-39 years old, mean 21.66 ± 4.43 years old) to participate in the 3-week study. The relevance of our results to real world decision makers are, in large part, due to an at-home sleep and circadian protocol

⁶ Note that the fixed payment for participation was separate from the cash incentive payments for outcomes in the decision experiments. Subjects could receive the \$80 fixed payment by check or Amazon gift code, which was sent out several days after the end of the experiment so that sleep data could be first checked for instances of apparent gross noncompliance. The compliance standard for issuing experiment payment was more liberal than the standard we used for considering the sleep data compliant with the treatment condition. In general, to maintain satisfaction in the subject pool, our position was to fully pay subjects for what appeared a good faith effort at compliance, even if data were ultimately considered noncompliant for analysis purposes. This more liberal payment policy was not known to subjects during their participation.

with high ecological validity. Table 1 shows the mix of circadian matched versus mismatched subjects for the between-subjects component of the design. Of these 184 subjects, 149 (30 control: 119 treatment) were deemed compliant based on objective actigraphy data.⁷ Compliant treatment subjects slept a minimum of 1 hour more per night during the SR week than the WR week, but there was variation in the level of compliance (e.g., the average difference between WR and SR nightly sleep was about 1.5 hours). The results below are based on the subset of compliant treatment subjects (n=119).

2. Results

Subjects self-reported sleepiness during the decision sessions using the Karolinska Sleepiness scale (Åkerstedt and Billberg, 1990; Kaida *et al.*, 2006). To test treatment manipulations we regress self-report sleepiness on treatment, demographics, and sleep control variables. Both SR and circadian mismatch significantly increased sleepiness ($p < .01$), as noted below,⁸ which is evidence of the validity of the protocol at manipulating sleepiness. In Table 2 we present descriptive statistics of the behavioral outcomes from each task, along with nonparametric tests of means. Nonparametric tests of the SR treatment effect uses the signed-rank test for matched data given each subject makes decisions under both SR=0 and SR=1 conditions. For testing means of behavioral outcomes resulting from circadian mismatch (MM), we used the Mann-Whitney test for unmatched data after first averaging each subject's decisions for a task across the two decision sessions. Given our experimental design choice, the test for MM effects is inherently less statistically powerful than our tests of SR effects on behavioral outcomes. Results in Table 2 indicate marginally significant SR effects on Dictator offers and trust decisions. Of course, such tests do not take into account other covariates, and so a more controlled statistical analysis follows.

⁷ A small number of treatment-subject observations were lost due to malfunction or corruption of actigraphy devices (9 subjects). The final sample of 119 compliant subjects and 30 control subjects all had complete actigraphy data records.

⁸ The full results of this estimation are shown in Table 6, which is the first stage regression of the two-stage endogenous variables regression used to evaluate sleepiness as a mediating variable in evaluating behavioral outcomes.

We next present results from the multivariate analysis. The general model structure we estimate is: $Y_t = a + \mathbf{X}\beta + \mathbf{Z}\gamma$ where Y_t is the behavioral outcome, Y , for task, t . \mathbf{X} represents a vector of sleep-related variables, which includes the experimentally manipulated measures of sleep restriction and circadian mismatch and a measure of chronic daytime sleepiness, *Epworth* sleepiness, which is derived from the pre-screen sleep survey. Note that *Epworth* sleepiness is a separate construct from state-level *Karolinska* self-reported sleepiness elicited during the decision sessions (simple correlation $\rho = .12$ between these two measures). \mathbf{Z} includes indicators for the session number (*Session 3* dummy indicates the second administration of the task) and timing (*Morning Session = 1*), as well as demographic controls such as age, gender, and sub-clinical depression and anxiety risk scores.⁹ We estimated the models using a random effects error structure for multiple observations per subjects.

Estimation results in Tables 3-6 identify predictors of the key outcome variables for each social decision task. The relevant treatment variables for sleep restriction and circadian mismatch are highlighted. The treatment variable, *Mismatch*, is an indicator variable equal to 1 if the subject is mismatched relative to her more preferred time of day. Because there exists variation in the level of compliance among subjects deemed compliant, a continuous measure of sleep level is of interest. For each decision outcome, two models are estimated to allow for different approaches to scoring sleep levels: The first model uses average nightly actigraphy-measured sleep during the week prior to the decision, *Nightly Sleep*, as the continuous variable measure for sleep levels. The second model uses a constructed hybrid measure, *Personal Sleep Deprivation*, which is the difference between a subject's self-reported optimal nightly sleep (from the earlier online sleep survey) and the actigraphy-measured nightly sleep the week prior to the decision. In a sense, the variable *Personal SD*, handicaps each individual for his/her perceived personal sleep need. Additional estimations are included in the Appendix where sleep restriction is coded as a dichotomous indicator variable ($SR=0,1$) to identify the SR treatment week. Regarding the non-treatment control variables, the *Session 3* indicator is robustly significant across estimations of the different behavioral outcome measures. The sign

⁹ Recall that subjects at clinical risk levels for depressive or anxiety disorder were not recruited for the study.

of the coefficient on *Session 3* captures a general tendency to behave in a more self-interested fashion during the last session.¹⁰

The estimated coefficients on the circadian mismatch and sleep level variables allow for a test of our behavioral hypothesis for each model. The estimated direct effect of circadian mismatch on behavioral outcomes is insignificant across all models (Tables 3-6), and so we turn our attention to the statistically significant ($p \leq .05$) estimated direct effect of sleep restriction on behavioral outcomes—these are an indication that sleep restriction may have a greater impact on behavior than circadian mismatch, but we should also remind the reader that the circadian mismatch dimension of the protocol has less statistical power than the sleep restriction dimension. Regarding sleep restriction, we find significant and robust effects on decisions in the Dictator and Trust games. Results from the Ultimatum game are less convincing, which should not be surprising. The Ultimatum game is standard in this suite of tasks, but it confounds self-interest with risk of a zero payoff in both first- and second-mover roles (or, the desire to accumulate money is in conflict with the desire for fairness, as noted in Sanfey *et al.*, 2003). We find weak evidence that ultimatum offers may be less generous for higher levels of *Personal SD* (i.e., the more sleep deprived. See Table 3, Model 3), but this effect is not robust across alternative coding of the sleep restriction variable (see Table 2 and Appendix).

Turning to the Dictator game, Table 4 identifies a robust result of reduced Dictator offers when one is sleep restricted (see also Appendix), which is consistent with our hypothesis. The predicted relationship between Dictator offers and *Personal SD* is shown graphically in Figure 2—demographics, session identifier, and sleep related control variables are held constant. Given the lack of a rejection threat in the Dictator game, offers in this game are more easily interpreted as a measure of altruism or prosocial behavior than are decisions in the

¹⁰ The same is generally true of results from the Control subjects, although there are only 30 such subjects. The only generally significant variable is the *Session 3*, variable which, when statistically significant, indicates and increased greed in choice. Unlike the data from the Treatment subjects, however, Control subjects are not significantly less trustworthy during the last session than the first decision session. Because of the small sample of Control Subjects, and the fact that each Treatment subject serves as his/her own baseline regarding the sleep restriction effect, we do not report Control Subject estimations here. They are available on request.

Ultimatum game results.¹¹ The size of the estimated effect is also non-trivial. Compared to a fairly typical \$3 offer in a Dictator game when not sleep-restricted (*Personal SD*=0), that same individual being 2 hours sleep-restricted per night (120 minutes) chronically sleep restricted would offer \$2.40. This represents a 20% reduction in Dictator giving level.¹²

Tables 5 and 6 show additional evidence of decreased prosocial behavior in the Trust game when sleep restricted. Table 5 also includes results from a “risk” version of the Trust game where it is common knowledge for subjects that the second-mover is an automated pass-back algorithm.¹³ As can be seen in Table 5, the significant reduction in trust due to sleep restriction occurs only when trust is imbedded in a social interaction (albeit a one-shot and anonymous interaction in this task). The reduction in trust is also significant because it documents how commonplace levels of sleep restriction introduce an inefficiency into the domain of social interactions. The predicted Trust game result is depicted in Figure 3, and the magnitude of reduced initial trust amount indicates a lost potential in the 2-person exchange that is economically significant. The size of the predicted reduction in trust is similar to the reduction in the Dictator game (\$.50-\$.60), but this represents a slightly smaller effect in percentage terms relative to typical trust levels, which are around \$5-\$6 out of a \$10 pie (see Berg *et al.*, 1995).

