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THE EFFECTS OF TOTAL SLEEP DEPRIVATION ON BAYESIAN UPDATING

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ABSTRACT

Subjects perform a decision task (Grether, 1980) in both a well-rested and experimentally sleep-deprived state. We have two main results: 1) final choice accuracy is unaffected by sleep deprivation, and yet 2) the estimated decision model differs significantly following sleep-deprivation. Following sleep deprivation, subjects place significantly less weight on new information in forming their beliefs. Because the altered decision process still maintains decision accuracy, it may suggest that increased accident and error rates attributed to reduced sleep in modern society stem from reduced auxiliary function performance (e.g., slowed reaction time, reduced motor skills) or other components of decision making, rather than the inability to integrate multiple pieces of information.

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A large volume of evidence suggests that individuals in industrialized nations are becoming increasingly sleep-deprived. According to a recent poll conducted by the *National Sleep Foundation*, the average American adult slept less than 7 hours per night in 2005. The nightly average was 7.5 hours in 1975 and 9 hours per night in 1910 (Coren, 1996). This trend has significant implications given the known effects of sleep deprivation: decreased motor and cognitive performance, reduced vigilance and reaction time, worsened mood, and reduced ability to think flexibly (Pilcher & Huffcutt, 1996; Harrison & Horne, 1999, 2000). Indeed, even 7 hours of habitual sleep per night leads to significantly diminished cognitive performance relative to 8 or 9 hours (Van Dongen, et al, 2003; Belenky, et al., 2003), which causes us to wonder about the more hidden *decision* effects of sleep loss. Nearly 50 million Americans, close to 25% of all adults, are estimated to suffer from some level of sleep deprivation[1], and so the effects of sleep deprivation on decision-making have widespread implications.

Many occupations promote a culture of sleep deprivation (e.g., emergency personnel, air traffic controllers, medical residents, military personnel, long-haul truck drivers, shift workers). Sleep deprivation costs the U.S. economy $40 billion dollars annually in lost productivity (Stoller, 1997). Additionally, it results in increased workplace accident rates (Melamed & Oksenberg, 2002; Akerstedt et al, 2002), increased absenteeism (Phillips et al., 1991; Kupperman et al., 1995), greater medical morbidity and related costs (Drake et al., 2004), and even slower career advancement (Johnson & Spinweber, 1983). Across numerous settings (work, home, driving, public accidents) Leger (1994) estimated the costs of accidents attributable to sleepiness at $43-$56 billion, in 1988 dollars. Sleep deprivation has also been implicated in several major historical

[1] See data reported by the *National Sleep Foundation*, accessible at www.sleepfoundation.org.

disasters, including the Space Shuttle Challenger explosion, the Exxon Valdez oil spill, and the Chernobyl Nuclear plant explosion (Coren, 1996). In sum, the impact of sleep deprivation in the workplace and on society as a whole, while difficult to measure precisely, is massive.

This paper reports results from a laboratory study that examines the effects of sleep deprivation on information processing. Examinations of flexible thinking, strategy updating, and risk assessment are relatively new to sleep research (see references in Harrison & Horne, 2000; McKenna et al, 2007, Killgore et al, 2006). Past research has utilized complex multi-modal tasks or operational settings that cannot identify *specific* aspects of decision-making affected by sleep deprivation. For example, Harrison and Horne (1999) utilize a marketing simulation game, and they report that 36 hours of total sleep deprivation led to stereotyped decisions failing to integrate previous feedback, resulting in large financial losses and production errors. In operational settings, similarly global outcome measures are reported (e.g., Friedl et al., 2004; Weinger and Ancoli-Israel, 2002). A recent meta-analysis of 60 studies found that "clinical outcomes", the culmination of many decisions, were negatively impacted by physician sleep loss (Philibert, 2005).

Thus, neither laboratory nor applied sleep deprivation studies have measured discrete, quantifiable decision parameters free of confounds. Even the well-known Iowa Gambling Task (IGT), which has been used to examine risky choice behavior in the sleep literature (Killgore et al., 2006), does not allow the researcher to separate risk attitude from subjective probability formation—the latter results from the fact that there is missing information with respect to outcome probabilities in the IGT. McKenna et al.

(2007) addressed this issue and showed that sleep deprivation desensitizes the decision-maker to risk.

