

Circadian effects on strategic reasoning

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Abstract The ability to strategically reason is important in many competitive environments. In this paper, we examine how relatively mild temporal variations in cognition affect reasoning in the Beauty Contest. The source of temporal cognition variation that we explore is the time-of-day that decisions are made. Our first result is that circadian mismatched subjects (i.e., those making decisions at off-peak time of day) display lower levels of strategic reasoning in the $p < 1$ Beauty Contest but not in the $p > 1$ game. This suggests that a cognitively more challenging environment is required for circadian mismatch to harm strategic reasoning. A second result is that choice adaptation or mimicry (i.e., a more automatic type of responding than what is typically considered to be “learning”) during repeated play is not significantly affected by circadian mismatch. This is consistent with the hypothesis that automatic thought is more resilient to cognitive resource depletion than controlled-thought decision making.

Keywords Guessing game · Sleep · Circadian mismatch · Experiments · Rationality

JEL Classification C92 · C70 · D83

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1 Introduction

The Beauty Contest game has garnered significant attention because of how it captures important features of strategic reasoning that are building blocks for decision making outside the lab.¹ Recent research documents neural correlates of Beauty Contest decisions in the medial prefrontal cortex, which has been implicated in the key cognitive process of mentalizing or anticipation of others' actions (Coricelli and Nagel 2009, and references therein). Burks et al. (2009) show that a key determinant of the between-subject heterogeneity in similar strategic reasoning environments appears to be one's trait-level cognitive skills. In this paper, we consider that *state*-level cognition, which can exhibit short-run temporal variations, may also impact strategic reasoning, and so this study helps fill an important gap in our understanding of cognition and strategic reasoning in economic decision making. If even a temporary depletion of cognitive resources harms the ability to anticipate others' actions, this puts one at a strategic disadvantage and higher risk of failure in competitive decision environments.

Specifically, we consider circadian mismatch—decision making at an off-peak time-of-day relative to one's diurnal preference of mornings versus evenings—as a pervasive modern societal example of temporal variation in one's cognitive resources. We implement a randomized circadian mismatch manipulation on 102 subjects to explore the hypothesis that this common real-world cause of temporal variations in cognition can harm one's ability to strategically reason. The Beauty Contest we implement is repeated, and the information feedback given after the initial round allows one to mimic others' choices (see Lakin et al. 2008)²—this is not possible when the decision environment is novel, such as a new game or new parameterization of the game. Thus, the repeated Beauty Contest allows us to explore the effects of circadian mismatch on both strategic reasoning during initial task performance and also on more automatic response processes engaged when a task is no longer novel and mimicry is possible. This is an important distinction in our hypotheses given that researchers have suggested that automatic thought processes are less affected by cognitive resources depletion than controlled thought processes (Ferreira et al. 2006; Posner and Snyder 1975). In other words, certain decision domains may be less susceptible to temporal variations in cognition, depending on how much they require controlled thought processes.

Our study is highly relevant in a world where most individuals fall prey to modern scheduling demands that require decision making at various times of the day. Recent

¹Among the examples of Beauty Contests are: pure coordination environments (Keynes 1936), stock market behavior (Ho et al. 1998), threat assessment, driving commute route choice. Behavioral researchers have evaluated such games experimentally (Nagel 1995; Stahl 1996; Duffy and Nagel 1997; Bosch-Domenech et al. 2002; Weber 2003; Costa-Gomes and Crawford 2006; Grosskopf and Nagel 2008), and generally conclude that individuals are strategic but not infinitely rational as assumed by economic theory. Results are consistent with bounded rationality.

²This is not a general comment on whether learning requires deliberate thought or not. Complex learning surely engages rational thought processes, as in Hampton et al. (2008). However, the “learning” of the sort engaged in $n > 2$ Beauty Contests with full information feedback is likely more simple. In fact, the simple mimicry of others' choices is not what most economists would consider “learning”. Thus, we avoid such terminology in our paper.

