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IZA DP No. 13242

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MAY 2020

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MAY 2020

ISSN: 2365-9793

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ABSTRACT

Sleep Restriction Increases Coordination Failure

When group outcomes depend on minimal effort (e.g., disease containment, work teams, or indigenous hunt success), a classic coordination problem exists. Using a well-established paradigm, we examine how a common cognitive state (insufficient sleep) impacts coordination outcomes. Our data indicate that insufficient sleep increases coordination failure costs, which suggests that the sleep or, more generally, cognitive composition of a group might determine its ability to escape from a trap of costly miscoordination and wasted cooperative efforts. These findings are first evidence of the potentially large externality of a commonly experienced biological state (insufficient sleep) that has infiltrated many societies.

JEL Classification: C91, D91

Keywords: coordination games, sleep, cooperative dilemma

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1 Introduction

Coordination games have widespread applications of interest across disciplines like economics, organizational behavior, and psychology. As such, how individuals solve coordination problems (or factors that predict coordination failure) are a natural focal point for behavioural research. Examples of coordination problems are found in a variety of diverse environments such as the occupational settings of team production or industrial disaster risk management[1], the behavioral anthropology of aboriginal subsistence whaling, and containment efforts of communicable disease outbreaks, to name a few. In fact, the recent COVID 19 pandemic has made clear that stakes can be high in coordination environments where outcomes are dictated by the minimal effort within a group (e.g., family unit, social circle, larger community groups).

Here we extend our understanding of how deliberation (i.e., high-level cognition) affects coordination success by randomly inducing a common cognitive state prior to decision making—we manipulate sleepiness. Specifically, before the administration of an incentivized coordination game task, we experimentally manipulate sleep levels over the course of one week to generate an ecologically valid set of well-rested (WR; assigned 8-9 hrs nightly sleep) and sleep restricted (SR; 5-6 hrs nightly sleep) experiment participants. These two treatments approximate the difference between recommended nightly sleep levels and those experienced by a significant portion of the adult population in many countries (Hafner et al., 2017). Thus, our study provides new insights into the consequences of worrisome sleep habits that have become pervasive in recent decades.

Game theorists also have a significant interest in coordination problems, in general, as a standard paradigm to study cooperative dilemmas. Because coordination games present a multiplicity of Pareto-ranked Nash equilibria, they stand in contrast to other well-known cooperative dilemmas in game theory like the Prisoners Dilemma or common pool resource problem. And, some research suggests that individuals may coordinate on the most inefficient possible Nash outcome in such games (Cooper et al., 1992; Ochs, 1995; Van Huyck et al., 1990, 1991), which makes the identification of factors that lead to both coordination failure and inferior coordination outcomes of great importance. Our behavioural focus on sleep restriction as one such potential factor is intended as a highly real-world relevant way to manipulate the likelihood that participants make coordination choices via more automatic versus more deliberative decision processes.[2]

This paper contributes to the literature in a timely fashion. Using an ecologically valid protocol, we manipulated sleep to levels that are commonplace in modern society–nearly 30%

[1]Other examples of interest to organizational researchers have been noted in the literature (Knez and Camerer, 2000).

[2]Killgore et al. (2012) highlights how decisions relying on critical components of the prefrontal cortex-those necessary for deliberation and executive function-are particularly vulnerable to the impact of sleep deprivation.

of U.S. adults operate daily at the levels of sleep restriction (SR) we induced in our study (Schoenborn and Adams, 2010). Recent estimates using data from several industrialized countries found that these levels of insufficient sleep can cost an economy anywhere from 1%-3% of its annual GDP (Hafner et al., 2017).[3] Yet, little is known about how commonly experienced SR impacts group interactions. Because SR can be thought of as an externally valid way to alter the cognitive mechanism used to make decisions, our results will have implications for our understanding of the general underpinnings of decision making as well. And, the controlled decision environment we examined allows us to quantify the costs of coordination failure in a way that is difficult in naturally occurring field settings. We find that wasteful miscoordination increases when sleep restricted individuals are part of the group. And, in an environment of repeated interaction with the same group members, which have been shown to improve coordination on Pareto superior Nash equilibria, sleep restricted group members can entirely eliminate the gains from repeated interaction.

2 Background

Experimental research has well-established results showing coordination failure (Cooper et al., 1990, 1992; Van Huyck et al., 1990, 1991). One of the most important factors found to improve the evolution of coordination success has been repeated interaction with the same group members (see Devetag and Ortmann (2007), for a review of different factors that may facilitate coordination). Our evidence shows that the benefits of repeated group interaction may be entirely undone in the presence of sleep restricted group members.[4]

The literature on sleep and social interactions is somewhat limited. We know of no studies that explicitly examine coordination games and sleep restriction, notwithstanding the growing literature on sleep and decision making. A few studies have looked at the impact of sleep restriction or deprivation on simple human social interactions, which may yield some clues to help guide our hypotheses. A single night of total sleep deprivation (TSD) reduces trust (Anderson and Dickinson, 2010), which is consistent with TSD increasing aversion to exploitation risk in a social interaction. TSD has also been shown to reduce risk taking and dictator giving in female, but not male, participants (Ferrara et al., 2015). In a larger sample of chronic but partially sleep restricted (SR) participants more similar to levels found in naturally occurring data, SR reduced prosocial behaviours (including trust) (Dickinson and McElroy, 2017). Finally, a recent study using multiple methodologies to examine sleep restriction found that insufficient sleep predicts reduced levels of civic engagement activities

[3]The U.S. Centers for Disease Control and Prevention has also labeled the levels of chronic partial sleep deprivation we study as a public health epidemic.

[4]Others have recognized the potential impact of sleep on successful work team outcomes from an organizational standpoint (Barnes and Hollenbeck, 2009). Our experimental approach is intended to examine an environment where coordination success or failure can be clearly quantified.

that can be considered prosocial (Holbein et al., 2019). Such results are consistent with related research showing that deliberative thinking, which is less likely with SR, is important for prosocial decisions (see also Chee and Chuah (2008); Krajbich et al. (2015); McCabe et al. (2001); Rilling and Sanfey (2011)).

Coordination games, of course, contain elements of risk as well as trust. TSD increases risky choice over monetary gambles in the gains domain McKenna et al. (2007). Relatedly, a more mild manipulation of sleepiness has been shown to increase preference for risk taking among sleepier participants Castillo et al. (2017).[5] So, the literature suggests SR likely reduces trust but increases willingness to take monetary risk. If successful coordination requires trust and/or willingness to take risk, then it is unclear what impact SR may have on coordination success/failure. This study provides first evidence on which of these SR effects likely dominates in a coordination setting that is more complex than simple 2-person trust recently examined in the literature.