Trustworthiness results are shown in Table 6 and Figure 4. Here, given the strategy-method nature of choice elicitation, the dependent variable is the average percentage passed back by the second-mover over all possible initially trusted amounts. We also include an

¹¹ Of course, the Dictator game is not itself without confounds in interpretation as Dictator giving may reflect concerns that the experimenter knows of one’s decision, or may reflect aversion to payoff inequality (see Fehr and Schmidt, 1999). Nevertheless, such factors are present in both the SR and WR decisions of subject, and any amount of giving intended to reduce inequality aversion can be considered a different form of prosocial behavior.

¹² The magnitude of the estimated effect is only slightly smaller—about a 16%-17% reduction—if using *Nightly Sleep* (model 1, Table 4) or a dichotomous scoring of SR (see Appendix, Table S2). In general, the magnitude as well as the significance of each estimated effect is similar, if not greater, using these alternative scorings of sleep restriction, and so we choose to focus on the measure *Personal SD* throughout the main text for displaying the results relative to an easily-understood benchmark of *Personal SD* = 0 (where someone is getting exactly the amount of sleep she thinks she needs).

¹³ That is, subjects were informed prior to making decisions that the risk associated with the first mover decision in this game was *not* a function of another individual’s choice in the current experiment group, but rather the amount “passed back” was a draw from a distribution that reflected typical pass back amounts from previous experiments. Previous research with social decision tasks has highlighted that behavior with a real human counterpart may differ from behavior when matched with a computerized or algorithmic counterpart (e.g., Kosfeld *et al.*, 2005; Krajbich *et al.*, 2009).

additional model in Table 6 (far-right column) where we average the percentage pass-back decisions over the subset of amounts where at least half the monetary pie is initially trusted. This evaluates whether sleep restriction impacts trustworthiness differently when the initial signal of trust is most clear.¹⁴ As can be seen in Table 6, sleep restriction significantly decreases trustworthiness, and the effect is also robust. Moreover, the effect is largest in magnitude and estimated with the most precision when evaluating trustworthiness in the face of strong initial signals of trust. In other words, sleep restriction harms trustworthiness more when the truster is most vulnerable, which should be concerning. Figure 4 shows that the effect size of the result in Table 6, column 2, is somewhat smaller than the Dictator or Trust effects of sleep restriction. Nevertheless, it is a robust result that indicates an approximate 7% reduction in trustworthiness over a baseline rate of 33% trustworthiness in response to an additional 2 hours of chronic sleep restriction.¹⁵

Given the Trust game results, one might argue that the SR effect of reduced trust is due to an accurate anticipation of reduced trustworthiness. Our data cannot directly test this, but we reject this interpretation for two reasons: First, the argument is not consistent with the Ultimatum results—there is at least some marginal evidence that SR increases first-mover greed (model 2 in Table 3; Table 7 discussed below), and yet there is no predicted change in MAOs. Second, this interpretation requires anticipation skills that have neural correlates in brain regions harmed by sleep loss (Coricelli and Nagel, 2009). Dickinson and McElroy (2012) also showed direct behavioral evidence that decisions requiring such anticipatory skills suffer from adverse sleep states.¹⁶

2.1 Sleep and Circadian mismatch interaction analysis

¹⁴ In this case, the strength of the initial trust signal is also significant because more than half the pie initially trusted implies the first-mover is exposing herself to being exploited by the trustee.

¹⁵ Here, the baseline trustworthiness of 33% is the approximate level of predicted trustworthiness for somewhat who is not sleep restricted (*Personal SD* = 0), and it also represents the Rate-of-Return breakeven level of trustworthiness for first-movers to recuperate their initial “investment” given the tripled-investment rule.

¹⁶ Results in Tables 3-6, as well as Appendix Table S2, are also robust to the exclusion of extreme sleeper subjects who were personally sleep deprived > 2.5 hours (SR condition) or < ½ hour (WR condition). Standard errors increase slightly, though key results are still statistically significant at $p < .10$ or better. The magnitude of the estimated effects from removing the more extreme sleepers, not surprisingly, is slightly reduced.

Van Dongen and Dinges (2003) and Burke *et al* (2015) suggest that sleep and circadian misalignment may have interactive effects, and other behavioral research has suggested that adverse sleep states may have cumulative effects (Dickinson and McElroy, 2010). We explore the possibility of interaction effects in Table 7. Here, we show results of estimations that include an interaction variable between circadian mismatch ($MM=1$) and the *Personal SD* variable used previously in the Tables 3-6 specifications. Consistent with these earlier estimations, significant adverse sleep state effects (on Dictator offers, trust, and trustworthiness decisions) are linked to *Personal SD* and not circadian mismatch. One difference with earlier estimations is that we find subjects with higher *Personal SD* pass less money in the trust game even when the second mover is a pass-back algorithm. However, this finding should not be considered robust given its absence from other specifications we estimated. Perhaps surprisingly, Table 7 results show no significant interaction effect on *Personal SD*MM*. Thus, our data do not show evidence of a magnified behavioral effect in subjects who are both sleep restricted and circadian mismatched.¹⁷

Relatedly, Table 8 shows the mean values and confidence intervals of self-reported sleepiness by treatment. Table 8 shows that, while both SR and MM significantly increase sleepiness, it is also clear that the magnitude of the SR effect on sleepiness is larger. The insignificant behavioral effects of MM we report may therefore be attributed to the fact that MM does not increase sleepiness as much as SR in our design, which may reflect the more cumulative effect of nightly chronic partial sleep restriction over the course of an entire week. Additionally, one can see from Table 8 that self-reported sleepiness for someone in both SR and MM states is not significantly higher than for someone only in the SR state. While it is unclear why self-reported sleepiness is not higher when one is both SR and MM, the lack of interaction effects in the Table 7 analysis is sensible in light of this finding.¹⁸

¹⁷ Full marginal effects of a change in MM and Personal SD (PSD) on each treatment variable are given, in general, by $\beta_{MM} + \beta_{(PSD*MM)} * PSD$ and $\beta_{PSD} + \beta_{(PSD*MM)} * MM$, respectively (evaluated at the mean levels of the treatment variables). However, the coefficients on the interaction term, $\beta_{(PSD*MM)}$, is statistically insignificant in every instance. Our focus is therefore on the main effect of each treatment variable on our behavioral outcomes measures.

¹⁸ It is possible that subjects in a condition of both SR and MM took countermeasures to counteract sleepiness (other than naps, which would contribute to noncompliance). However, the one item on which we collected countermeasure data during the decision session—whether caffeine or sugar was consumed in the 3 hours prior to

2.2 Sleepiness as a mediating variable

Because our manipulations were intended to increase sleepiness, we also evaluate whether sleepiness is the primary mediator of these results (Tables 9 and 10). For the two-step estimation we first regress the Karolinska sleepiness scores (KSS) on demographics, session, and sleep variables (Table 9). As noted earlier, both SR and circadian mismatch significantly predict higher KSS ($p < .01$), which is seen in the first stage estimation results. This provides validation for our treatment manipulations and identifies the variation in KSS due to subject-specific factors as well.¹⁹

Step two used the predicted KSS values as a covariate (instrument) in the main decision outcome estimations. Table 10 estimates indicate that higher levels of the KSS-instrument predict lower levels of ultimatum proposals, dictator altruism, trust, and trustworthiness. These estimates indicate that our experimental treatments alter behavioral outcomes through their impact on sleepiness. They also highlight that circadian mismatch, while not estimated to directly impact behavioral outcomes, is estimated to *indirectly* affect social behavior via its impact on sleepiness. In conjunction with our other analysis represented in Tables 3-6, these results describe a common theme whereby prosocial decisions decline due to sleepiness that is symptomatic of an adverse sleep or circadian state.

3. Discussion

We examined the impact of mild but chronic sleep restriction and suboptimal circadian timing on decisions in simple social interactions. The sleep levels studied and the times-of-day utilized are highly relevant to what a large segment of the population experiences in everyday life. Because we utilized an ecologically valid setting, and compensatory strategies to combat

the lab session—did not show significant differences across individual or combined sleep conditions (see Appendix Table S1). These data are, however, not precise in terms of standardized size and caffeine/sugar content—we used simple count data on self-reported caffeine/sugar consumption prior to the session as a 0/1 variable).