In general, the mechanism by which sleep deprivation alters decision making remains unclear. Given that many real-world decisions involve multiple cognitive processes, it is important to separately examine several of those components in an effort to determine which may be specifically impacted by sleep deprivation and which may not. The studies examining risk preference cited above are examples of such work. Another process found in many decisions is the ability to integrate multiple pieces of information into a decision. Sleep deprivation might alter subject tendencies to utilize one or more pieces of information in decision-making.[2] Or, the arrival of new information may elicit an impulsive response as subjects react to new evidence, and this impulse may be altered following sleep deprivation. The current study examines this specific aspect of decision-making through the study of Bayesian updating. The experiment is administered to subjects both well-rested and after 22-25 hours (μ=22.72, σ=.60) of controlled total sleep deprivation. For comparison to previous economics research, we replicate the Bayes rule experiment of Grether (1980).

One can examine the effects of sleep deprivation on the ability to integrate information into a decision from at least two perspectives, each with their own strengths and weaknesses. One involves asking whether individuals can calculate the true Bayesian probability given base rate probabilities and new sample evidence. This requires asking subjects for their judgment of the actual probability of event A occurring and, therefore, focuses on finding the "ideal" answer to a problem. Although this

[2] For example, psychological framing effects are shown to decrease in effortful thinking (McElroy and Seta, 2003).

approach has merit, our interest was in determining the weight placed on the odds and evidence when an actual forced choice was made (i.e., A was more likely to occur). Such a scenario is more aligned with many naturally-occurring decision environments in everyday life. For example, one may have to decide which of two routes to a destination is faster *right now* given the prior knowledge of the rate of traffic on each route and the new information of the current day and time. A surgeon may have to decide to perform an emergency procedure given prior knowledge of the relative success of the procedure and the new information of the current condition of the patient. In short, when individuals make real decisions, they must often choose a specific course of action (i.e., a dichotomous choice) rather than a probability estimate. It is the influence of sleep deprivation on making such decisions that is our interest.

Because information updating is a fundamental component of decision making under uncertainty, this research is relevant to a wide variety of behavioral applications. Sleep research has indirectly pointed towards failed information assimilation under sleep deprivation (e.g., increased hesitance and reduced focus among sleep-deprived junior doctors in Goldman et al, 1972, and increased stereotyping of responses in Harrison & Horne 1997, 1998). However, more direct evidence is needed, and Harrison and Horne (2000) recognize the lack of sleep deprivation research on specific decision models. As behavioral economics continues to explore decision-making, one cannot ignore the evidence indicating that many decision-makers are often sleep-deprived to some degree. Sleep loss effects on decision-making would also imply a potential confound in some experimental data sets: students employed as shift workers, or students during exam week, may include relatively more sleep-deprived subjects than other populations.[3]

METHODS

As noted, the experiments replicate the Grether (1980) design for a hand-run Bayes rule decision task. Two bingo cages are each filled with six colored balls: Cage A is filled with four green and two red balls, and Cage B is filled with three red and three green balls. Six draws, with replacement, were made from one of the cages behind an opaque divider. Each subject was informed of a "prior" probability of using Cage A in terms of a die roll. For example, a 1/3 prior odds of Cage A was implemented by selecting Cage A if the die roll was 1-2 (3-6 implied use of Cage B). Subjects did not see the actual die roll but were shown each ball drawn, and after six draws they were asked to indicate whether the balls came from Cage A or B.

It is important to note that we do not vary the new evidence sample size in our design, rather just the strength of the evidence in favor of Cage A. Griffin and Tversky (1992) show that, though both strength of evidence *and* sample size contribute to the likelihood ratio for the event in question, subjects place more weight on the sample proportion in favor of a particular outcome. Our results, therefore, do not speak to subject weighting on new evidence in general, but rather new evidence as represented by

[3] A small amount economics research has examined sleep. Biddle and Hammermesh (1990) incorporate labor productivity effects of sleep in a theoretical model of time allocation. Their empirical results from a variety of sources lead them to conclude that increased wages reduce sleep (more so for men than women), while increasing waking leisure time, as opposed to increasing hours of work. Their results are consistent with the aggregate evidence on sleep reduction in many industrialized countries with rising wages, and they imply that sleep deprivation may be an inevitable byproduct of wage growth in a society. Kamstra et al. (2000) examine daylight saving time changes on financial market returns, and they show that returns drop both after losing an hour (Spring) *and* gaining an hour (Fall). This suggests that minor disruptions of one's internal (biological) circadian rhythm can affect behavior and decisions, independent of sleep loss.

sample proportion in our design.[4] It would, however, be interesting to explore the weight of evidence through sample size as another manipulation for future research. Importantly, subjects in our design were *not* required to memorize the sample drawn, which would confound our task with short-term memory skills. A correct (incorrect) cage response resulted in payment of $12 ($2).