3 Experimental Design

3.1 Sleep protocol

A preliminary online survey was first administered to generate a database of several hundred potential participants on which we had necessary demographic and sleep data for recruitment to the main one-week study. The preliminary survey administered a validated short form of the morningingness-eveningness questionnaire (Adan and Almirall, 1991), validated short-form screening instruments for depressive (Kroenke et al., 2003) and anxiety disorder (Spitzer et al., 2006), and a few other self-report sleep measures. We did not recruit subjects scoring at risk for major depressive or anxiety disorder or those who self-reported a sleep disorder. Strong morning- and evening-type individuals were excluded so that we did not introduce the confounding factor of circadian timing of the decision.[6]

Once excluded respondents were removed from the database, remaining participants were randomly assigned, ex ante, to the well-rested (WR: 8-9 hr/night attempted sleep) or sleep-restricted (SR: 5-6 hr/night attempted sleep) treatment condition. At that point, recruitment emails invited individuals to participate in a one-week experiment that would involve a prescribed nightly sleep level for 7 consecutive nights (the recruitment email included the specific sleep prescription randomly assigned to the subject).[7] Participants were also informed

[5]Specifically, their protocol randomly assigned validated morning-type and evening-type participants to take part in the risky (individual) asset bundle choice experiment at either an early morning (7:30 am) or later evening (10:00 pm) experiment session time.

[6]Also important to remove circadian timing effects was the fact that all sessions for the main experiment were held at non-extreme times-of-day (between 10am-4pm) and sessions were only held Tuesday-Thursday (to minimize weekend effects).

[7]Participants were not allowed to opt out of one treatment in order to select the other. Thus, they either

they would be required to wear an actigraphy device to objectively yet passively measure sleep levels, keep a basic sleep diary provided by the experimenters, and participate in a 1.5 hour decision session at the end of the week.[8] This one-week experiment protocol therefore required two lab visits by the subject. Session 1 included informed consent procedures, survey instruments to collect data on a 6-item cognitive reflection task (Primi et al., 2015) and short-version of the Big Five personality measures (Gosling et al., 2003)[9], assignment of the actigraphy device and sleep diary, and participant questions were answered regarding the prescribed sleep treatments in a way that did not reveal any participants treatment assignment. Groups of typically 15-18 subjects were recruited at a time, and these groups were a mix of SR and WR subjects so that the coordination games would contain some sleep level heterogeneity in the group compositions.

Upon leaving Session 1, participant contact with experimenters was limited to daily text or emails each subject would send to report bed/wake times. This was in addition to similar information reported in each participants sleep diary, but the emails allowed the experimenter to have some daily monitoring of attempted sleep levels. Nevertheless, these emails are self-reports and only serve as complementary and subservient to the objective sleep data in the final sleep data scoring. The experimenters also emailed participants every 1-2 days during the treatment week to remind the subjects of the prescribed sleep levels, caution the subjects regarding risk of certain activities when sleepy (which was a likely byproduct of the SR treatment, of course), and to remind subjects of the approaching decision Session 2. Because the at-home nature of the sleep protocol presents certain risks, it is important to note risk management measures that were employed. We followed similar at-home sleep protocol procedures as in each single treatment week of a recent at-home sleep manipulation protocol (Dickinson et al., 2017), which included risk disclosure during informed consent procedures, regular cautionary emails during protocol week, and zero restrictions on compensatory behaviours like caffeine or sugar consumption. Because subjects were also free to withdraw from the study at any point and non-compliance to the sleep prescription did not produce large consequences for the subject (except possibly a reduction in their fixed compensation in extreme cases), we (as experimenters) bore the extra cost burden of non-compliant subjects in exchange for some additional risk mitigation in the at-home protocol.

Session 2 occurred one week after session 1 (at the same time of day as Session 1)

participated in their randomly assigned sleep condition, or they could not participate in the experiment.

[8]The actigraphy device is a wrist-worn accelerometer intended to be worn all day every day with few exceptions. Importantly, we used devices common to sleep research (the Actiwatch Spectrum Plus) that have several advantages over lower cost commercial devices. The validity of the particular devices we used has been established in the literature and actigraphy is well-accepted as a way to generate objective and valid data on sleep levels in non-disordered individuals (Sadeh, 2011).

[9]We do not find significant pre-existing differences between compliant and not compliant subjects in terms of CRT tests or personality ($p>.05$). The only significant pre-existing difference between SR and WR is regarding Openness (p-value$<.05$), which should not have an impact on coordination game decisions given anonymity within the task.

and included a short survey and self-report on sleepiness, decision task administration, and then the removal of actigraphy devices and cash payments for the decision experiments. In addition to variable payoffs for outcomes in the decision experiments, subjects also received a fixed payment of $25 for adherence to the conditions of the sleep treatment week. Subjects were made aware that the fixed payment would be received several days later by Amazon.com gift code or check (their choice) after sleep data were downloaded and the experimenters could verify good faith efforts at compliance.[10]

We recruited a total of n=127 treatment participants into the main study. Again, the main study involved random assignment to a nightly SR or WR sleep level for one week and required use of a wrist-worn actigraphy device (or, "sleep watch") to passively but objectively measure participant sleep levels (see Methods for more detail). However, not all of the participants finished the one-week protocol. Of those recruited, n=11 participants failed to show up for Session 1 at the start of the protocol (i.e., no-shows: n=8 female), of which n=3 had been assigned to the SR condition. Of the 116 participants who started the protocol, 14 (n=12 female) withdrew at some point during the sleep treatment week. Those who withdrew were all assigned to the SR condition.[11] We therefore had a total of 102 treatment participants who completed the main one-week sleep treatment and decision experiment at the end of the treatment week. Sleep watch data were corrupted for two participants, leaving 100 participants worth of complete sleep and decision data (n=62 females. n=47 SR, n=53 WR participants).

3.2 Minimum effort coordination game

During the decision session, participants were administered the minimum effort coordination game through the Veconlab online platform.[12] The task was incentivized such that real monetary payoffs, which were paid in cash after the experiment, resulted from choices made in the coordination game. The basic idea of the game is that members of a group must each decide on a level of hypothetical (but costly) effort. Once all decisions have been made, the payoff outcome in the game was dictated by the *minimum* effort choice from within the group. Thus, effort costs are wasted if group members fail to coordinate on a given level of effort choice, and coordination at higher effort choices is payoff-preferred to coordination at

[10] It is important to note that our standard for compliance with respect to paying subjects the $25 payment was not as stringent as our standard for compliance regarding our data analysis later in this paper. In general, we wished to err on the side of paying subjects the $25 in most instances and gave partial payment to the few subjects who withdrew partway through the sleep treatment week.

[11] The fact that all those lost due to mid-week attrition were SR particants is likely due to SR compliance being more difficult than anticipated. That said, of those who finished the protocol, most who were deemed noncompliant with the sleep prescription were WR participants. Thus, rather than withdraw from the study, the WR subject was more likely to simply finish the protocol, only later to be identified as noncompliant.

[12] See http://veconlab.econ.virginia.edu/cg/cg.php for the Veconlab experiment page describing the coordination game.

lower effort choices.