¹⁹ We also note that the impact of gender is implicit in these results. First-stage estimates show that female subjects are significantly sleepier, controlling for other demographics and sleep variables.

the sleepiness were not prohibited, our results can be viewed as conservative estimates of the impact of common sleep states in the simple games we examined.

The simple social decision tasks we used are well-known and they form the building blocks of more complex interactive environments where social behaviors loom large. We found robust results consistent with the hypothesis that commonly experienced adverse sleep states reduce prosocial behaviors. Our results showing reduced dictator giving are the most precisely estimated, although trust and trustworthiness are also found to decrease significantly in most specifications. We hypothesized that adverse sleep states would likely reduce prosocial behaviors due to their tendency to reduce deliberative thought. While we do not have direct evidence that our sleep and circadian manipulations impacted prefrontal activation, a body of literature in sleep science clearly links more extreme adverse sleep states (e.g., total sleep deprivation) to disproportionate reductions in PFC activation, as well as altered emotional response region activation. For example, sleepy subjects may experience increased fear of betrayal as a mechanism that results in reduced trust, as was proposed in an earlier study (Anderson and Dickinson, 2010). While suggestive, we also note that our sleep restriction manipulation produced the same general behavioral effects as others have found in patients with vmPFC damage (Krajbich *et al.*, 2009). Future research may wish to document the impact of these more mild sleep/circadian manipulations on neural activation patterns in order to establish direct evidence for the mechanism that drives this behavioral result.

Our findings imply that commonly experienced adverse sleep states reduce prosocial behaviors and lead to unrealized gains in simple social interactions. A reduction in an individual's social capital has multiplier effects, and so any factor that can be shown to reduce prosocial behavior is noteworthy. One of the main criticisms of recent work identifying the decline in social capital in America (Putman, 1995) is the fact that many of the arguments are based on correlational data (see critique in Sobel, 2002). While we do not claim to have uncovered the hidden source of decreased social capital during recent decades, we do stand on firmer ground in claiming at least one micro-level causal determinant of reduced prosocial behavior in these simple environments.

3.1 Conclusions

Given the prevalence of social interactions in everyday life, it should be concerning to identify that sleep restriction adversely impacts prosocial behavior. This research helps show that a common trend in current adult sleep habits may not just affect decision outcomes, but it also may undermine the potential to generate value in interpersonal interactions and possibly reduce social capital multiplier effects. Such social “inefficiency” costs of sleepiness are not fully appreciated and largely ignored. Future research should seek to evaluate whether these effects are also robust to repeated, longer-term, or non-anonymous interactions. In the meantime, it seems like a piece of timely advice for encouraging prosocial behavior would be to tell everyone to “just sleep on it”.

Acknowledgements: This work was supported by National Science Foundation grant BCS-1229067. We thank Sean P.A. Drummond for assistance designing the sleep protocol, T. Perry, A. Beckett, L. Morris, R. McClelland, and E. Wilson for help with sessions, and N. Stroh and S. Rule for assistance with decision tasks and sleep data preparation and scoring. Comments from seminar participants at numerous universities is gratefully acknowledged. The authors also thank Mike McKee for helpful comments. Data are available from Dickinson on request.

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Table 1
Sample Size Per Design Cell (treatment subjects)

	Morning Session	Evening Session
Morning-type	34 (30)	38 (28)
Evening-type	39 (30)	38 (31)
Sample size = 149 subjects Matched obs = 76, Mismatched obs = 73 (compliant & sleep data intact shown in parenthesis)		

Notes: Circadian mismatches cells shaded
Table reproduced from Dickinson *et al* (2016)

Table 2: Behavioral Summary Statistics and nonparametric treatment effect tests
(sample is n=119 compliant treatment subjects. Data pooled across sessions and subjects)

Variable	Ult \$\$ Offer	Ult \$\$ MAO	Dictator \$\$ Offer	Trust person \$\$ Offer	Trust Distribution \$\$ Offer	Trust-worthiness % passback
SR=0	4.49 (1.04)	2.87 (1.77)	3.44 (2.00)	5.19 (2.47)	4.62 (2.58)	.274 (.147)
SR=1	4.42 (1.01)	2.89 (1.82)	3.02 (2.24)	4.77 (2.27)	4.56 (2.49)	.261 (.146)
Signed-Rank (Matched)	Z= -0.57 p=.57	Z= -0.14 p=.88	Z= -1.80 p=.07	Z= -1.65 p=.10	Z= -0.09 p=.93	Z= -0.59 p=.56
MM=0	4.42 (1.04)	2.77 (1.75)	3.33 (2.01)	5.04 (2.34)	4.43 (2.42)	.268 (.137)
MM=1	4.49 (1.02)	3.00 (1.84)	3.13 (2.25)	4.92 (2.43)	4.86 (2.63)	.267 (.156)
Mann-Whitney (Unmatched)	Z= -0.08 p=.93	Z= -0.69 p=.49	Z= 0.69 p=.49	Z= 0.93 p=.35	Z= -1.14 p=.25	Z= 0.24 p=.80

Note: Significance levels are for 2-tailed tests. Test on SR treatment effect use matched data, whereas MM effect tests use unmatched data on each subject's *average* behavioral effect across both decision sessions. Alternative nonparametric tests (Sign test of SR effect; Median test of MM effect) produce similar results. For those SR effects on Dictator Offers and Trust decision, which had $p \leq .10$ for the 2-tailed signed-rank test, the corresponding p-values of the Sign (binomial) test are $p=.13$, and $p=.05$, respectively.

Table 3: Ultimatum Decisions
 Random effects GLS regression (for full sample)
 Dependent Variable= *Portion of \$10 pie offered to 2nd-mover*

Variable	<i>Ultimatum Offers</i> (n=234 obs)		<i>Minimum Acceptable Offer</i> (n=236 obs)	
	(1) Coef (st. error)	(2) Coef (st. error)	(3) Coef (st. error)	(4) Coef (st. error)
Constant	4.70 (.60)***	5.29 (.54)***	4.71 (1.01)***	4.70 (.94)***
Female (=1)	-.29 (.18)	-.27 (.18)	-.61 (.31)**	-.62 (.31)**
Age	-.02 (.02)	-.02 (.02)	-.08 (.04)**	-.08 (.04)**
Depression score	.10 (.12)	.11 (.12)	.02 (.21)	.02 (.21)
Anxiety score	-.02 (.04)	-.02 (.04)	.06 (.07)	.06 (.07)
Epworth score	-.01 (.03)	-.01 (.03)	-.03 (.04)	-.03 (.04)
Session #3	-.18 (.09)**	-.18 (.09)**	-.59 (.13)***	-.59 (.13)***
Morning Session (=1)	.10 (.17)	.10 (.17)	.47 (.30)	.47 (.30)
Morningness Score	.01 (.02)	.01 (.02)	.02 (.03)	.02 (.03)
Circadian Mismatched (=1)	.02 (.17)	-.004 (.17)	.11 (.30)	.11 (.30)
Nightly Sleep (minutes)	.001 (.001)	---	-.00003 (.001)	---
Personal SD (min/night)	---	-.0014 (.0007)**	---	.00001 (.001)
Wald chi-squared test (10)	9.43	12.32	31.68***	31.75***

Notes: *, **, *** indicate significance at the .10, .05, and .01 levels, respectively, for the 2-tailed test

Table 4: Dictator Decisions

Random effects GLS regression (for full sample)

Dependent Variable= *Portion of \$10 pie offered to 2nd-mover*

Dictator Offers		
(n=237 obs)		
	(1)	(2)
Variable	Coef (st. error)	Coef (st. error)
Constant	1.39 (1.22)***	3.96 (1.05)***
Female (=1)	-.03 (.36)	.06 (.35)
Age	-.01 (.04)	-.02 (.04)
Depression score	-.23 (.23)	-.18 (.23)
Anxiety score	.11 (.08)	.10 (.08)
Epworth score	.06 (.05)	.06 (.05)
Session #3	-.71 (.20)***	-.72 (.20)***
Morning Session (=1)	.29 (.34)	.31 (.33)
Morningness Score	-.01 (.03)	-.02 (.03)
Circadian Mismatched (=1)	-.22 (.34)	-.32 (.33)
Nightly Sleep (minutes)	.004 (.002)**	---
Personal SD (min/night)	---	-.005 (.002)***
Wald chi-squared test (10)	23.02***	29.36***