U.S. data also show that over 21 million wage and salary workers annually ($\approx 18\%$) performed some type of shift work, which implies that performance and high-level thinking at off-peak circadian hours is not uncommon. Furthermore, a disproportionate percentage of shift work is present in occupations where decision outcomes have significant safety implications, such as health care support (27.9%), protective services (50.4%—includes police and firefighters), and transportation or material moving (29%) (McMenamin 2007). Though our study focuses on circadian mismatch as the cause of depleted cognitive resources, we believe it sheds light on the more hidden decision consequences of temporal variations in state-level cognition, in general.

2 Circadian effects on cognition and decisions

The circadian rhythm determines a daily cyclical variation in sleepiness/alertness that can differ across individuals. For example, an evening-type individual (a.k.a., an “owl”) is typically quite sleepy at 8 a.m., while this is a point of relative alertness for a morning-type (a “lark”). Researchers have found that circadian mismatch (i.e., performance at one’s non-preferred time-of-day) negatively affects recall memory, subjective alertness, visual attention, and reaction times (Wright et al. 2002; Horowitz et al. 2003). Others have shown performance dips among shift workers (Bjerner et al. 1955) and driver safety (Coren 1996a) at circadian off-peak times-of-day. Such findings should raise concerns in an increasingly 24/7 society, and several major disasters in recent history have implicated sleepiness among their causes (e.g., The Exxon Valdez oil spill, the Three Mile Island nuclear reactor incident, the Chernobyl nuclear explosion—Coren 1996a).³

Higher-level “controlled” thought processes are concentrated in the prefrontal cortex (PFC) brain region, which is known to be affected by sleep loss (Horne 1993). We hypothesize that circadian mismatch should also affect the PFC given that it can contribute to sleepiness independent of sleep deprivation. Coricelli and Nagel (2009) document that prefrontal brain regions are more active when a subject submits a Beauty Contest decision closer to the rational equilibrium prediction. Thus, because we believe that circadian mismatch may disproportionately affect the PFC areas responsible for anticipatory thought, our key behavioral hypothesis is that circadian mismatch will produce inferior Beauty Contest outcomes when strategic reasoning is engaged.

3 Experiment design and procedures

A total of 102 subjects (46 females) participated in a two-session experiment. Sessions were set one week apart and both took place at a common randomly assigned

³Clearly, sleep deprivation must be mentioned in any discussion of the effects of sleepiness on decisions. Reduced cognitive performance (Van Dongen et al. 2003; Belenky et al. 2003), increased accident rates (Coren 1996a, 1996b), and decision error (Baldwin and Daugherty 2004; Weinger and Ancoli-Israel 2002) have all been attributed to sleep loss. Though we do not manipulate sleep levels in our experiments, we discuss in the Experiment Design section our use of objective controls regarding sleep levels of our subjects.

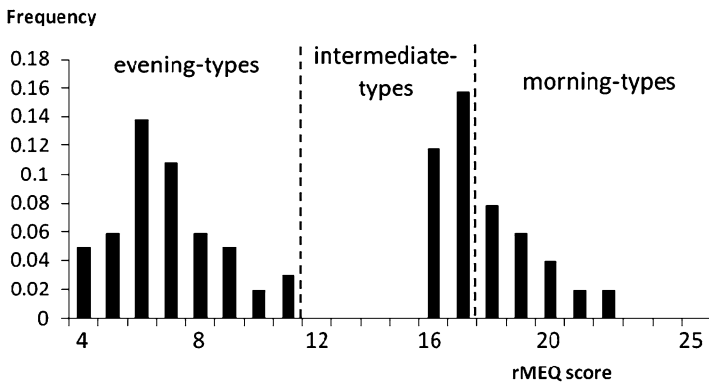


Fig. 1 Distributions of subject recruitment (total $N = 102$)

morning (8 a.m.) or evening (8 p.m.) time of day, depending on the experiment group. The subjects were recruited from a database of several hundred subjects who had completed a short sleep survey. Exclusion criterion included: self-reported diagnosed sleep disorder or objectively scored high risk for major depressive or anxiety disorder (as ascertained from validated survey questions). The sleep survey also included a validated short-form diurnal preference survey instrument (Adan and Almiral 1991), which was a reduced form of the morningness-eveningness survey instrument in Horne and Östberg (1976). This instrument, henceforth rMEQ, classifies respondents as follows: 4–11 Evening types, 12–17 Intermediate types, 18–25 Morning types. Due to the rarity of true morning types (less than 10% in similar young adult populations—Chelminski et al. 2000), we extended our recruitment cutoff to include rMEQ scores of 16 and 17. Thus, most intermediate-types were screened out, but not all. The main objective was a clear rMEQ score separation between groups (see Fig. 1).