For our study, the coordination game was played with groups of 3 members and the session administered both a 10-round treatment using a partner matching protocol and a 10-round treatment using a stranger matching protocol (order of treatments varied across groups).[13] Table A1 (in the online Supplementary Information (SI) Appendix) describes payoffs as a function of one's effort choice and the minimum effort choice of the other two players in one's group. Effort choice, e, has marginal cost of effort for each group member of $c = \$0.64$. Given this parameterization and our range of effort choice $e \in [1.1, 1.7]$ (choice option granularity was 0.01 units), each marginal increase of 0.1 effort units cost a member \$0.064. Thus, effort-waste costs are present for any group outcome that does not involve identical effort choices, though miscoordination can still vary in severity. Among the set of Nash equilbrium outcomes there is a rank order of payoff preference such that maximal effort choice at e=1.7 for all group members is the payoff preferred Nash equilibrium.

Due to the multiplicity of Nash equilibria in the coordination game, an equilibrium refinement or selection criterion may help guide our baseline prediction. Given the parameterization we implement for our coordination games, the general prediction across a range of equilibrium selection criteria would be coordination on the Pareto-worst minimal effort level choice. This would be the case assuming risk dominance, applying a heuristic-based reference outcome criterion (Schneider and Leland, 2015)[14], or using a game-theoretic selection criterion that considers maximization of a potential function (Monderer and Shapley, 1996). For a useful example of how effort choice varies with cost of effort in this minimal effort choice environment, see also Goeree and Holt (2005). Based on existing literature on coordination games (see survey in Devetag and Ortmann (2007)), there are behavioural foundations to predict increased coordination at non-minimal effort levels when engaged in repeated interactions with the same group members. Thus, established behavioural results may weaken the minimal effort prediction in our otherwise strong equilibrium selection prediction.

4 Results

4.1 Compliance, and manipulation check.

Sleep data on the 100 human participants were scored using standard procedures and the objective nightly average sleep levels of subjects in the SR and WR treatment conditions are shown in Figure 1 (kernel density estimates). The main analysis utilized the full set

[13]Because this design required a group size divisible by 3, we recruited a small number of participants for Session 2 as backup participants so that we would be able to fully use the data on the more costly sleep treatment participants. Backups recruited for only the decision session were administered a separate informed consent document because of the fact that they did not participate in the sleep treatment week.

[14]e.g., the Reference Dependent Maximin criterion (Schneider and Leland, 2015) would predict minimal effect for all $c > \$0.50$.

of 102 participants who completed the protocol, and therefore considered the SR or WR condition as an "intent to treat" variable—this included two participants whose sleep watch data were corrupted given that the sleep data are not necessary for the dichotomous intent-to-treat scoring. Alternatively, we also conducted robustness analysis using the subsample of data deemed compliant with the sleep prescription condition based on complete and objective actigraphy-measured sleep levels (SI Tables A2-A6). Our standard for treatment condition compliance required SR participants to have < 375 minutes objectively measured nightly sleep and WR participants > 405 minutes nightly sleep.[15] The highlighted region of noncompliance between 6.25 and 6.75 nightly hours of sleep (see Figure 1) is close to average nightly sleep levels in adults from recent survey evidence.[16] Robustness analysis that removes subjects with nightly sleep near average levels can be thought of as a way to remove subjects difficult to clearly classify as SR or WR.[17]

Validity of the protocol at manipulating sleep levels and/or sleepiness in our sample can be shown by comparing several distinct measures across the treatment groups. In addition to the objective sleep level data, during Session 2 we elicited self-report sleepiness using the well-validated Karolinska 9-point scale (Åkerstedt and Gillberg, 1990) as well as the extent to which the protocol altered one's typical sleep level ("self-report sleep gain/loss" range was [-4, +4] where 0 would imply "no effect" on typical sleep levels). A fourth measure was constructed, Personal SD, to describe the personal sleep deprivation for a participant. This measure was constructed by subtracting one's objective nightly sleep quantity from that participant's self reported nightly sleep need for optimal performance, expressed in hours/night. This subjective measure of optimal sleep, however, was collected during the preliminary sleep survey at an earlier point in time and is therefore not endogenous with respect to the experimental treatment assignment. To the extent that a participant has an accurate assessment of whether he/she is a higher or lower sleep-need individual, the Personal SD measure could be considered a more individually accurate measure of one's level of SR (or WR) in our study (SI Tables A2-A8 include analysis using this constructed measure as the sleep descriptor). For all measures consider, we report a highly statistically significant difference between the SR and WR group, whether including all subjects or the subset of compliant subjects ($p < 0.01$ in all instances; see lower section of Figure 1, which

[15]Using a within-subjects protocol, Dickinson et al. (2017) also used a compliance standard that was subjective but somewhat data driven and based on the desire to minimize the likelihood that a treatment participant was statistically indistinguishable from a control-group participant not assigned to SR.

[16]See National Sleep Foundation. 2005. 2005 Sleep in America Poll. [Online] Available: http://www.sleepfoundation.org/sites/default/files/2005_summary_of_findings.pdf [accessed March 31, 2017]. More recent Gallup poll results highlight that average sleep levels of younger adults, as compared to all adults, are lower and so average sleep levels of young adults the age of our college student sample are likely within our noncompliance range of sleep (see http://www.gallup.com/poll/166553/less-recommended-amount-sleep.aspx [accessed March 31, 2017])

[17]Note that this compliance standard identifies more WR noncompliant subjects (n=14) than SR noncompliant subjects (n=2). In total, we have n=84 subjects deemed compliant by this standard.

reports results of Mann-Whitney nonparametric tests for differences in median values of each measure across treatment groups).

4.2 Coordination game effort choices and earnings

We analyzed the following outcome measures from our data: effort choices, earnings, the likelihood of group coordination, and total effort waste costs. Table 1 reports results from models estimating individual Effort choice (column (1)) and Earnings (column (2)) as a function of the treatment (Partner vs. Stranger matching protocol), treatment order (dummy variable for Partner condition in 2nd 10-round treatment), the Round ($= 1 - 20$), a dummy variable for assignment to the SR condition, and interactions between SR, Round, Partner matching, and Partner treatment order. Similar models to those estimated in Table 1 were estimated using continuous measures of sleep level, models using only the subset of compliant participant data, and models using intent-to-treat to predict personal sleep deprivation levels (constructed using objective actigraphy-measured total sleep time) in a 2-stage instrumental variables approach. Our results are robust to these alternative estimation approaches (SI Tables A3, A4).

Table 1 results in column (1) indicate that the trend across rounds was a decrease in effort choices towards the predicted minimal effort, but the Partners condition predicts significantly higher effort levels–marginally more in the initial rounds, but significantly more when Partners occurs after the Strangers treatment (Table 1, column (1)). This is consistent with previous results in the literature that report higher average effort levels with Partner matching (Devetag and Ortmann, 2007). Regarding the impact of SR on effort choice, our estimates indicate that SR participants contribute significantly more than WR participants with Partner matching in general, and the trend towards lower effort choices across rounds occurs at a slower rate in SR participants (SI Fig A1). This estimated impact of SR on effort levels is robust across the various specifications (SI Table A3).