Each round or trial—choose the cage, draw the sample, indicate which cage was used—was repeated six times, with one well-rested and one sleep-deprivation trial randomly selected for payment after the final Bayes rule experiment—subjects did not know their accuracy or winnings until after all decisions were made. The design was balanced across prior A odds of 1/3, 1/2, and 2/3, which occurred in a random order chosen for each subject.[5] Because an accurate cage choice pays more, it is incentive compatible to indicate Cage A if one's subjective (posterior) probability of Cage A is greater than 50%. A Bayesian subject will equally consider both the prior odds and sample evidence in making choices.

SUBJECTS

A total of 24 subjects, were administered the Bayes rules experiment as part of their participation in a total sleep deprivation study, which involved a stay of several consecutive nights and days in the Laboratory for Sleep and Chronobiology at the University of California-San Diego.[6] These 24 subjects ranged in age from 18 and 39

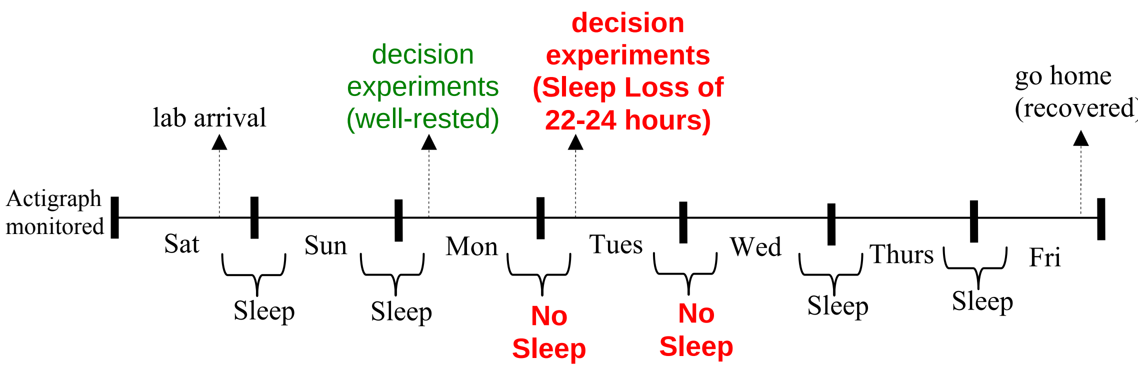
[4] The authors thank an anonymous reviewer for highlighting this point.
[5] One implementation inadvertently utilized one instance each of the prior odds of 1/6 and 5/6.
[6] Though the sample size is small, multiple subject trials create a panel of N=144 well-rested and N=144 sleep-deprived observations. A small number of total subjects is quite common in sleep-deprivation studies, because of the screening criteria, the requirement that subjects stay in the sleep lab several days, and the total compensation per subject for a total sleep-deprivation experiment (often several hundred dollars per subject).

years of age (μ=23.83, σ=5.37), Subjects were compensated a flat fee for participation in the sleep study, but it was made clear that these experiments afforded the opportunity to earn extra cash payoffs based on the experiment outcomes. Testing on various cognitive dimensions occurred approximately every two hours during their lab stay. Each subject completed the basic 30-minute (6-trial) Bayes rule experiment twice; once in a well-rested state, and once after 22-24 hours of total sleep deprivation. Both administrations of the task occurred during morning hours for all subjects, so that there is no confound between sleep loss and natural circadian sleep-wake cycles. The total number of observations is N=288 (N=144 well-rested and N=144 following sleep deprivation).

Screening criteria allowed right-handed, healthy, and "normal" sleeper subjects—those with consistent sleep-wake schedules to include 7-9 hours in bed each night. Subjects are indirectly monitored for one week prior to reporting to the sleep lab by keeping a sleep journal and wearing an actigraph.[7] During this week, subjects are required to keep normal sleep-wake routines and refrain from use of stimulants for 72 hours prior to reporting to the lab. In short, all subjects (including Control subjects, discussed later in this section) enter the lab in a similar well-rested state. During the total sleep deprivation treatment, subjects were not allowed any sleep, not allowed stimulants of any sort, and they were under constant supervision by lab staff to ensure this. Figure 1 describes the basic timeline of the subjects' lab stay relative to their participation in these decision experiments.