Notes: *, **, *** indicate significance at the .10, .05, and .01 levels, respectively, for the 2-tailed test

Table 5: Trust Decisions

Random effects GLS regression (for full sample)

Dependent Variable= *Portion of \$10 pie trusted to 2nd-mover*

Variable	Trust person (Social)		Trust algorithm (Asocial)	
	(n=230 obs)		(n=236 obs)	
	(1)	(2)	(3)	(4)
Variable	Coef (st. error)	Coef (st. error)	Coef (st. error)	Coef (st. error)
Constant	3.91 (1.39)***	6.51 (1.18)***	4.48 (1.48)***	5.40 (1.28)***
Female (=1)	-.33 (.40)	-.24 (.39)	-.47 (.43)	-.45 (.42)
Age	-.01 (.04)	-.01 (.04)	.01 (.05)	.01 (.05)
Depression score	-.27 (.26)	-.23 (.26)	-.39 (.28)	-.36 (.28)
Anxiety score	.10 (.09)	.09 (.09)	.10 (.09)	.09 (.09)
Epworth score	.01 (.06)	.01 (.06)	-.03 (.06)	-.03 (.06)
Session #3	-.64 (.25)***	-.63 (.25)**	-.02 (.25)	-.04 (.25)
Morning Session (=1)	.26 (.38)	.27 (.38)	-.32 (.41)	-.31 (.40)
Morningness Score	-.06 (.04)	-.06 (.04)	-.02 (.04)	-.03 (.04)
Circadian Mismatched (=1)	-.13 (.37)	-.21 (.37)	.52 (.40)	.47 (.40)
Nightly Sleep (minutes)	.005 (.002)**	---	.001 (.002)	---
Personal SD (min/night)	---	-.004 (.002)**	---	-.003 (.002)
Wald chi-squared test (10)	15.44	16.07*	6.91	8.58

Notes: *, **, *** indicate significance at the .10, .05, and .01 levels, respectively, for the 2-tailed test

Table 6: Trustworthiness Decisions
 Random effects GLS regression (for full sample)
 Dependent Variable= *average % returned (from strategy choice set)*

(n=234 obs)	DV = Avg Trustworthiness over all amounts trusted	DV = Avg Trustworthiness over all amounts trusted	DV = Avg Trustworthiness for amounts ≥ \$5 trusted
	(1)	(2)	(3)
Variable	Coef (st. error)	Coef (st. error)	Coef (st. error)
Constant	.19 (.09)**	.31 (.08)***	.41 (.08)***
Female (=1)	-.01 (.03)	-.003 (.03)	-.02 (.03)
Age	.001 (.003)	.0001 (.003)	-.001 (.003)
Depression score	-.02 (.02)	-.02 (.02)	-.02 (.02)
Anxiety score	.005 (.006)	.005 (.006)	.008 (.006)
Epworth score	.001 (.004)	.001 (.004)	.001 (.004)
Session #3	-.04 (.01)***	-.04 (.01)***	-.03 (.01)***
Morning Session (=1)	.001 (.03)	.002 (.02)	.01 (.03)
Morningness Score	-.001 (.002)	-.002 (.002)	-.002 (.003)
Circadian Mismatched (=1)	.0004 (.02)	-.004 (.02)	-.01 (.03)
Nightly Sleep (minutes)	.0002 (.0001)*	---	---
Personal SD (min/night)	---	-.0002 (.0001)**	-.0003 (.0001)***
Wald chi-squared test (10)	12.84	15.60	18.24**

Notes: *, **, *** indicate significance at the .10, .05, and .01 levels, respectively, for the 2-tailed test
 Model 3 shows that Personal SD more strongly impacts trustworthiness when a strong initial trust signal is received.

Table 7: Analysis of Sleep Restriction and Circadian Mismatch Interaction Effects

Random effects GLS regressions						
Standard errors clustered on subject in parenthesis (2 observations per subject)						
Variable	Ult \$\$ Offer	Ult \$\$ MAO	Dictator \$\$ Offer	Trust person \$\$ Offer	Trust Distribution \$\$ Offer	Trust-worthiness [^] % passback
Constant	5.26 (.54)***	4.65 (.95)***	4.03 (1.06)***	6.54 (1.20)***	5.70 (1.30)***	.31 (.08)***
Female (=1)	-.27 (.18)	-.62 (.32)**	.06 (.34)	-.24 (.39)	-.43 (.42)	-.003 (.03)
Age	-.02 (.02)	-.08 (.04)**	-.02 (.04)	-.01 (.04)	.01 (.05)	.0002 (.003)
Depression score	.11 (.12)	.02 (.21)	-.18 (.23)	-.23 (.26)	-.35 (.28)	-.02 (.02)
Anxiety score	-.02 (.03)	.06 (.07)	.10 (.08)	.09 (.09)	.09 (.09)	.005 (.006)
Epworth score	-.01 (.03)	-.03 (.04)	.06 (.05)	.01 (.06)	-.03 (.06)	.001 (.004)
Session #3	-.19 (.09)**	-.59 (.13)***	-.72 (.20)***	-.63 (.25)**	-.03 (.25)	-.04 (.01)***
Morning Session (=1)	.11 (.18)	.48 (.30)	.30 (.33)	.27 (.38)	-.39 (.41)	.003 (.02)
Morningness Score	.01 (.02)	.02 (.03)	-.02 (.03)	-.06 (.04)	-.03 (.04)	-.002 (.002)
MM (=1)	.06 (.22)	.19 (.37)	-.43 (.45)	-.27 (.53)	-.13 (.55)	.0001 (.03)
Personal SD	-.001 (.001)	.0004 (.001)	-.006 (.002)***	-.005 (.003)*	-.005 (.003)**	-.0002 (.0001)*
Personal SD*MM	-.001 (.001)	-.001 (.002)	.001 (.003)	.001 (.004)	.006 (.004)	-.00004 (.0002)
N	234	236	237	230	236	234
Wald χ^2 (11)	12.50	31.75***	29.35***	16.03	10.96	15.58

Notes: *, **, *** indicate significance at the .10, .05, and .01 levels, respectively, for the 2-tailed test.

[^] Estimation of the Trustworthiness model using the alternative dependent variable of average returned amount when 50% or more of pie is trusted shows that the *Personal SD* reduced trustworthiness when high levels of trust are at stake ($\beta = -.0003$, $p < .05$), and there is no significant effect of MM or the *Personal SD*MM* interaction.

Table 8: Karolinska Sleepiness Score (KSS) by Condition

Treatment Combination	KSS Mean	Robust St Errors (clustered on subj)	[95% CI]
SR=0 & MM=0	3.934	.173	[3.591, 4.278]
SR=1 & MM=0	6.621	.193	[6.234, 7.002]
SR=0 & MM=1	5.018	.243	[4.535, 5.500]
SR=1 & MM=1	6.473	.218	[6.041, 6.906]

Note: self-reported sleepiness is based on average KSS from each decision session (measures taken at beginning and end of the session).

Table 9: Instrumental Variables Estimation to examine sleepiness mediating effects. First-stage estimation of determinants of self-reported sleepiness.

1st-Stage Regression (errors clustered on subject)	
<i>KSleepy</i> estimated using following instruments: Demographic/Session variables: <i>Session#3</i> , <i>Female</i> , <i>Age</i> , <i>Anxiety Score</i> , <i>Depression Score</i> , <i>Morning Session</i> , <i>Epworth Score</i> , <i>rMEQ score</i> , <i>Personal SD</i> , <i>Mismatch</i>	
Predictors of <i>KSleepy</i> : Coefficient (st error)	
Demographic/Session Indicators	Sleep Related Variables
<i>Session#3</i> : -.365 (.225)	<i>Epworth Score</i> : .073 (.035)**
<i>Female</i> : .877 (.245)***	<i>rMEQ score</i> : .008 (.022)
<i>Age</i> : .048 (.021)**	<i>Personal SD</i> : .010 (.001)***
<i>Anxiety Score</i> : -.040 (.047)	<i>Mismatch</i> : .865 (.212)***
<i>Depression Score</i> : -.282 (.155)*	
<i>Morning Session</i> : -.649 (.208)***	
Model F – test (10 , 226) = 10.86***	

Notes: *, **, *** indicate significance at .10, .05, and .01 levels, respectively.