Column (2) of Table 1 shows results from estimating similar specifications with participant Earnings as the dependent variable (see also SI Table A4). Here, we see that Partners matching increases earnings, and earnings also increase with each round. Given the column (1) results in Table 1 showing a trend towards lower effort levels, this Earnings result is likely due to a decrease in wasteful (uncoordinated) effort choices. Regarding earnings, there is no significant interaction between SR and the Partners matching (as was found with effort levels). However, SR has a robust effect of decreasing earnings across rounds due to the slower convergence to lower efforts compared to WR group members (see Table 1 and SI Table A4).

The results on Effort and Earnings can be reconciled by evaluating the impact of SR on effort choice disparity or "gap" in a group (SI Table A7) as well as the distinct minimum and maximum effort levels in a group (SI Table A8). These highlight how differential impacts on group minimum and maximum effort levels are responsible for coordination failure costs.

In general, our data show that groups populated with SR participants failed to coordinate significantly more often than groups populated with WR participants. Fisher's exact tests were used to document this general finding of decreased proportions of in-equilibrium play when the number of SR subjects in a group increases (see SI Appendix Table A9)

To more formally examine the likelihood of equilibrium play, Table 2 shows estimation results where the likelihood of coordination on any equilibrium outcome (no matter which of the multiple equilibria it is) was regressed on the number of SR participants in the 3-person group. We find that, even controlling for Round of play, additional SR group members significantly decrease the likelihood of effort coordination.[18] Table 2 also confirms the established result in the literature that Partners matching significantly increases the likelihood of successful coordination (Devetag and Ortmann, 2007) (in addition to increasing the level of effort to a more payoff-preferred equilibrium, as was noted in Table 1). Notably, the coefficient estimates on the SR dummy variables in Table 2 indicate that the increased miscoordination due to sleep restricted group members can be sufficiently high so as to negate the coordination-improving effect of Partner interactions.

4.3 Miscoordination costs

Finally, we note that coordination failure, per se, may not be very costly if miscoordination is minor. We define effort waste costs as the sum of group effort that is in excess of the minimum effort in the group, which represents costly effort that does not increase the group outcome. The previous analysis of equilibrium play likelihood does not consider that miscoordination (i.e., disequilibrium play) can vary in its severity. Figure 2 plots the cumulative distribution function of the total effort waste amount in each coordination game. Here, we see that cumulative group effort costs generally increase when a group contains SR members. Most stark is the low effort waste in Partner treatment groups that contain zero SR subjects. Here, we see that the introduction of just one SR member is sufficient to significantly increase effort waste costs. The Kolgomorov-Smirnov test of differences in distribution of waste between groups with no SR subjects and groups with all SR subjects is significant (p-value$<.001$) and robust to potential correlation between rounds of play.[19] Our key result, therefore, is that we document first evidence on how SR increases coordination failure and miscoordination waste, which are costs of sleep restriction in cooperative dilemmas that have gone previously unidentified.

While it may appear that the costs of miscoordination are born largely by SR team mem-

[18]Only in comparing 2-SR versus 1-SR subject groups in the Partners treatment do we find that an additional SR member increases the likelihood of coordination, although 2-SR member groups in Partners do not coordinate significantly more than 3-WR member groups (and significantly less than 3-SR member groups).

[19]In 10,000 bootstrap draws accounting for serial correlation in Partner sessions, we find that the Kolmogorov-Smirnov test is significant at the 5% level 93% percent of the time.

bers, we can more carefully examine this issue both with the estimation results in Table 1 and with analysis of earnings distributions in Figure 3. The coefficient estimates in column (2) of Table 1 indicate that the earnings improvements across rounds, which resulted from improved coordination on lower effort choices, occurred at a marginally slower rate for SR group members (i.e., the SR*Round interaction). This would suggest that miscoordination costs are disproportionately born by the SR individuals. However, the cumulative distribution functions of earnings that focus on the final 5 rounds of each treatment (r=6-10 in the figure legend) show that, even after experience in a particular treatment of the coordination game, SR individuals seem to have a larger *variance* in earnings compared to WR individuals. Results of a series of Interquantile (IQ) regressions are shown in the lower portion of Figure 3, where earnings were regressed on an SR indicator. These results show significant impact of SR on increasing earning variance relative to WR group members for several of the IQ regions, most notably in the Partners condition. The increased variance in earnings suggests there were SR group members with both lower *and* higher earnings than WR group members in these later rounds. As such, we cannot claim that it is only the SR group members who pay the price of the miscoordination in these groups. Moreover, it is clear that in operational settings a lack of successful coordination carries the additional spillover cost to other stakeholders who have an interest in team member coordination.

5 Discussion

This investigation was aimed at understanding how commonly experienced levels of sleep restriction impact choices and coordination in group interactions. Our results are consistent with the hypothesis that equilibrium play (i.e., successful coordination of choices) decreases when more group members are sleep restricted. This finding highlights that coordination failure and its associated inefficiency are important, though previously unidentified, costs of insufficient sleep levels. Because our sleep restriction protocol is highly externally valid, these results have great relevance for real world decision makers in operational settings. Coordination failure in team production may be more likely if workers have poor sleep habits. This may, for example, increase flight delays because airline ground time depends on the minimal effort of the team of employees prepping the aircraft for its next flight. As another example, sleep restricted community members (or local authorities) may hamper regional efforts to manage the spread of communicable disease, such as COVID 19. One final example might be that of around-the-clock search and rescue efforts, which may face higher risk of unsuccessful or costly delays from miscoordination inefficiencies if team members are sleep deprived. The inefficiency costs resulting from wasted effort in such groups would be challenging to identify in field settings, and it would be difficult to unambiguously assign blame regarding the causal factor leading to miscoordination. A main contribution of our study is that we combined elements of both field and laboratory methods in our examination of this important cooperative dilemma environment: the sleep restriction we induced is realistic and at levels commonly experienced in the "real world", the coordination game setting we used is controlled and generated quantifiable data, and our use of random assignment and validated objective sleep measurements facilitates our ability to claim SR has a causal impact on the behavioral outcomes measured.

A common concern in many developed countries is the increasing prevalence of insufficient sleep within its population. The impacts of insufficient sleep are somewhat well-known in terms of its likely impacts on worker productivity, absenteesm, and adverse health effects. These specific impacts of insufficient sleep have been found to cost the economies of several developed countries 1%-3% of their respective annual GDP (Hafner et al., 2017). However, these statistics do not consider behavioural effects, which likely represent another important dimension to the full costs of insufficient sleep. And, while research has made significant recent advances in our understanding of how sleep restriction impacts decision making, almost none of this research has focused on social or interactive group decision environments. In a sense, estimates of the cost of sleep deprivation might conflate individual output declines and system-level loss of productivity due to miscoordination. We hope that this study will help fill this void and draw attention to this area of inquiry.