[7] The actigraph measures wrist movement as a proxy of gross motor activity. This movement, in turn, is used to determine sleep and wake. These data verify that subjects are engaged in normal sleep patterns prior to their lab stay and are not partially sleep deprived at the beginning of the experiment. The complete list of experimental inclusion/exclusion criteria is fairly standard for sleep deprivation research, and they are available on request.

FIGURE 1
A week in the sleep lab: time-line



Note: Some subjects stayed in the lab one less day and participated in a one-night sleep deprivation study. Our examination of sleep loss effects after one night of sleep deprivation allowed us to combine subjects from different sleep studies, whether or not they participated in a one or two night study.

EXPERIMENT DESIGN ISSUES

In a more recent paper, Grether (1992) notes that there are limitations to his simpler 1980 design. The dichotomous choice of Cage A or B does not allow us to infer strength of belief (i.e., 55% versus 95% certain that the balls came from Cage A), though this is possible using a rather complicated incentive compatible probability elicitation procedure (see Grether, 1992). As noted earlier, the dichotomous choice environment more closely mirrors naturally-occurring decision environments. Additionally, the more simple dichotomous choice design is easier to understand, which is important given that subjects complete one of the trials following sleep deprivation. On the other hand, the dichotomous choice environment implies that simple decision heuristics may be available, which could possibly confound an examination of Bayesian updating. We present data and analysis, however, that strongly support the conclusion that subjects weight *both* prior odds and new evidence in making their decisions.

Because of the existing sleep protocol, subjects always performed the task first well-rested and then following sleep deprivation. Given the potential learning confound in our main data, we also recruited an additional 12 control subjects (mean age μ=24.12, σ=4.28) who performed the Bayes rule task twice (N=144 total observations), at approximately 22-24 hours apart on consecutive mornings, but they were well-rested both times. Decision model estimates for the control subjects show *no* significant differences across the two administrations of the task—contrary to the main finding in the sleep deprivation data. In other words, we find no evidence that the differences in decision-making we report in the next section are due to subject learning. Additionally, if subjects learned, choice accuracy would be higher in the second Bayes rule experiment, but it is not. Or, learning might imply that a particular empirical model should better fit the data as choices converge to a particular set of model parameters—Grether (1980) finds this for experienced subjects, for example. Our results also show that this is not the case. We are therefore confident in attributing the second-trial effects to sleep deprivation.

RESULTS

Table 1 shows the aggregate data in terms of the proportion of overall subject choices of Cage A relative to the total observations for a particular combination of prior odds, P_A, and evidence. For comparison, the Bayesian posterior probabilities—those calculated by Bayes rule—are included in parenthesis in each instance. At this point, the aggregate data offer the best estimate of overall "strength of belief" for our pooled data, given that each individual subject makes a simple dichotomous choice. A quick scan of Table 1 clearly highlights that, holding P_A constant, the proportion of subject choices of

Cage A increases as the evidence favors Cage A (i.e., as more green balls are drawn in the sample evidence). It is also the case that, holding the evidence constant, the proportion of choices of Cage A rises with P_A. Both of these observations are true for the well-rested and sleep-deprived subsamples. In short, the evidence is supportive of the hypothesis that subjects care about both prior odds and evidence in forming belief. We next turn to our decision model estimates to examine relative weights placed on each source of information.

TABLE 1: Proportion of Cage A choices as a fraction of total observations (Bayesian probabilities in parenthesis)

Evidence		**Well-rested**			**Total Sleep Deprivation**		
Green	**Red**	**P_A=.33**	**P_A=.50**	**P_A=.67**	**P_A=.33**	**P_A=.50**	**P_A=.67**
0	6	--- (.04)	--- (.08)	--- (.15)	--- (.04)	--- (.08)	--- (.15)
1	5	**0/6** (.08)	**0/3** (.15)	--- (.26)	**1/6** (.08)	**1/5** (.15)	**0/3** (.26)
2	4	**0/7** (.15)	**0/9** (.26)	**1/2** (.41)	**1/5** (.15)	**0/7** (.26)	**4/9** (.41)
3	3	**0/13** (.26)	**3/15** (.41)	**9/13** (.58)	**2/15** (.26)	**1/15** (.41)	**6/11** (.58)
4	2	**9/17** (.41)	**10/13** (.58)	**20/21** (.73)	**3/14** (.41)	**8/9** (.58)	**12/14** (.73)
5	1	**3/3** (.58)	**7/7** (.73)	**8/8** (.85)	**3/4** (.58)	**8/9** (.73)	**8/8** (.85)
6	0	--- (.74)	**1/1** (.85)	**5/5** (.92)	**2/2** (.74)	**5/6** (.85)	**2/2** (.92)