From this database of eligible morning-type (MT) and evening-type (ET) subjects, all are ex ante randomly assigned to a morning or evening experiment session, and then recruited for the main experiment phase. Thus, our protocol randomly assigns each subject to be matched or mismatched to his/her more optimal time of day for the decision experiment session. Individuals could choose to not respond to the recruitment message, but they could not self-select into the alternative time slot by declining their randomly assigned session time slot. Table 1 shows relevant demographic and sleep statistics on the study subjects within each treatment cell of the 2×2 circadian match/mismatch manipulation. Evening-types in our sample generally show a 1.5–2 hour delay in bed and wake times compared to morning-types, as we should expect.

The two stages of the main experiment phase were as follows: Session 1 occurred on a Tuesday, Wednesday, or Thursday (morning or evening session). Session 2 was the Beauty Contest session that took place one week later on the same day of the week as Session 1. Midweek days were used to avoid weekend sleep effects that might confound the circadian manipulation. In Session 1 subjects gave voluntary informed consent, completed 3 short survey instruments, and were given instructions on the use of a wrist-worn acceleromometer device (i.e., actigraphy) for sleep data acquisition and

Table 1 Chronotype and time session assignments. (Summary sleep statistics shown were actigraphy-generated—see [Appendix](#))

Chronotype	Time session for decision experiment Summary statistics (<i>N</i> , averages, standard deviations)	
	8:00 a.m. (morning)	8:00 p.m. (evening)
Morning type	<i>N</i> = 24 (15 female)	<i>N</i> = 26 (5 female)
	Age = 23.9 ± 9.4 years old	Age = 26.9 ± 12.1 years old
	Avg. rMEQ score = 17.42 ± 1.5	Avg. rMEQ score = 18.07 ± 1.8
	Avg. nightly sleep = 374 ± 65 min	Avg. nightly sleep = 379 ± 61 min
	Avg. wake time = 8 : 12 a.m. ± 73 min	Avg. wake time = 7 : 37 a.m. ± 60 min
	Avg. bed time = 12 : 21 a.m. ± 73 min	Avg. bed time = 12 : 03 a.m. ± 77 min
	Avg. sleep efficiency* = 82% ± 5.0%	Avg. sleep efficiency* = 80% ± 6.7%
Evening type	<i>N</i> = 27 (10 female)	<i>N</i> = 25 (16 female)
	Age = 20.7 ± 2.5 years old	Age = 21.4 ± 5.4 years old
	Avg. rMEQ score = 7.15 ± 2.2	Avg. rMEQ score = 6.56 ± 1.32
	Avg. nightly sleep = 389 ± 49 min	Avg. nightly sleep = 380 ± 64 min
	Avg. wake time = 10 : 04 a.m. ± 72 min	Avg. wake time = 9 : 59 a.m. ± 94 min
	Avg. bed time = 1 : 46 a.m. ± 66 min	Avg. bed time = 2 : 03 a.m. ± 61 min
	Avg. sleep efficiency* = 81% ± 7.1%	Avg. sleep efficiency* = 84% ± 4.8%

*Sleep efficiency is the percentage of the time within the subjects nightly attempted sleep intervals that is scored as actual sleep. That is, sleep efficiency takes into the time taken to fall asleep as well as any bouts of wakefulness during the night as measured by the actigraphy

Note: ± indicates standard deviation

complementary sleep diaries. Subjects then return one week later for Session 2, having worn the actigraphy and kept sleep journals for the week. Importantly, subjects were instructed to carry out their typical sleep routine, such that our sleep quantity measure is both objective and ecologically valid. At the time of decision-making in Session 2, we therefore have an objective measure of sleep during the week leading up to the administration of the Beauty Contest. Sleep levels are included as a control in our econometric analysis of circadian mismatch effects but, because sleep levels are not manipulated in our design, it is possible that they reflect some other omitted variable correlated with sleep levels (e.g., short-term stress).⁴ Thus, we cannot make any proper conclusions regarding the effects of sleep levels on Beauty Contest outcomes from this design.