Unlike other group or socially-interactive decision environments, coordination problems are unique in their ability to discriminate between choices when multiple Pareto-ranked equilibria are present. When groups converge to any payoff-inferior equilibrium, there are unrealized gains that imply an opportunity cost of suboptimal effort coordination. The practical reality of those opportunity costs can range from mere lost workplace output and profit, to increased risk of deadly delay in search-and-rescue effort, to defeated attempts to contain contagious disease or illness outbreaks. Our results focus more on the inefficiency costs of wasted effort choices that exist when coordination is not realized, no matter how high or low the average effort choice within a group. It should also be highlighted that in operational settings failed coordination imposes additional spillover costs beyond the decision making "team" that our study cannot quantitatively measure. Failed coordination imposes costs on the village (unsuccessful whale hunt), the community (failed disaster/disease risk management efforts), or on firm outcomes that matter to management and shareholders (e.g., successful and timely development of new products). Our novel findings suggest that mild but chronic sleep restriction may be a silent contributor to such spillover costs. Our identification of miscoordination costs that result from sleep restriction also has implications for what countermeasures may be most helpful. Wellness programs that focus on sleep health may be advisable, or there may be an increased need for hierarchy in an organizational setting as a way to improve coordination through supervisory control. However, such workplace strategies to help limit coordination failure are themselves costly to implement. While future research will no doubt shed additional light on this important topic, it is clear that insufficient sleep is costly in ways not previously appreciated.

Acknowledgements

DLD received support from Appalachian State University and the National Science Foundation under grants BCS-1229067 and SES-1734137. Many useful comments were provided by seminar participants at Monash University, the University of New South Wales, the University of Queensland, the University of Auckland, and the University of Montpellier.

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Figure 1: Treatment validation