Cage A=4 Green, 2 Red Balls Cage B=3 Green, 3 Red Balls

The decision model estimates we report account for the potential non-independence of decisions of a given subject across trials as a subject-specific random effect, but our results are robust to error-term specification. Here, as in Grether (1980), we estimate the following decision model:

$$(1) \qquad Y_{it}^{*} = \alpha + \beta_1 \ln LR(A)_t + \beta_2 \ln\left(\frac{P_A}{1-P_A}\right)_t + \mu_i + \varepsilon_{it}$$

where Y_{it}^{*} is the subject i's subjective log odds in favor of Cage A in trial t, $LR(A)_t$ is the likelihood ratio (evidence) for Cage A, and $\left(\frac{P_A}{1-P_A}\right)_t$ is the prior odds ratio for Cage A.

The dichotomous variable Y_{it} is observed equal to 1 if $Y_{it}^* \geq 0$, and so we estimate (1) using a random effects probit technique. Grether (1980) estimates logit results for this model, without accounting for subject-specific random effects, and so our econometric specifications are similar but not identical. The Bayes rule hypothesis is that α=0, and $\beta_1=\beta_2>0$, while overweighting the evidence implies $\beta_1>\beta_2\geq 0$. Grether (1980) found that, for most groups, subjects overweight the evidence relative to the prior odds.

To evaluate the effects of sleep deprivation (SD), we estimate the decision model with a dummy variable SD=0,1 and interaction terms allowing for SD-specific effects on either prior odds and/or new evidence weighting. Specifically, we estimate:

$$
\begin{aligned}
(2)\quad & Y_{it}^* = \alpha + \beta_1 \ln LR(A)_t + \beta_2 \ln\left(\frac{P_A}{1-P_A}\right)_t \\
& + \beta_3 * SD_{it} + \beta_4(\ln LR(A)_t * SD_{it}) + \beta_5\left(\ln\left(\frac{P_A}{1-P_A}\right)_t * SD_{it}\right) + \mu_i + \varepsilon_{it}
\end{aligned}
$$

And finally, to evaluate the potential learning confound, we estimate a model similar to (2) for the control data, except with a dummy variable and interaction terms to account for the second administration of the task. That is, the control subject model estimated is:

$$
\begin{aligned}
(3)\quad & Y_{it}^* = \alpha + \beta_1 \ln LR(A)_t + \beta_2 \ln\left(\frac{P_A}{1-P_A}\right)_t + \beta_3 * \text{2ndAdmin}_{it} + \\
& \beta_4(\ln LR(A)_t * \text{2ndAdmin}_{it}) + \beta_5\left(\ln\left(\frac{P_A}{1-P_A}\right)_t * \text{2ndAdmin}_{it}\right) + \mu_i + \varepsilon_{it}
\end{aligned}
$$

The estimation results of models (1), (2), and (3), are shown in Table 2, and the models are all reasonably accurate at predicting subject choices. The consistency across models is that both prior odds and new evidence are significant predictors of Cage A

TABLE 2: Probit estimates of $\boldsymbol{Y_{it}^{*}}$ models (1), (2), and (3)
(random effects specification. p-values given in parenthesis)

Coefficient Estimated	Model (1) Main data (N=288) Coeff.	marg. effect	Model (2) Main data (N=288) Coeff.	marg. Effect	Model (3) Control subjects (N=144) Coeff.	Marg. Effect
Constant	.03 (.83)	.01 (.83)	.14 (.61)	.051 (.61)	-.01 (.97)	-.004 (.96)
β_1 **(evidence)**	1.27 (.00)***	.48 (.00)***	2.41 (.00)***	.90 (.00)***	1.40 (.00)***	.55 (.00)***
β_2 **(prior odds)**	1.10 (.00)***	.42 (.00)***	1.53 (.00)***	.57 (.00)***	1.51 (.00)***	.60 (.00)***
β_3 **(SD/2nd day)**	--	--	-.21 (.48)	-.08 (.48)	-.77 (.30)	-.30 (.30)
β_4 **(SD/2nd day*evidence)**	--	--	-1.43 (.00)***	-.53 (.00)***	.69 (.22)	.27 (.22)
β_5 **(SD/2nd day*prior odds)**	--	--	-.59 (.24)	-.22 (.24)	.03 (.96)	.01 (.97)
% correctly predicted by model	84.38%		84.38%		85.42%	