Experimental treatment variables *Personal SD* and *Mismatch* both are estimated to significantly increase self-reported sleepiness ($p < .01$). Minor differences in number of observations across tasks (e.g., small number of blank responses) are noted in 2nd-stage regression N below. This implies minor differences in estimated coefficients and standard errors for the 1st-stage regression equations. Shown above are the 1st-stage results of the Dictator game model, which are similar in sign and significance to all other 1-stage equations.

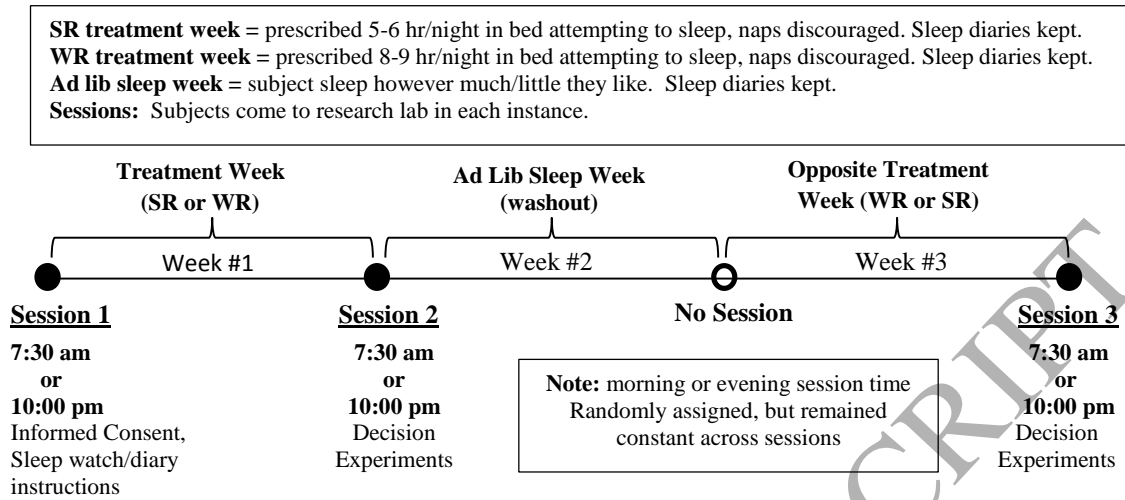
Table 10: Instrumental Variables Estimations to examine sleepiness mediating effects. Second-stage estimations of behavioral outcomes using instrumented values of self-reported sleepiness.

2nd-Stage Regression: <i>KSleepy</i> instrumented from 1 st -stage regression						
Standard errors clustered on subject (2 observations per subject)						
Variable	Ultimatum \$\$ Offer	Ultimatum \$\$ MAO	Dictator \$\$ Offer	Trust person \$\$ Offer	Trust algorithm \$\$ Offer	Trust-worthiness % passback
<i>KSleepy(instr)</i>	-0.17 (.08)**	-0.04 (.13)	-0.46 (.16)***	-0.35 (.16)**	-0.17 (.17)	-0.02 (.01)**
Session #3	-0.25 (.10)**	-0.60 (.14)***	-0.88 (.22)***	-0.79 (.27)***	-0.10 (.25)	-0.04 (.02)***
Female	-0.13 (.15)	-0.59 (.31)*	.42 (.37)	-0.002 (.04)	-0.41 (.45)	.01 (.03)
age	-0.01 (.02)	-0.06 (.04)	-0.02 (.04)	-0.02 (.04)	-0.005 (.04)	.0001 (.002)
Depression score	.06 (.09)	.002 (.19)	-0.30 (.22)	-0.27 (.27)	-0.34 (.28)	-0.02 (.02)
Anxiety score	-0.03 (.03)	.05 (.07)	.12 (.07)	.10 (.07)	.09 (.07)	.005 (.005)
Constant	5.91 (.71)***	4.83 (1.19)***	5.93 (1.31)***	7.44 (1.22)**	5.75 (1.27)***	.38 (.09)***
N	234	236	237	230	236	234
Wald χ^2	10.42	30.97***	25.45***	12.19*	5.89	11.91*

Notes: *, **, *** indicate significance at the .10, .05, and .01 levels, respectively, for the 2-tailed test.

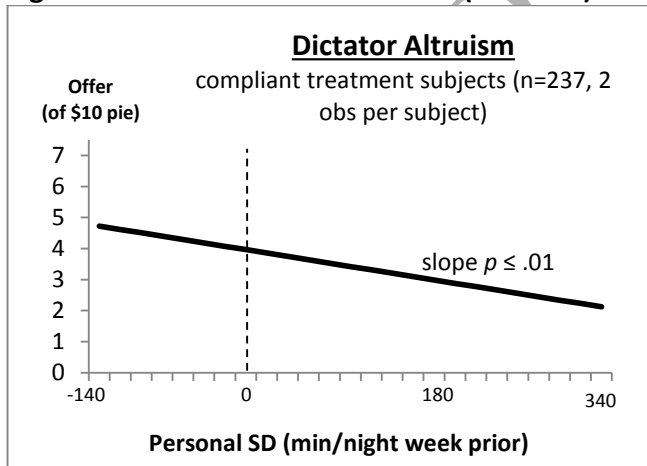
The same 2SLS model run using the alternative Trustworthiness variable (i.e., average percentage passed back on all possible first-mover trusted amounts of at least half the pie), shows similar results to the Trustworthiness model above, except that the magnitude of the instrumented *KSleepy* variable is slightly larger in magnitude (-0.023 vs -.019) and estimated a bit more precisely ($p = .03$ vs $p = .038$). This is consistent with what we report in the text that the impact of sleep restriction (here, via the mediating variable, *KSleepy*) to reduce trustworthiness is somewhat magnified for those decisions involving a more sizeable and clear signal of trust by the first-mover.

FIGURE 1: Protocol Details and Timeline



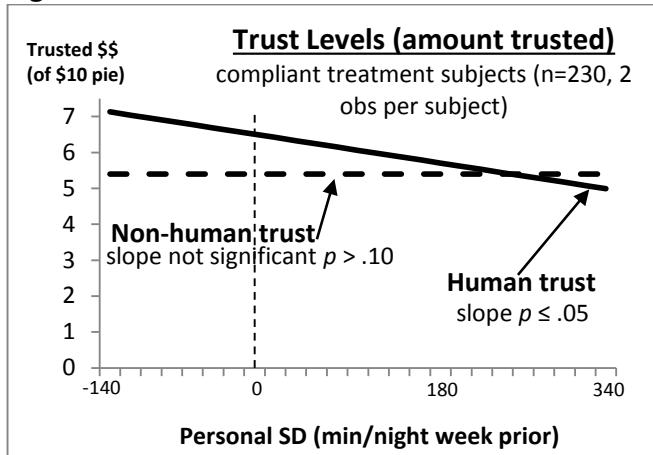
Note: Figure reproduced from Dickinson *et al* (2016)

Figure 2: Predicted Dictator Offers (Altruism)



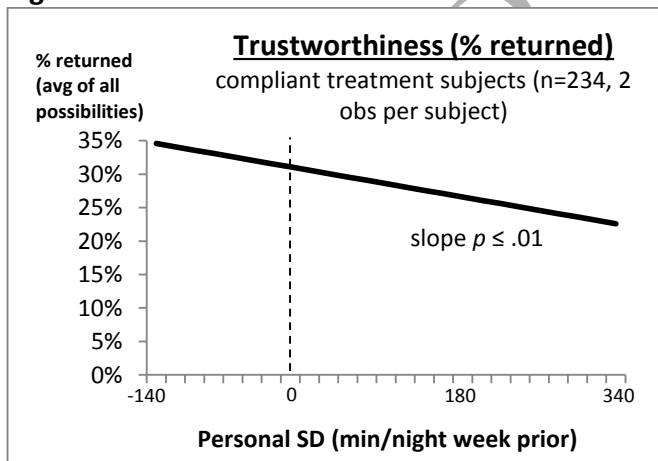
NOTES: Forecast derived from Table 4 with levels of all statistically insignificant variables set to zero. Fig. shows range of values of *Personal SD* observed in sample of compliant treatment subjects. Results show that when dictators are more chronically sleep restricted, they are less altruistic.