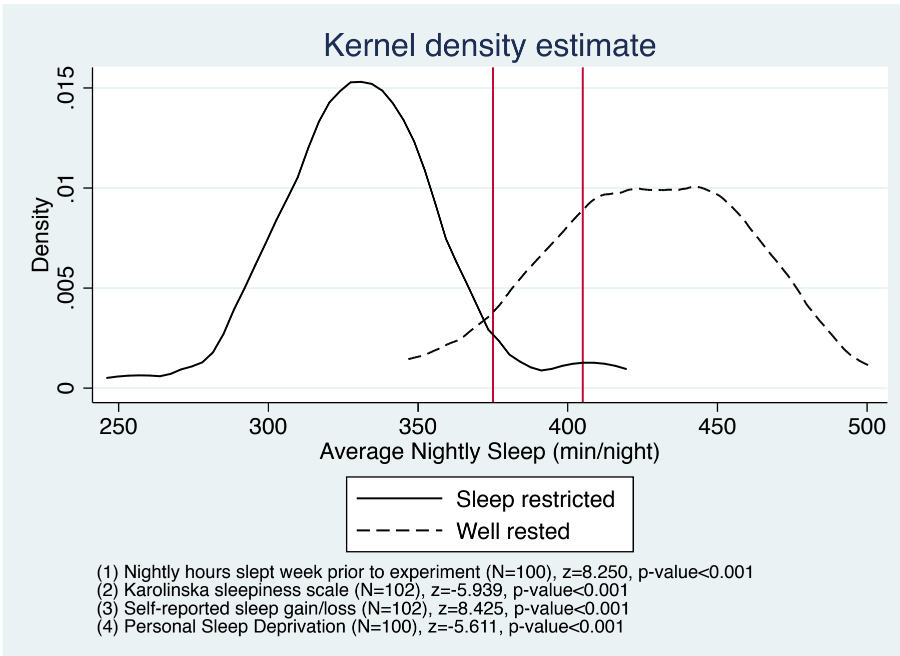


Figure 2: Effort waste costs

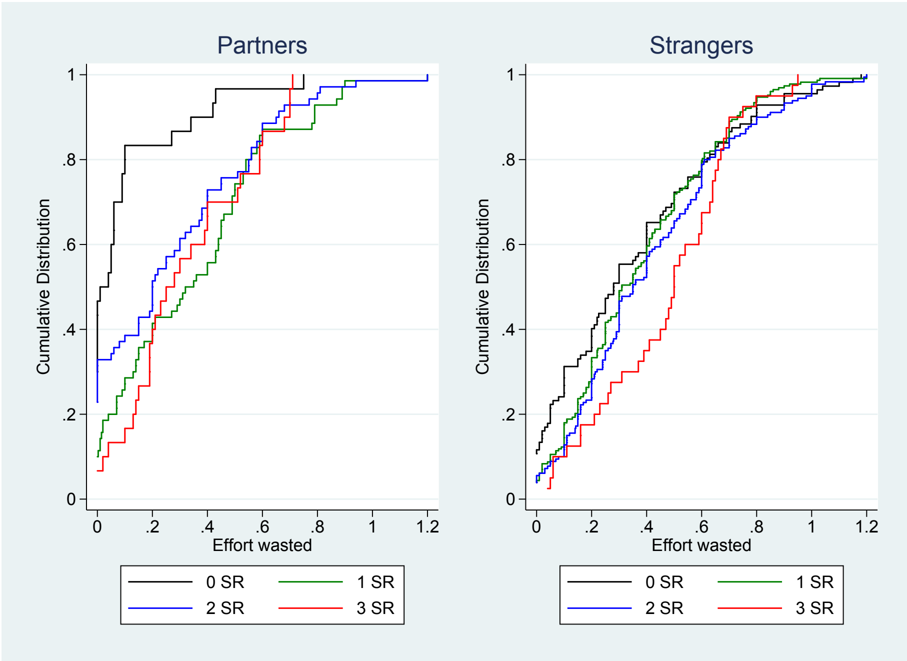


Figure 3: Earning distributions

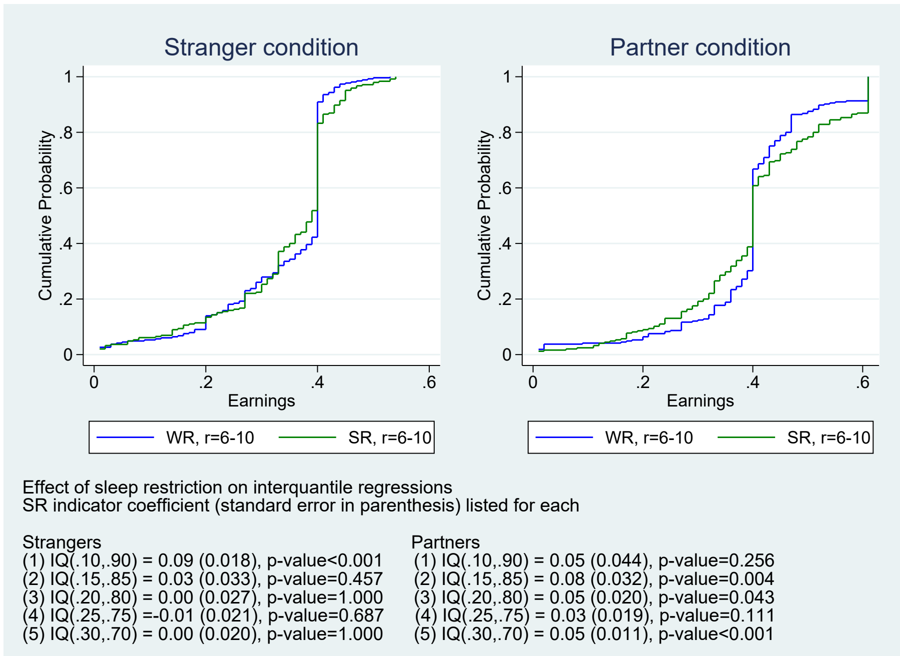


Table 1: Individual Behavior

VARIABLES	(1) Effort	(2) Earnings
Partner cond. in last 10 rounds	0.115***	0.013
	[0.032]	[0.013]
Partner condition	0.030*	0.045***
	[0.018]	[0.011]
Round	-0.010***	0.005***
	[0.002]	[0.001]
Sleep restricted	-0.040	0.029
	[0.041]	[0.020]
Sleep restricted × Partner cond.	0.046*	-0.006
	[0.026]	[0.017]
Sleep restricted × Partner cond. last	-0.059	-0.007
	[0.045]	[0.018]
Sleep restricted × Round	0.006**	-0.002*
	[0.002]	[0.001]
Constant	1.347***	0.276***
	[0.027]	[0.016]
Observations	2,040	2,040
R-squared	0.110	0.060

Robust s.e. in brackets, errors clustered at the individual level
*** p<0.01, ** p<0.05, * p<0.10

Table 2: Likelihood of equilibrium play (i.e., identical effort choices)

VARIABLES	(1) All	(2) Strangers	(3) Partners
Partner condition	0.069***		
	[0.021]		
Round	0.011***	0.007***	0.020***
	[0.002]	[0.002]	[0.005]
One SR subject in group	-0.080***	-0.038**	-0.166**
	[0.025]	[0.018]	[0.067]
Two SR subjects in group	-0.032	-0.048**	-0.035
	[0.027]	[0.022]	[0.068]
Three SR subjects in group+	-0.061***	-	-0.106**
	[0.020]	-	[0.041]
χ^2 test on SR dummies	15.91	6.259	12.80
d.f.	3	2	3
p-value	0.001	0.044	0.005
1 SR = 2 SR	4.66	0.80	6.32
d.f.	1	1	1
p-value	0.0308	0.3721	0.0119
2 SR = 3 SR	0.80	-	6.75
d.f.	1	-	1
p-value	0.3721	-	0.0094
Number of clusters	401	365	38
Observations	760	340	380

Probit models (marginal effects). Robust s.e. in brackets, clustered at group level. + Dummy for 3 SR members perfectly predicts failure of equilibrium play in Strangers.

*** $p<0.01$, ** $p<0.05$, * $p<0.10$

Online Supplementary Information Appendix

Human Subjects Protections

This study was reviewed and approved by the Office of Research Protections Institutional Review Board at Appalachian State University. The online preliminary survey was approved under IRB 09-0252, and the main study (sleep manipulation and decision making) was approved under IRB 16-0067. Consent for the online survey was obtained on p.1 of the survey (necessary for subjects to continue through the survey), and consent for the main study was obtained from participants at the beginning of Session 1.

Actigraphy Sleep Data Acquisition

Participants were assigned wrist-worn actigraphy devices commonly used in sleep research and validated against polysomnographic measures of total sleep time (Sadeh, 2011). Unlike commercial sleep trackers, these Actiwatch Spectrum Plus (Philips) devices provide subjects no sleep data feedback during the treatment week (this is only made known upon downloading the sleep data upon study completion) and the device batteries are sufficient to collect data the entire study week without recharge. Participants were also issued sleep diaries to complete daily and turn in at the end of the treatment week, and participants were required to send daily emails to the experimenter to report wake/bed times. The emails and sleep diaries were complementary to assist the actigraphy data scoring following procedures common to sleep studies (Goldman et al., 2007). The experimenter emailed participants about every 2 days to maintain contact, provide details and reminders of study parameters, caution participants regarding behaviours that might put them at risk if experiencing drowsiness as a result of participation in the experiment, and to remind them of the upcoming Session 2 that finalized the study.

Procedures

The experiments were conducted in the APPEEL laboratory for experimental economics at Appalachian State University. Nine cohorts of participants were recruited, each containing a mix of participants who were randomly assigned the SR or WR sleep level in the study invitation email. Sleep treatment assignments were kept private in the laboratory session, and interactions in the coordination game were anonymous. Decision sessions included three tasks in total, one of which was the coordination game. Participants received a fixed $25 compensation for compliance with the prescribed sleep levels (verified by actigraphy), and providing completed sleep diaries. Fixed compensation (by check or Amazon gift code) was paid several days after completion of Session 2, which was known to the participants, so that researchers could first download the sleep data and verify compliance efforts. Participants also received variable cash payoffs for outcomes in the decision experiments, including the coordination game. The coordination game task was computerized and administered through the Veconlab platform for experiments-the coordination game option used is at http://veconlab.econ.virginia.edu/cg/cg.php.

Table A1: Payoff matrix of coordination game

		Minimum of Other Members' Effort Choices						
		1.1	1.2	1.3	1.4	1.5	1.6	1.7
My effort choice	1.1	.396	.396	.396	.396	.396	.396	.396
	1.2	.332	.432	.432	.432	.432	.432	.432
	1.3	.268	.368	.368	.368	.368	.368	.368
	1.4	.204	.304	.404	.504	.504	.504	.504
	1.5	.140	.240	.340	.440	.540	.540	.540
	1.6	.076	.176	.276	.376	.476	.576	.576
	1.7	.012	.112	.212	.312	.412	.512	.612

Note: Light-shaded payoff cells reflect costly effort choice waste of other group members that do not directly impact one's own payoff.

Table A2: Protocol validation

Intent-to-treat	
Nightly hours slept week prior to experiment ($N = 100$)	$z = 8.250$, p-value< 0.001
Karolinska sleepiness scale ($N = 102$)	$z = -5.939$, p-value< 0.001
Self-reported sleep gain/loss ($N = 102$)	$z = 8.425$, p-value< 0.001
Personal Sleep Deprivation ($N = 100$)	$z = -5.611$, p-value< 0.001
Compliant subjects	
Nightly hours slept week prior to experiment ($N = 84$)	$z = 7.870$, p-value< 0.001
Karolinska sleepiness scale ($N = 84$)	$z = -5.336$, p-value< 0.001
Self-reported sleep gain/loss ($N = 84$)	$z = 7.748$, p-value< 0.001
Personal Sleep Deprivation ($N = 84$)	$z = -6.157$, p-value< 0.001

Note: Mann-Whitney tests. The number of observations for Intent-to-treat reflect two lost observations of sleep data from the actigraphy device measurements. Those two participants were still able to provide the self report measures of Ksleepy or sleep gain/loss