choice. Model (1) is most similar to the Grether (1980) estimations[8], though the tendency in the pooled data (i.e., both well-rested and sleep-deprived sessions) to overweight the evidence is not statistically significant.[9] Model (2) offers just one way to estimate the effects of sleep deprivation on the decision weight placed on prior odds versus new evidence, but our results are robust to different estimation approaches.

[8] Grether (1980) finds that financially rewarded subjects typically overweight new evidence, which he attributes to use of a "representativeness" heuristic. However, in his more general (1992) design, when the heuristic is not as available, this overweighting of new evidence is not borne out as a more general result.
[9] For comparison to Grether's (1980) *logit* estimations, we also perform a logit estimation of the model similar to (1) above, but without the random effects error-term specification. The pooled results that Grether reports for his financially motivated subjects yield the estimated model Y_{it}= -.11+2.25*lnLR(A)$_{it}$+1.82*$P_A/(1-P_A)_{it}$, where α, β_1, and β_2 are statistically significant. In estimating the same logit model for our pooled data, the results are Y_{it}= .04+2.26*lnLR(A)$_{it}$+1.95*$P_A/(1-P_A)_{it}$, with β_1 and β_2 being statistically significant (p=.00). So, our results are quite comparable to those reported in Grether (1980), and logit estimations of any of the models in this section are consistent with the results we find in the probit estimations that we report. The results we find are also similar for a fixed effects specification (logit and/or fixed effects estimation results available from the authors on request).

The estimation of model (2) highlights the key result that, following sleep loss, the decision weight placed on the new evidence is significantly reduced (see shaded cells). The results from Model (3) do not show a similar effect of the second-day session in the control subjects, thus indicating that our key result is not caused by learning or ordering of the tasks. We arrive at the same basic conclusion if we directly test this by pooling all data (i.e., experimental and control data) and including dummy variables and interactions terms to examine whether our key main finding is robust. To do this, we create a dummy variable for *Control* subjects and for the *Session* (=1 for second session). Note that *Session=1* and *Control =0* implies a sleep-deprived subject, and estimate the following model:

$$
\begin{aligned}
&Y_{it}^{*} = \alpha + \beta_1 \ln LR(A) + \beta_2 \ln\left(\frac{P_A}{1-P_A}\right) + \beta_3 * Session + \beta_4 Control + \\
(4)\qquad &\beta_5(\ln LR(A) * Session) + \beta_6(\ln LR(A) * Control) + \beta_7(Session * Control) \\
&+ \beta_8(\ln LR(A) * Session * Control) + \mu_i + \varepsilon_{it}
\end{aligned}
$$

Table 3: Summary results from pooled data (experimental and Control)

Parameter	α	β_1	β_2	β_3	β_4	β_5	β_6	β_7	β_8
***Marginal* effect**	.05	**.84**	**.48**	-.07	-.05	**-.46**	**-.34**	-.07	**.73**
p-value (two-tailed test)	.61	**.00**	**.00**	.51	.78	**.01**	**.04**	.64	**.02**

As can be seen in Table 3 results, both the evidence and the prior odds matter in the expected way, with subjects overweighting the evidence (p=.01 for the X^2 test of $\beta_1=\beta_2$) in making their Cage A choice. Control subjects weight the evidence less than the main experiment subjects ($\beta_6<0$). Most importantly, the interaction between *lnLR(A)*Session*Control)* indicates that control subject *increase* the weight they place on the evidence in the second session (β_8>0). Because the general tendency is to reduce weight on the evidence in the second session (β_5<0), this indicates a fundamental difference in the second session effect for Control subjects versus main experiment subjects (for whom session two means "sleep-deprived"). In fact, these results indicate there may be a general trend to increase the weight placed on evidence when one repeats the task a day later, but sleep loss reverses that tendency. This supports our claim that the reduction in estimated decision weight on new evidence following sleep deprivation in Table 2 is *not* driven by the ordering of our treatments.