During Session 2 we administer two treatments of the Beauty Contest following Ho et al. (1998). Treatment 1 has parameterization $p = 0.7$ and guess interval $[0, 100]$, whereas a second treatment used $p = 1.3$, and guess interval $[100, 200]$.

⁴We thank a thoughtful reviewer for pointing out that our objective measure of sleep level may be capturing some other omitted variable effects. Though we screen out subjects at risk of major depressive or anxiety disorders, it is possible that other short-term factors may be responsible for variations in sleep across subjects.

Both Treatments 1 and 2 were administered as a block of 10 rounds with full information feedback for a total of 20 decision rounds per experiment. Some experiment groups were first administered Treatment 1, whereas others were first administered Treatment 2. Subjects were compensated a fixed \$30 for the 7 days of actigraphy and sleep diary data. The Beauty Contest also included a prize for the winning guess in each round (prize shared in the event of a tie guess). Payoffs averaged a total of \$52.55 for each subject: \$30 for the week of actigraphy and sleep diary data, and then $\$22.55 \pm \8.84 (standard deviation) from the Beauty Contest decisions.

Strategic reasoning in the Beauty Contest In Treatment 1 of the Beauty Contest the equilibrium guess is zero, but it is only reached by *infinite* elimination of dominated strategies (i.e., infinite depth of strategic reasoning). Deviations from equilibrium are typically used as evidence of some cognitive limit to rationality. Experimental results have shown finite reasoning in this environment consistent with a cognitive hierarchy model (Nagel 1995; Ho et al. 1998; Coricelli and Nagel 2009; Bosch-Domenech et al. 2002), and additional evidence favoring bounded rationality is found in two-person Beauty Contests where most choices involve dominated strategies (Grosskopf and Nagel 2008). Importantly, in the Treatment 2 parameterization, equilibrium is reached at *finite* depth of reasoning at the upper bound guess of 200. As such, we consider Treatment 2 to be more cognitively simple than Treatment 1. Treatment 1 may therefore provide a sharper test of our primary hypothesis.

As noted before, uninformed play of the Beauty Contest engages higher-level executive thought processes (Coricelli and Nagel 2009). Repeated play with feedback renders the game less novel and subjects should rely on more automatic decision processes (Shiffrin and Schneider 1977).⁵ Subjects can simply mimic winning guesses and respond more automatically. Thus, rounds 2–10 are not considered novel and should not engage more thoughtful/strategic reasoning. Existing research supports our contention that processing of a novel, unfamiliar task in the initial round should lead to more thoughtful reasoning, whereas a learned task, such as subsequent rounds of the Beauty Contest (for a given parameterization), should lead to more unconscious/automatic responses (Jacoby and Kelley 1987; Kahneman and Frederick 2002; Shiffrin and Schneider 1977; Schneider and Shiffrin 1977; Sloman 1996; Stanovich and West 1998).

In sum, two testable hypotheses follow from our design. These hypotheses result from the extant literature on Beauty Contests and the fact that task novelty should engage more deliberate thought regions, whereas non-novel tasks and simple adaptation (or mimicry) requires little deliberate thought and allow for more automatic responses.

Hypothesis 1 First-round guesses—when the task or parameterization is novel—will be farther from the equilibrium for circadian mismatched subjects.

⁵It is important to note that, while Coricelli and Nagel (2009) administered multiple rounds of the Beauty Contest, they provided no feedback between trials and they changed p for each trial. Thus, each and every round in their design would be considered a novel task that engaged strategic reasoning, whereas in our setting with feedback it is only the first round of a treatment that engages such reasoning.