Table A3: Effort

	(1)	(2)	(3)	(4)	(5)	(6)
	Model 1			Model 2		
			OLS		IV	
VARIABLES	All	Compliant	All	Compliant	All	Compliant
Partner cond. in last 10 rounds	0.115***	0.124***	0.108***	0.136***	0.159**	0.158**
	[0.032]	[0.040]	[0.040]	[0.049]	[0.068]	[0.067]
Partner condition	0.030*	0.018	0.002	0.002	-0.016	-0.020
	[0.018]	[0.019]	[0.019]	[0.020]	[0.042]	[0.034]
Round	-0.010***	-0.009***	-0.010***	-0.009***	-0.018***	-0.015***
	[0.002]	[0.002]	[0.002]	[0.002]	[0.004]	[0.004]
Sleep restricted	-0.040	-0.034				
	[0.041]	[0.043]				
Sleep restricted × Partner cond.	0.046*	0.065**				
	[0.026]	[0.028]				
Sleep restricted × Partner cond. last	-0.059	-0.056				
	[0.045]	[0.053]				
Sleep restricted × Round	0.006**	0.005**				
	[0.002]	[0.002]				
Personal sleep deprivation			-0.015	-0.012	-0.047	-0.031
			[0.016]	[0.018]	[0.036]	[0.031]
Per. sleep depriv. × Partner cond.			0.030***	0.029***	0.037*	0.038**
			[0.009]	[0.009]	[0.022]	[0.018]
Per. sleep depriv. × Partner cond. last			-0.011	-0.022	-0.040	-0.037
			[0.019]	[0.022]	[0.036]	[0.033]
Per. sleep depriv. × Round			0.001	0.001	0.006**	0.005**
			[0.001]	[0.001]	[0.002]	[0.002]
Constant	1.347***	1.332***	1.354***	1.336***	1.415***	1.376***
	[0.027]	[0.028]	[0.031]	[0.034]	[0.067]	[0.057]
Observations	2,040	1,680	2,000	1,680	2,000	1,680
R-squared	0.110	0.116	0.112	0.111	0.083	0.092

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.10

Table A4: Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	Model 1		Model 2			
			OLS		IV	
VARIABLES	All	Compliant	All	Compliant	All	Compliant
Partner cond. in last 10 rounds	0.013	0.011	-0.011	-0.006	0.020	0.013
	[0.013]	[0.015]	[0.016]	[0.019]	[0.029]	[0.025]
Partner condition	0.045***	0.053***	0.055***	0.063***	0.054**	0.062***
	[0.011]	[0.014]	[0.014]	[0.017]	[0.024]	[0.022]
Round	0.005***	0.005***	0.006***	0.007***	0.010***	0.010***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.003]	[0.003]
Sleep restricted	0.029	0.037				
	[0.020]	[0.024]				
Sleep restricted × Partner cond.	-0.006	-0.013				
	[0.017]	[0.020]				
Sleep restricted × Partner cond. last	-0.007	-0.003				
	[0.018]	[0.020]				
Sleep restricted × Round	-0.002*	-0.003*				
	[0.001]	[0.002]				
Personal sleep deprivation			0.021**	0.025**	0.041**	0.039**
			[0.009]	[0.011]	[0.018]	[0.016]
Per. sleep depriv. × Partner cond.			-0.007	-0.009	-0.004	-0.007
			[0.007]	[0.008]	[0.013]	[0.012]
Per. sleep depriv. × Partner cond. last			0.010	0.007	-0.006	-0.002
			[0.007]	[0.008]	[0.015]	[0.012]
Per. sleep depriv. × Round			-0.001**	-0.002**	-0.004**	-0.004**
			[0.001]	[0.001]	[0.002]	[0.001]
Constant	0.276***	0.269***	0.253***	0.243***	0.216***	0.215***
	[0.016]	[0.019]	[0.019]	[0.023]	[0.034]	[0.033]
Observations	2,040	1,680	2,000	1,680	2,000	1,680
R-squared	0.060	0.066	0.072	0.076	0.043	0.058

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.10

Table A5: Distance to group minimum effort

	(1)	(2)	(3)	(4)	(5)	(6)
	Model 1		Model 2			
			OLS		IV	
VARIABLES	All	Compliant	All	Compliant	All	Compliant
Partner cond. in last 10 rounds	0.028*	0.033*	0.049**	0.053**	0.036	0.043
	[0.015]	[0.017]	[0.022]	[0.025]	[0.034]	[0.030]
Partner condition	-0.035***	-0.047***	-0.054***	-0.062***	-0.060**	-0.070***
	[0.012]	[0.015]	[0.015]	[0.018]	[0.028]	[0.025]
Round	-0.009***	-0.009***	-0.010***	-0.010***	-0.016***	-0.015***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.003]	[0.003]
Sleep restricted	-0.044*	-0.050*				
	[0.024]	[0.027]				
Sleep restricted × Partner cond.	0.022	0.037*				
	[0.019]	[0.022]				
Sleep restricted × Partner cond. last	-0.013	-0.017				
	[0.025]	[0.027]				
Sleep restricted × Round	0.004***	0.005***				
	[0.002]	[0.002]				
Personal sleep deprivation			-0.027**	-0.030**	-0.058***	-0.051***
			[0.010]	[0.012]	[0.020]	[0.018]
Per. sleep depriv. × Partner cond.			0.017**	0.019**	0.018	0.020
			[0.007]	[0.008]	[0.016]	[0.014]
Per. sleep depriv. × Partner cond. last			-0.014	-0.015	-0.008	-0.011
			[0.010]	[0.011]	[0.019]	[0.017]
Per. sleep depriv. × Round			0.002***	0.002***	0.006***	0.005***
			[0.001]	[0.001]	[0.002]	[0.002]
Constant	0.210***	0.212***	0.235***	0.239***	0.294***	0.281***
	[0.018]	[0.021]	[0.021]	[0.024]	[0.039]	[0.036]
Observations	2,040	1,680	2,000	1,680	2,000	1,680
R-squared	0.063	0.065	0.071	0.070	0.041	0.047

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.10

Table A6: Distance to group median effort

	(1)	(2)	(3)	(4)	(5)	(6)
	Model 1		Model 2			
			OLS		IV	
VARIABLES	All	Compliant	All	Compliant	All	Compliant
Partner cond. in last 10 rounds	0.002	0.006	0.009	0.004	-0.015	-0.002
	[0.011]	[0.012]	[0.015]	[0.017]	[0.025]	[0.021]
Partner condition	-0.016**	-0.031***	-0.019*	-0.025**	-0.014	-0.038**
	[0.007]	[0.007]	[0.009]	[0.011]	[0.019]	[0.016]
Round	-0.005***	-0.005***	-0.005***	-0.005***	-0.009***	-0.009***
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]
Sleep restricted	-0.017	-0.024				
	[0.016]	[0.018]				
Sleep restricted × Partner cond.	-0.007	0.011				
	[0.013]	[0.013]				
Sleep restricted × Partner cond. last	0.017	0.014				
	[0.016]	[0.017]				
Sleep restricted × Round	0.002**	0.002**				
	[0.001]	[0.001]				
Personal sleep deprivation			-0.005	-0.006	-0.028*	-0.028*
			[0.008]	[0.009]	[0.015]	[0.014]
Per. sleep depriv. × Partner cond.			0.001	0.001	-0.004	0.005
			[0.005]	[0.006]	[0.011]	[0.009]
Per. sleep depriv. × Partner cond. last			0.001	0.006	0.014	0.008
			[0.007]	[0.008]	[0.013]	[0.011]
Per. sleep depriv. × Round			0.000	0.000	0.003**	0.003**
			[0.000]	[0.000]	[0.001]	[0.001]
Constant	0.144***	0.148***	0.144***	0.145***	0.186***	0.187***
	[0.011]	[0.013]	[0.015]	[0.018]	[0.028]	[0.029]
Observations	2,040	1,680	2,000	1,680	2,000	1,680
R-squared	0.047	0.049	0.041	0.045	0.012	0.024