Figure 3: Predicted Trust Levels



NOTES: Forecast derived from Table 5 with levels of all statistically insignificant variables set to zero. Fig. shows range of values of *Personal SD* observed in sample of compliant treatment subjects. Results show that when subjects are more chronically sleep restricted, they trust less of the \$10 pie. Non-human trust line shows predicted trust when 2nd-mover is known to be an automatic pass-back algorithm as opposed to another subject. Trusted amounts in non-human trust are not significantly affected by *Personal SD*.

Figure 4: Predicted Trustworthiness



NOTES: Forecast derived from Table 6 with levels of all statistically insignificant variables set to zero. Fig. shows range of values of *Personal SD* observed in sample of compliant treatment subjects. Results show that when subjects are more chronically sleep restricted, they are less trustworthy. (i.e., average % of pie returned is lower).

APPENDIX A (Data and Analysis)

Sleep Data Acquisition

Actigraphy data acquisition (Actiwatch Spectrum Plus devices; Philips Respironics) was at 30-second time epochs. Each epoch is initially scored as “sleep” or “wake” with manufacturer’s software. Rest period start/end times are then adjusted manually, if necessary, using subject-entered actigraph event markers and with the input of complementary sleep diaries. All manual scoring is conducted using validated scoring protocols (Goldman *et al*, 2007).

The devices use an MEMS type accelerometer and sample data at 32 Hz. Devices are waterproof at 1m for 30 minutes and so subjects were instructed to wear them 24 hr a day during the 3-week protocol, except for the exceptional removal to avoid device damage (e.g., contact sports, working with chemicals, etc). Battery life at 30-second data sampling epochs is over 30 days and so subjects had no concerns with battery life or device recharge.

Sample Information

A comprehensive analysis of attrition, compliance, and validation of the protocol in generating significant differences in sleepiness can be found in Dickinson *et al* (2016). A total of 256 subjects were recruited for the study. Of these, 35 (14%) failed to show up for Session 1, and of the remaining 221 subjects, 184 completed the protocol (n=30 control subjects, n=154 treatment subjects). Higher depression risk scores on the primary care PHQ-2 screening questionnaire (Kroenke, Spitzer, and Williams, 2003) predict a lower likelihood of showing up for the first study session (after recruitment signup), and they additionally predict a lower likelihood of completing the protocol conditional on showing up for the first session. Higher scores on the anxiety disorder risk screener GAD-7 (Spitzer *et al*, 2006) predict an increased likelihood of completing the protocol, conditional on starting the protocol (i.e., showing up for the first study session). It should be noted that scores on these two primary care screening questionnaires have some predictive power regarding selection and attrition in our sample, even though subjects surpassing the standard primary care cutoff for risk of depressive or anxiety disorder were *not* recruited at all. In other words, the variation in the screener score results within the “safe zone” (i.e., below the cutoff that typically generates follow-up screening for these disorders) explains some of the final sample qualities. For this result, these screener scores are included in the main statistical analysis as a way to account for their potential effects on behavioral outcomes via their impact on predicting protocol completion. The only other demographic or session control variable predicting study completion was *Morning Session*, where we find that subjects randomly assigned to a morning session group were more likely to finish the protocol conditional on starting the protocol.

Of the 184 subjects completing the study, actigraphy malfunction caused the loss of data on a few subjects, such that we have 179 subjects (n=30 control subjects, n=149 treatment subjects) of complete actigraphy data. Using our standard for compliance for treatment subjects, which requires a subject to have at least a ≥ 60 minutes difference between nightly sleep during the well-rested compared to sleep-restricted weeks, we have 119 compliant treatment subjects—a compliance rate of about 80% (n=30 control subjects were 100% compliant). Figure S1 shows the average nightly sleep levels based on

scored actigraphy data for the treatment versus control subjects for the different weeks of the protocol. Figure S2 shows the distribution of differences (WR-SR) of nightly sleep amounts for the treatment versus control subjects. As can be seen from Fig. S2, a compliance standard of a treatment week difference of at least 60 min/night sleep is statistically sound. That is, a subject drawn at random from our study who has at least this 60 min difference between nightly sleep amounts in the two treatment weeks is unlikely to have been drawn from the control subject sample (see distribution overlap points).

Figure S1: Nightly Sleep Averages (actigraphy measured)

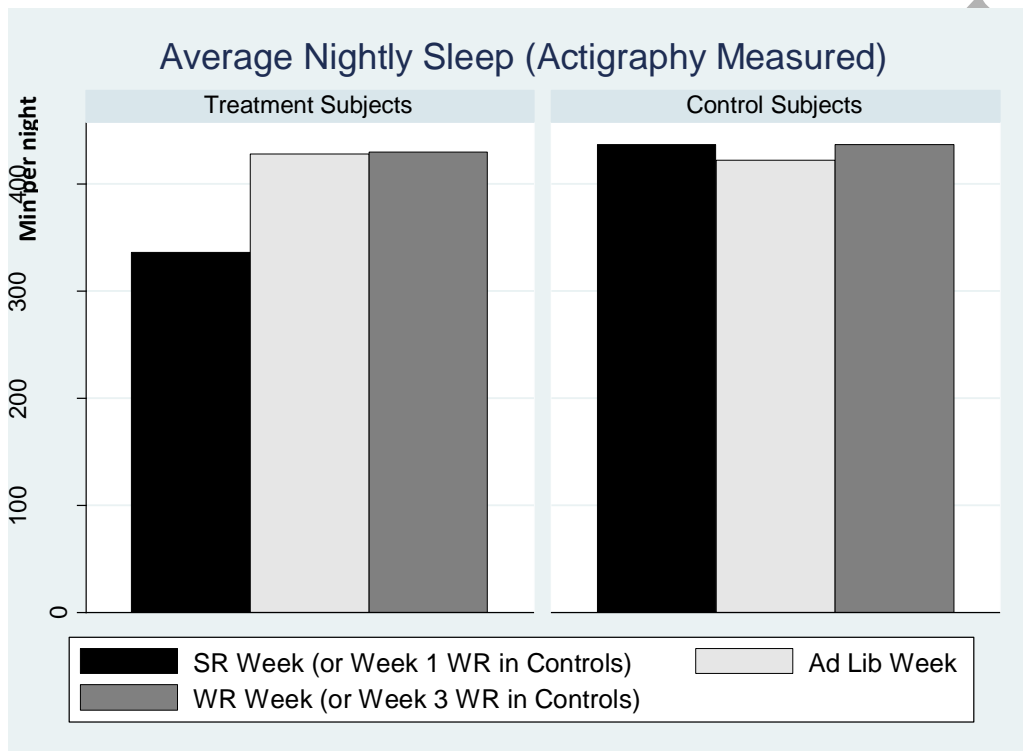
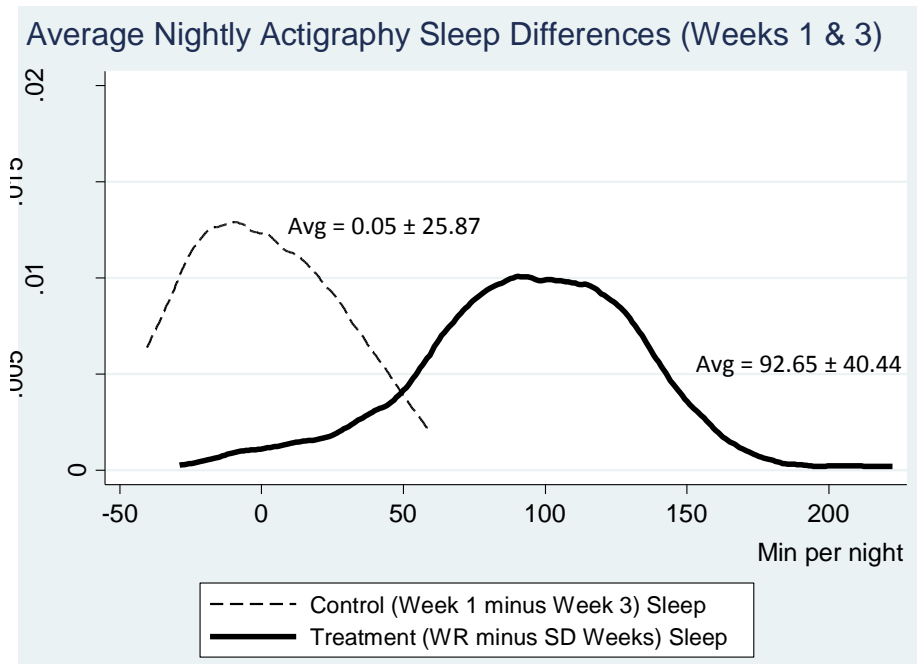


Figure S2: Sleep difference distributions (control vs. treatment subjects)

Note: Figure reproduced from Dickinson *et al* (2016)

In the main text, we report significant increases in subjective sleepiness as a result of both the sleep restriction and circadian mismatch manipulations. There is some evidence that the overall effectiveness of the manipulation is stronger regarding the sleep restriction manipulation, compared to the circadian mismatch manipulation. This claim is substantiated from data on a subset of $n=80$ of our subjects for whom we administered the PANAS instrument to measure positive and negative affective states (Watson *et al*, 1988). Sleep restriction is found to significantly increase subject self-reported irritability and decrease self-reported alertness. The same is not true of the circadian mismatch manipulation as there is no estimated difference in these mood states resulting from the mismatch manipulation.,

We also collected self-report data on caffeine and/or sugar consumption in the 3 hours prior to the lab session, scored as a dichotomous variable as equal to 1 without regard to quantity of either as specific quantities were not elicited. Table S1 below shows no evidence of significant differences in either items as a function of sleep condition, though the mean level is somewhat higher in the SR=1 MM=0 data.