Hypothesis 2 Adaptation during repeated play will be more resilient to circadian mismatch.

4 Results

4.1 Circadian mismatch and Hypothesis 1

Summary results for all rounds are shown in Figs. 2a (Treatment 1) and 2b (Treatment 2). Statistical tests reported below are one-tailed when our alternative hypothesis to the null is one-sided—otherwise two-tailed tests are used. Table 2 shows the results of nonparametric tests of mean differences between circadian matched (CM) and mismatched (MM) groups for the subset of initial treatment rounds ($n = 102$ for each treatment). Hypothesis 1 is confirmed in Treatment 1 for circadian states ($p = 0.02$). Circadian mismatch led to round 1 decisions consistent with lower

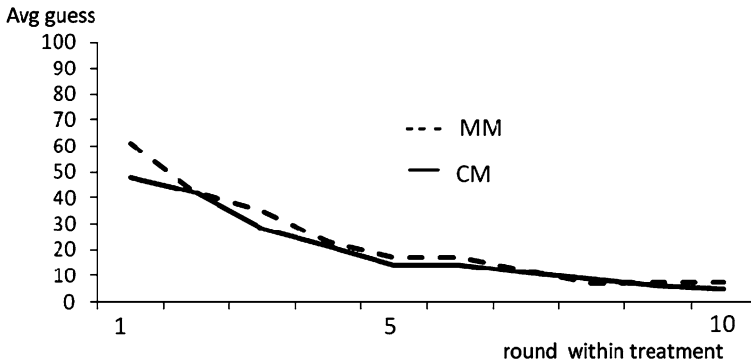


Fig. 2a Treatment 1—average guesses by circadian mismatch state (MM = circadian mismatched, CM = circadian matched)

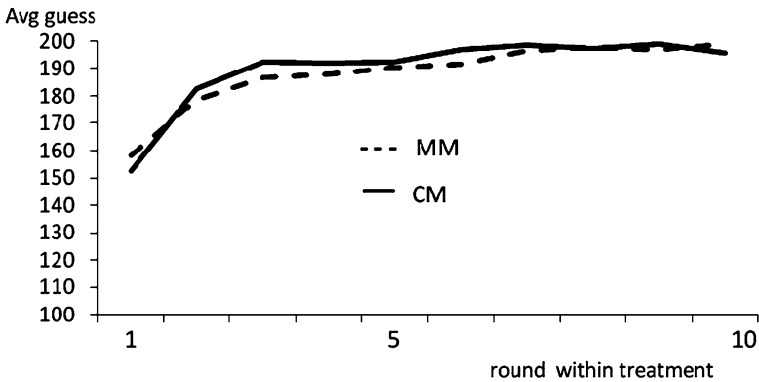


Fig. 2b Treatment 2—average guesses by circadian mismatch state (MM = circadian mismatched, CM = circadian matched)

Table 2 Initial guesses (first round of treatment is round 1 or 11)

		Mann-Whitney ranks-sum test (one-tailed tests)
Treatment 1		
Guess _{MM} = 61.15	Guess _{CM} = 48.01	Guess _{MM} > Guess _{CM} ($p = 0.02$)
Treatment 2		
Guess _{MM} = 157.98	Guess _{CM} = 152.25	Guess _{MM} = Guess _{CM} ($p > 0.10$)

Note: MM = circadian mismatched, CM = circadian matched

Table 3 Levels of iterated dominance by sleep and circadian state (data from 1st round of treatment)

Level of iterated dominance (guess interval)	Percentage reported	
	MM ($n = 53$)	CM ($n = 49$)
Treatment 1		
$R(0)$ [70, 100]	49.06	28.57
$R(1)$ [49, 69]	13.21	14.29
$R(2)$ [34, 48]	16.98	28.57
$R(3)$ [24, 33]	9.43	6.12
$> R(3)$ [0, 23]	11.32	20.41
Treatment 2*		
$R(0)$ [100, 130]	16.98	26.53
$R(1)$ [131, 169]	45.28	34.69
$R(2)$ [170, 200]	37.74	38.78

*In Treatment 2, a maximal guess of $x = 200$ implies level of iterated dominance $R(2)$, so we are unable to identify higher-level reasoning in Treatment 2

levels of strategic reasoning (see also Table 3). If we further examine the Treatment 1 circadian mismatch result by time of day, we find that initial guesses do *not* differ in the morning sessions when comparing morning-types and evening-types (Guess_{MM,8a.m.} = 57.15 \approx 58.63 = Guess_{CM,8a.m.}, $p > 0.10$, Mann-Whitney 1-tailed test). In other words, evening-types at 8 a.m.—a circadian mismatch—strategically reason similar to morning-types at 8 a.m.—a circadian match.