Robust standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.10

Table A7: Gap at the group level

VARIABLES	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) OLS	(6) IV
Partner condition	-0.051***	-0.053***	-0.050***	-0.053***	-0.052***	-0.057***
	[0.018]	[0.018]	[0.018]	[0.020]	[0.018]	[0.022]
Round	-0.012***	-0.014***	-0.012***	-0.012***	-0.015***	-0.020
	[0.001]	[0.003]	[0.001]	[0.002]	[0.003]	[0.012]
No. of sleep restricted subjects in group	0.023**	0.010				
	[0.010]	[0.017]				
No. of SR subjects in group × Round		0.001				
		[0.002]				
Mean Personal Sleep Deprivation of subjects in group			0.010	0.090**	-0.006	0.045
			[0.011]	[0.044]	[0.019]	[0.078]
Mean Pers. SD of subjects in group × Round					0.002	0.004
					[0.002]	[0.007]
Constant	0.381***	0.399***	0.394***	0.255***	0.423***	0.335**
	[0.021]	[0.031]	[0.026]	[0.079]	[0.037]	[0.144]
Observations	760	760	760	760	760	760
R-squared	0.167	0.168	0.158	0.051	0.159	0.051

Robust s.e. in brackets, errors clustered at the group level
*** p<0.01, ** p<0.05, * p<0.10

Table A8: Group Dynamics

	OLS						IV		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Min	Max	Gap	Min	Max	Gap	Min	Max	Gap
Partner condition	0.077***	0.026	-0.051***	0.076***	0.025	-0.050***	0.076***	0.024	-0.053***
	[0.023]	[0.025]	[0.018]	[0.022]	[0.023]	[0.018]	[0.023]	[0.024]	[0.020]
Round	-0.002	-0.014***	-0.012***	-0.002	-0.014***	-0.012***	-0.002	-0.014***	-0.012***
	[0.002]	[0.002]	[0.001]	[0.002]	[0.002]	[0.001]	[0.002]	[0.002]	[0.002]
No. of SR subjects in group	0.007	0.029**	0.023**						
	[0.015]	[0.014]	[0.010]						
Mean Personal SD				0.045***	0.055***	0.010	0.026	0.114**	0.089**
				[0.013]	[0.013]	[0.011]	[0.056]	[0.051]	[0.044]
Constant	1.171***	1.552***	0.381***	1.100***	1.495***	0.394***	1.135***	1.391***	0.257***
	[0.027]	[0.029]	[0.021]	[0.029]	[0.033]	[0.026]	[0.095]	[0.092]	[0.079]
Observations	760	760	760	760	760	760	760	760	760
R-squared	0.064	0.161	0.167	0.113	0.188	0.158	0.104	0.139	0.054

Robust s.e. in brackets, errors clustered at the group level

*** p<0.01, ** p<0.05, * p<0.10

Table A9: Equilibrium play

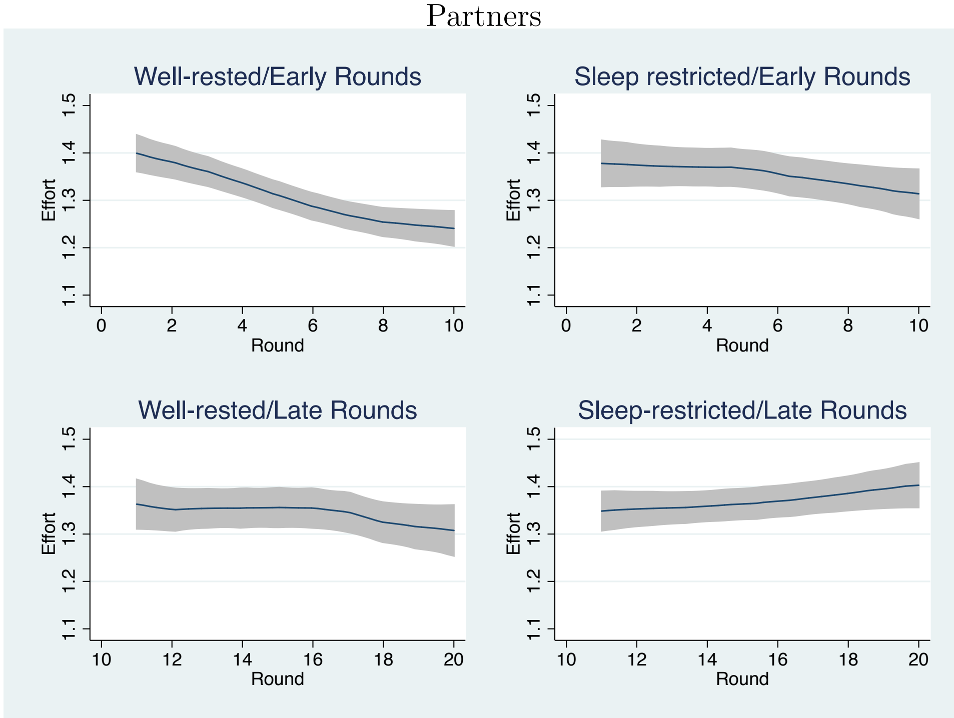
	All				
	SR subjects in group				
	0	1	2	3	Total
Off equilibrium	115	281	217	68	681
Percent	0.81	0.94	0.87	0.97	0.90
In equilibrium	27	17	33	2	79
Percent	0.19	0.06	0.13	0.03	0.10
Fisher's exact test p-value < 0.001					

	Partners				
	SR subjects in group				
	0	1	2	3	Total
Off equilibrium	52	132	109	28	321
Percent	0.74	0.94	0.78	0.93	0.84
In equilibrium	18	8	31	2	59
Percent	0.26	0.06	0.22	0.07	0.16
Fisher's exact test p-value < 0.001					

	Strangers				
	SR subjects in group				
	0	1	2	3	Total
Off equilibrium	63	149	108	40	360
Percent	0.88	0.94	0.98	1.00	0.95
In equilibrium	9	9	2	0	20
Percent	0.12	0.06	0.02	0.00	0.05
Fisher's exact test p-value $= 0.009$					

Figure A1: Effort by round and experimental condition

Partners



Strangers