Table S1: Self-report Caffeine and/or Sugar consumption (Caf/Sug=1)

Treatment Combination	Caf/Sug Mean	Robust St Errors (clustered on subj)	[95% CI]
SR=0 & MM=0	.328	.060	[.208, .447]
SR=1 & MM=0	.403	.063	[.279, .527]
SR=0 & MM=1	.333	.063	[.209, .458]
SR=1 & MM=1	.339	.064	[.213, .465]

Note: self-reported caffeine and/or sugar consumption is elicited at the beginning of the decision session.

Statistical Analysis: Additional Estimations

Table S2 below shows results from estimations that using only a dichotomous scoring of sleep-restriction (SR=1 or 0). In general, the results are similar to what is reported in the text using the more informative continuous measure of sleep restriction, though significance is reduced as one would expect. The trustworthiness result is only significant when considering how SR impacts trustworthiness in the fact of high initial trust levels, which is again consistent with estimations reported in the main text.

Table S2: Key Outcome Estimations with categorical variables only for experimental manipulation (along with controls for morningness/eveningness preference, session time-of-day, and session number)

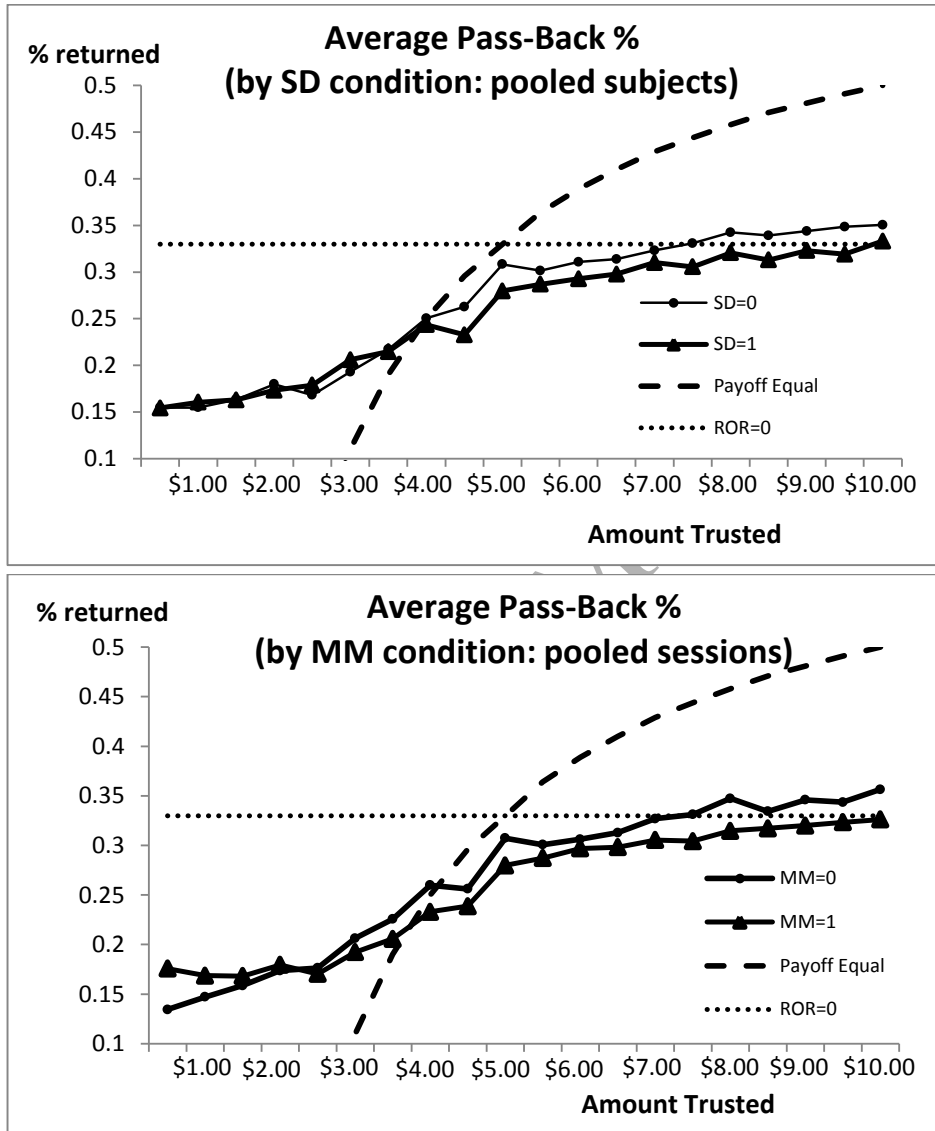
Random effects GLS regressions Standard errors clustered on subject (2 observations per subject)						
Variable	Ultimatum \$\$ Offer	Ultimatum \$\$ MAO	Dictator \$\$ Offer	Trust person \$\$ Offer	Trust Distribution \$\$ Offer	Trust- worthiness [^] % passback
SR (=1)	-.084 (.088)	-.019 (.131)	-.491 (.199)**	-.476 (.248)*	-.076 (.249)	-.017 (.013)
MM (=1)	.077 (.170)	.234 (.300)	-.217 (.333)	-.119 (.363)	.521 (.396)	-.001 (.024)
Morning Session (=1)	.079 (.169)	.362 (.300)	.284 (.333)	.273 (.363)	-.288 (.396)	.003 (.024)
MEQ score (higher=MT)	.0007 (.015)	-.012 (.027)	-.016 (.030)	-.053 (.033)	-.024 (.036)	-.001 (.002)
Session #3 (=1)	-.175 (.088)**	-.590 (.131)***	-.704 (.199)***	-.622 (.248)**	-.014 (.249)	-.037 (.013)***
Constant Term	4.503 (.245)***	2.044 (.429)***	3.994 (.488)***	6.102 (.537)***	4.818 (.583)***	.303 (.035)***
N	234	236	237	230	236	234
Wald χ^2	5.05	22.60***	18.40***	11.91**	2.84	9.36*

Notes: *, **, *** indicate significance at the .10, .05, and .01 levels, respectively, for the 2-tailed test.

[^] Estimation of the Trustworthiness model using the alternative dependent variable of returned amounts when 50% or more of pie is trusted shows that the sleep restriction treatment reduced trustworthiness when high levels of trust are at stake ($\beta = -.024, p < .05$).

Finally, we present a graphical depiction of the trustworthiness results for the reader to compare outcomes with the return amount that would equate payoffs versus the return amount that would generate a zero rate of return (ROR) for the trustor. As can be seen in Figures S3, the impact of SD or MM on pass-back levels is to reduce them. The Figures highlight how pass-back levels, overall, approach payoff equality levels for the higher initial trust levels.

Figure S3: Trustworthiness Distributions (by sleep condition)



Notes: This Fig. S3 does not control for other variables or take into account the repeat-administration nature of the data and is for illustrative purposes only. ROR is the rate-of-return on the first-mover's investment or amount trusted. Because the experimenter triples whatever amount is initially trusted, a one-third trustworthiness level will return back the invested capital to the first-mover. The Payoff Equal amount takes into account that only the first-mover is endowed with the initial \$10 to trust or not. For example, if all \$10 is trusted, then the second-mover possesses \$30 (and the first mover possesses zero). The second-mover will have to send back 50% to guarantee equal payoffs of \$15 for each.