Thus, the Treatment 1 circadian mismatch result is due to evening decisions that are closer to equilibrium among evening-type subjects (Guess_{MM,8p.m.} = 65.31 > 37.81 = Guess_{CM,8p.m.}, $p = 0.00$, Mann-Whitney 1-tailed test). This result is perhaps surprising given that morning-types are still typically awake at 8 p.m. (compared to evening-types at 8 a.m.). We also find that evening-types at their optimal time of day make Beauty Contest guesses that are significantly closer to equilib-

rium than morning-types at their optimal time of day ($\text{Guess}_{\text{CM},8\text{p.m.}} = 37.81 < \text{Guess}_{\text{CM},8\text{a.m.}} = 58.63$, $p = 0.01$, Mann-Whitney 2-tailed test). Thus, circadian matched evening-types may simply be better at strategic reasoning than circadian matched morning-types, though more research is needed to further evaluate such a possibility. The sessions times we chose correspond to equal levels of expected alertness for circadian matched subjects (Smith et al. 2002), but strategic reasoning is apparently mediated by other concerns specific to diurnal preference.

In Treatment 2, we do not find support for Hypothesis 1. Recall that equilibrium in Treatment 2 is reached at finite depth of reasoning, and so we consider that strategic reasoning is not as difficult in Treatment 2. An additional breakdown of the distribution of initial rounds decisions is presented in Table 3, which categorizes decisions by level of depth of reasoning. From this unconditional nonparametric data analysis, we find adverse circadian mismatch effects on strategic reasoning in Treatment 1. This indicates that adverse time-of-day effects are more likely to appear in cognitively more complex decision environments. We next use multivariate regressions to allow for additional controls in the analysis.

4.2 Predicting strategic reasoning

We estimate several econometric models of decision outcomes as a function of one's circadian state. While an obvious outcome variable is the subject's *Guess*, we also evaluate two additional outcome measures to check for robustness of results. In one case, we define the dependent variable as the subject's rank in the guessing game (the winner has $\text{GuessRank} = 1$), and this variable has the same ordinal scale in both treatments. And finally, we also consider as a dependent variable the squared distance between a subject's guess and the winning guess for that round, *Dist-sq*, which is a cardinal outcome ranking. Higher levels of iterative reasoning correspond to lower (higher) *Guess* in Treatment 1 (Treatment 2), lower *GuessRank* in both treatments, and lower *Dist-sq* in both treatments.

For each model estimated, we include controls for gender, trait-level cognitive skills, time-of-day (as differentiated from circadian mismatch), and whether the decision is in round 1 or round 11 (i.e., the second treatment).

Consider the following model of a decision outcome measure for the i th subject ($N = 102$):

$$\begin{aligned} \text{Outcome}_i = & \alpha + \beta_1 * \text{Female}_i + \beta_2 * \text{NFC}_i + \beta_3 * \text{MisMatch}_i \\ & + \beta_6 * \text{MorningSession}_i + \beta_6 * \text{2ndTreatment}_i + \varepsilon. \end{aligned} \quad (1)$$

The two main subject-specific variables are gender (variable *Female*) and Need for Cognition (*NFC*) score. The *NFC* score is commonly used in the psychology literature as a measure of one's preference to engage in and enjoy thinking, and it does not interact with subject gender (Cacioppo and Petty 1982). We use *NFC* as a proxy for one's trait-level cognitive skills. As noted before in the Experiment Design section, we generated objective data on one's sleep level for the week prior to decision making and include controls in (1) for sleep levels. These controls include a linear and