Our second result is quite intriguing. Though the estimated decision model significantly differs following sleep loss, choice accuracy is maintained. Whether well-rested or sleep-deprived, subjects indicated the correct cage 67-68% of the time.[10] To the extent that relevant new information is valuable, our Model (2) results in Table 2 indicate that accuracy might be expected to drop, in general, following sleep deprivation because the decision-weight on new evidence falls. However, well-rested subjects were overweighting the new evidence relative to efficient Bayesian updating (i.e., β_1>β_2 in Model 2, p=.03). Thus, the altered decision process following sleep deprivation reduces (non-Bayesian) hyper-focus on new information—this should, *ceteris paribus*, increase choice accuracy. That choice accuracy is not altered following total sleep deprivation may result from our simple dichotomous choice environment, and is therefore not a general result. A more sensitive outcome measure, such as probability estimates, may highlight interesting effects masked in our design. Nevertheless, our design recreates a

[10] Choices and accuracy are **not** consistent with random decisions. In the well-rested subsample, the actual Cage A frequency is 54.2%, and subjects chose Cage A 52.8% of the time (actual accuracy was 68.1%). In the sleep-deprived subsample, Cage A frequency was 43.8%, and Cage A choice occurred 46.5% of the time (67.4% accuracy).

dichotomous choice environment that is similar to many real-world environments where choice is among two courses of action.

Actual choice accuracy may be biased if the more likely Cage, based on Bayesian updated probabilities, is often not the actual Cage (just by random chance). However, further examination of subject choices indicate that they coincide with the more likely Bayesian event 85% and 84% of the time when subjects are well-rested and sleep-deprived, respectively.[11] We also note that our second result (i.e., maintained choice accuracy following sleep loss) implies equal accuracy only in assessing the likelihood of being in state A versus state B. In many cases, an individual cannot choose the decision environment, and so it is important to note that our results do not imply that all decisions, in general, are resilient to short-term sleep loss.

DISCUSSION

Our results are significant in today's modern sleep-deprived society. Existing research has not thoroughly examined the effects of sleep deprivation on decision-making, and the present results on Bayesian updating suggest certain components of decision-making are resilient to at least some level of sleep loss. That is, we find no evidence that roughly 24 hours of total sleep deprivation affects the quality of final choices in this binary choice environment, and these results do not appear to mask important individual subject differences in the data.[12]

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[11] The same is true of control subjects, in that subject choices coincide with the more likely Bayesian event 85% and 86% of the time for the first and second administrations of the task, respectively. These results further argue that learning is not the cause of our result, as the control data results are similar to the main data results in every way *except* for the effect of the second administration on decision weights.

[12] Choice accuracy is examined at the individual level, with roughly equal numbers of subjects being slightly more or less accurate on day one versus day two (for both main data and control data). Though the data are limited, there are some occurrences of the *exact* same statistical sample and prior odds for a subject

Of course, decision accuracy in this environment may deteriorate with longer bouts of sleep deprivation, but more extended periods of sleep loss would have less external relevance. For example, Van Dongen et al., (2003) show that some of the effects of 24 hours of sleep deprivation are replicated when subjects get 4-6 hours of sleep per night for up to a week (i.e., partial but chronic sleep deprivation), which may reflect more typical sleep loss. Decision quality may also suffer under more complex tasks, but caution must be exercised in any more complex task design so as to not confound the pure task of probability updating with other decision-making dimensions (e.g., short-term memory). Because total sleep deprivation has been shown to impair functioning in other areas (e.g., short-term memory, reaction time, motor function), our evidence suggests that the empirical data on increased accidents/errors due to sleep loss are not necessarily attributable to reduced abilities to integrate multiple pieces of information into a decision. This argues for additional research to more thoroughly examine this surprising finding.

Our experiment involves an unavoidable risky decision environment. Emerging evidence indicates that sleep deprivation may lead individuals to select, on average, more risky decision environments (McKenna et al., 2007). Though we find error *rates* to be unaffected by sleep deprivation (in our simple task), the cost of each error may be higher in a riskier scenario. In our related research, individuals seem desensitized to risk following approximately 24 hours of total sleep deprivation (McKenna et al., 2007), with preferences converging towards risk neutrality in both the payoff gains and loss domains. This has interesting implications for, among others, military personnel choosing to

both when well-rested and sleep-deprived. Examining these data, subject choices are quite stable (of the N=63 paired observations of this sort, in only 12 cases did subject choice change after sleep deprivation).

engage or not engage in a riskier outcome scenario, or a physician choosing between two courses of surgical action.