APPENDIX B (Task Instructions)

INSTRUCTIONS (Common to all 4 Decision Environments)

This experiment task involves a series of decisions between you and a randomly chosen counterpart from your experiment group. There are 4 different “Decision Environments” in which you must make decisions. When all decisions have been completed, only **one** of the Decision Environments will be chosen (at random) and used for actual payoff based on you and your counterpart’s decision. The counterpart with whom you are randomly matched for this task will remain anonymous to you, and you will remain anonymous to him/her. Below, we will describe the common features of each of the 4 Decision Environments. The decision sheets that follow will then present the unique details of each Decision Environment and your choices for each environment are made on those sheets.

- **Rounds and Matchings:** Each Decision Environment is a one-round decision experiment, in which you will be randomly matched with an anonymous counterpart. You will never know who your counterpart is in this experiment.
- **Roles:** There are two roles in each Decision Environment. One of the roles is the initial decision maker or “**first mover**”, and the other role is the responder or “**second mover**”.
- **Decisions for each role:** We will ask you to make a decision for *both* roles in each Decision Environment. In fact, you will **not** know which role you are assigned in the Decision Environment that is randomly chosen for pay until after all decisions are made. Thus, at the time of making your decisions you do not know whether you will end up being the **first mover** or **second mover** in this task.
- **Only one Decision Environment will count:** Once all decisions have been made (both **first mover** and **second mover** decisions) by all subjects, decision sheets for this task will be collected. At the end of today’s session, the experimenters will then randomly choose one of the four Decision Environments to count for pay. There is an equal chance that any of the four Decision Environments will be selected for pay.
- **Random counterpart assignment:** Once a Decision Environment is randomly chosen, the experimenters will also randomly match subjects to form anonymous pairs. The identities of these assignments will not be revealed to either subject.
- **Random role assignment:** Once pairs have been randomly assigned, the experimenters will also randomly assign one subject in the pair to be the first-mover, and the other subject will be the second-mover.
- **Payment at end of Session:** For the Decision Environment randomly chosen for pay, you and your counterpart’s decision in your randomly assigned roles will determine your payoff for this task. This payoff will not affect your payoffs from any other part of the experiment.

(Instructions for the 4 decision environments follow. **Note to Readers:** Decision Environments 1, 2, 3, and 4 are the Ultimatum, Dictator, Trust (human), and Trust (algorithm) tasks, respectively).

First Mover Decision: You must propose a division of \$10 between you and your randomly chosen counterpart. Choices can be made in \$.25 increments, and any proposal is allowed as long as the amount you keep plus the amount the **second-mover** receives sum to \$10. In this Decision Environment #1, the **second-mover** may choose to either accept or reject your proposal (and rejection implies you will both get a zero payoff).

DECISION TO MAKE AS FIRST-MOVER

If randomly assigned as the **first-mover** in Decision Environment #1 (*and* Decision Environment #1 is chosen to count for real payoffs), then your decision here, along with the responder's decision, **will determine your payoff**.

I propose to divide the \$10 as follows:

I keep=

The second-mover receives=

DECISION TO MAKE AS SECOND-MOVER

You must indicate whether to accept or reject each of the possible proposals that could be made by the **first-mover**. Please make an "Accept" or "Reject" choice for each and every possibility in the Table below. Your decision for a particular proposal **will determine your actual payoff** (and the **first-mover's** payoff) should this Decision Environment be selected to count for real payoffs *and* you are assigned as the **second-mover**.

Proposal amount (i.e., the amount you would receive out of the \$10 amount)	My choice A=accept R=reject	Proposal amount (i.e., the amount you would receive out of the \$10 amount)	My choice A=accept R=reject
\$0.00		\$5.25	
\$.25		\$5.50	
\$.50		\$5.75	
\$.75		\$6.00	
\$1.00		\$6.25	
\$1.25		\$6.50	
\$1.50		\$6.75	
\$1.75		\$7.00	
\$2.00		\$7.25	
\$2.25		\$7.50	
\$2.50		\$7.75	
\$2.75		\$8.00	
\$3.00		\$8.25	
\$3.25		\$8.50	
\$3.50		\$8.75	
\$3.75		\$9.00	
\$4.00		\$9.25	
\$4.25		\$9.50	
\$4.50		\$9.75	
\$4.75		\$10.00	
\$5.00			

First Mover Decision: You must propose a division of \$10 between you and your randomly chosen counterpart. Choices can be made in \$.25 increments, and any proposal is allowed as long as the amount you keep plus the amount the **second-mover** receives sum to \$10. In this Decision Environment #2, the **second-mover must** accept your proposal (i.e., it cannot be rejected).

DECISION TO MAKE AS FIRST-MOVER

If randomly assigned as the **first-mover** in Decision Environment #2 (*and* Decision Environment #2 is chosen to count for real payoffs), then your decision here, along with the responder's decision, **will determine your payoff.**

I propose to divide the \$10 as follows:

I keep=

The second-mover receives=

DECISION TO MAKE AS SECOND-MOVER

In Decision Environment #2, the **second-mover** has no choice to make and must simply accept the **first-mover's** decision.

First Mover Decision: You start this experiment with \$10. You must now choose how much (if any) of this amount to keep and how much (if any) if this amount to pass to the **second-mover**. Choices can be made in \$.50 increments. Whatever amount is passed to the **second-mover** will be *TRIPLED* by the experimenter. The **second-mover** will be given a choice of how much, if any, of the tripled amount he/she wishes to pass back to the **first-mover**. Your pass/keep decision **will be binding** should you be assigned the role of **first-mover** and Decision Environment #3 is randomly chosen for payment.

DECISION TO MAKE AS FIRST-MOVER

If I am assigned to be the **first mover** in this task, I choose to

KEEP=

PASS=

DECISION TO MAKE AS SECOND-MOVER

If the **first-mover** decides to pass any amount from the initial \$10.00 dollars along to you, this amount is multiplied by **3**. At this point, you must then decide how much (if any) to pass back to the **first mover**. *Whatever you do not pass back is yours to keep* and would be your experimental earnings in this Decision Environment #3. Please decide (below) how much (if any) to pass back for each of the possible **first-mover** decisions below. Your pass/keep decision for any particular contingency **will be binding** should you be assigned the role of **second-mover** and should Decision Environment #3 be randomly chosen for payment.

Amount received by second mover (i.e., the amount passed by the first mover multiplied by 3)	I (second mover) choose to pass back
\$0.00	
\$1.50	
\$3.00	
\$4.50	
\$6.00	
\$7.50	
\$9.00	
\$10.50	
\$12.00	
\$13.50	

Amount received by second mover (i.e., the amount passed by the first mover multiplied by 3)	I (second mover) choose to pass back
\$15.00	
\$16.50	
\$18.00	
\$19.50	
\$21.00	
\$22.50	
\$24.00	
\$25.50	
\$27.00	
\$28.50	
\$30.00	

First Mover Decision: You start this experiment with \$10. You must now choose how much (if any) of this amount to keep and how much (if any) if this amount to pass to the **second-mover**. Choices can be made in \$.50 increments. Whatever amount is passed to the **second-mover** will be *TRIPLED* by the experimenter. The **second-mover** will be given a choice of how much, if any, of the tripled amount he/she wishes to pass back to the **first-mover**. Your pass/keep decision **will be binding** should you be assigned the role of **first-mover** and Decision Environment #3 is randomly chosen for payment.

***In Decision Environment #4, the second-mover is a computerized choice algorithm (i.e., not a real person). The computerized choice will mechanically choose an amount to pass back to the first-mover. The computerized (mechanical) choice is based on the distribution of actual pass-back amounts for each of the possible amounts below, as derived from real human decisions.**

(In other words, the computerized pass-back amount will never be an amount that a human subject has never chosen, amounts rarely passed back by human subjects will be rarely passed back by the computerized second-mover, and the amounts most often passed-back by human subjects are most likely to be passed back by the computerized second-mover—this is true for each of the different first-mover choice contingencies)

DECISION TO MAKE AS FIRST-MOVER

If I am assigned to be the **first mover** in this task, I choose to

KEEP=

PASS=

DECISION TO MAKE AS SECOND-MOVER

There is no decision to make as a **second-mover** in Decision Environment #4, as the **second-mover** is a computerized choice algorithm.