The finding of significant differences in estimated decision models for well-rested versus sleep-deprived subjects also merits further exploration. Our results are consistent with recent research that has found that underlying cognitive processes may be quite different following sleep deprivation even though task performance is unaffected (Drummond et al., 2000; Venkatraman et al., 2007). As such, the change in estimated decision model we find may be a first clue to the types of decision-related cognitive processes altered following sleep loss. For example, if the trend to decrease the weight placed on the evidence continues with longer bouts of sleep deprivation, this might suggest that individuals are eventually unable to integrate multiple pieces of information into a decision, instead relying on only a single variable and/or a stereo-typed response.

One might hypothesize that subjects run up against the constraints of bounded rationality following sleep deprivation, thus forcing a change in their methods of inference. For example, Gigerenzer and Goldstein (1996) showed that simplistic models of inference need not do worse than more complex algorithms. In may merely be a coincidence in our data that the estimated decision model following sleep deprivation appears Bayesian (i.e., equal weighting of prior odds and evidence). For example, subjects appear to place less weight on both evidence *and* prior odds following sleep deprivation, except that the estimated effect on prior odds is smaller in magnitude not statistically significant (p=.24, see Model (2) in Table 2). This may indicate that what appears Bayesian may actually be an artifact of subjects weighting all sources of information to (varied) lesser degrees following sleep loss. While our studied was not designed to discriminate between use of simple heuristics, it would be useful to explore heuristics further in a distinct sleep study.

Decreased flexibility in responding to external stimuli during sleep deprivation has been reported by other authors (Harrison and Horne, 1998 and 1999). A possible related explanation for our results might be that subjects put less effort into calculating the actual probability of a given outcome during sleep deprivation and instead rely on approximating the answer (Drummond et al., 1999). This could be similar to verbatim decisions and gist decisions in Fuzzy Trace Theory (Reynanerd, 1991) and would be consistent with the finding of less weight being placed on both the prior odds and the sample evidence in the decision model following sleep deprivation. Finally, the effects of sleep deprivation reported here may result more from an impairment in the ability to integrate information during sleep loss, rather than specifically related to decision making per se. While the process of integrating information has not been well studied during sleep deprivation, several studies report deficits in the ability to maintain and manipulate information in working memory during sleep deprivation (Bartel, et al., 2004; Chee et al., 2006; Malemed and Oksenberg, 2002; Smigh et al., 2002; Turner et al., 2007), and this would be expected to impair one's ability to integrate multiple sources of information during decisions.

Evidence from behavioral neuroscience studies indicate that biological processes are altered following sleep deprivation. For example, Drummond et al. (2000) studied behavioral and neural outcomes in free recall memory tasks. Though behavioral outcomes showed no significant change, neural responses following sleep deprivation were significantly different. Such results are consistent with a hypothesis of “compensatory recruitment”, whereby distinct brain regions may be recruited to compensate for the adverse condition of sleep loss. Others have reported similar increases in brain activation and intact performance during sleep deprivation on a variety of tasks (Drummond et al, 2001, 2004, 2005; Portas et al, 1998; Chee & Choo, 2004), and Stricker et al (2006) have reported changes in the neural networks that perform a given task after sleep deprivation. Hsu et al., (2005) examined decision-making under uncertainty in a neuroeconomics experiment, and they suggest a multi-regional neural system for evaluating uncertainty.

Though we examine only behavioral outcomes in this paper, the evidence we find may be a clue indicating neural activation differences in information-updating environments. For example, the ventrolateral prefrontal cortex (VLPC) has been implicated in the neural process of integrating new contingencies (Paulus, et al, 2004) while the dorsolateral prefrontal cortex (DLPFC) plays a major role in integrating multiple pieces of information (Cabeza & Nyberg, 2000; Cohen et al, 1997). Thus, we might hypothesize decreased activation of the VLPC and/or DLPFC following sleep deprivation. Existing sleep research suggests that compensatory activation may occur in the parietal lobes following sleep loss, thus maintaining performance (e.g., Drummond et al., 2000, 2001, 2005). While this is suggestive, further research is certainly necessary, because it highlights that our understanding of judgments and decision-making following sleep deprivation is incomplete at best.

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