Table 4 OLS estimation of Outcome measures (robust standard errors in parenthesis) first round of treatment only ($n = 102$)

Variable	Treatment 1			Treatment 2		
	Dep Var = <i>Guess</i>	Dep Var = <i>GuessRank</i>	Dep Var = <i>Dist-sq</i>	Dep Var = <i>Guess</i>	Dep Var = <i>GuessRank</i>	Dep Var = <i>Dist-sq</i>
Constant	354.78 (87.77)***	27.87 (7.50)***	10994.28 (3440.8)***	-20.36 (87.59)	19.15 (8.09)	19828.7 (9776.95)***
Female	13.73 (6.51)**	0.39 (0.49)	515.04 (214.03)**	-8.16 (5.15)	0.69 (0.55)	672.41 (7.75)
NFC	-0.10 (0.09)	-0.01 (0.007)*	-1.33 (4.14)	0.03 (0.09)	0.007 (0.007)	4.96 (7.75)
Mismatch	14.08 (5.60)**	0.98 (0.51)*	199.81 (232.62)	4.80 (5.60)	-0.18 (0.66)	-283.56 (605.70)
Morning session	5.84 (3.83)	-0.31 (0.54)	179.28 (300.94)	-4.50 (2.90)	0.11 (0.35)	-436.77 (426.47)
2nd treatment of session	12.95 (3.83)***	-0.28 (0.55)	253.91 (273.94)	3.06 (2.53)	0.57 (0.38)	167.58 (404.94)
R^2	0.33	0.14	0.17	0.14	0.10	0.10

*,**,*** indicate significance at the 0.10, 0.05, and 0.01 levels, respectively for the two-tailed test. Linear and quadratic terms for sleep levels are included as controls in all models (detailed results available upon request)

quadratic sleep level term in each model estimated (estimated coefficients from these variables are not shown in Table 4 but available upon request).⁶

The OLS estimates of (1) for each of the three dependent variables for each treatment are shown in Table 4. As can be seen, significant results are found only in Treatment 1, and so we limit our discussion to Treatment 1 results. Though this lack of significant effects in the OLS estimates for Treatment 2 indicates a lack of robustness in our results, this is not completely unexpected given that Treatment 1 is considered to provide a sharper test of strategic reasoning.

Subjects with higher trait-level cognitive ability (*NFC*) scores are estimated to have outcomes consistent with higher strategic reasoning in Treatment 1, although the result is only statistically significant in the model of *GuessRank*. In two of three Treatment 1 models, we estimate that female subjects submit guesses consistent with significantly lower levels of reasoning, all else equal. We find a previous treatment

⁶We do not include results from these sleep level controls so that attention is not diverted from the circadian mismatch estimates. Our initial intent was to use this experiment design to evaluate the effect of both circadian mismatch *and* voluntary sleep choice on Beauty Contest outcomes, but the fact that sleep levels are not manipulated and may therefore correlate with some omitted variable has led us to use these variables solely as sleep controls. Estimates from these sleep control variables are statistically significant in Treatment 1 and indicate that voluntary sleep loss may have an even more robust adverse effect on strategic reasoning than circadian mismatch (simple correlation between sleep quantity and circadian mismatch is a statistically insignificant 0.0575). However, we cannot rule out that these variables capture the effects of another omitted variable. Future research that uses a design with an explicit sleep manipulation is needed to properly examine if sleep loss harms strategic reasoning.

anchoring effect on *Guess* when the first round of Treatment 1 is in round 11 (i.e., the 2nd treatment of the session). We do not estimate a significant independent time-of-day effect (*Morning Session*).

We now turn to our state-level cognitive variable of interest, *Mismatch*. Circadian mismatch is estimated to be statistically significant in two of the three models, and in the predicted direction—mismatch harms behavioral outcomes. Overall the nonparametric and econometric results reveal that circadian mismatch only affect strategic reasoning in the $p < 1$ Guessing Game (Treatment 1), indicating that the temporal cognition effects of relatively mild circadian mismatch may only manifest in more cognitively complex strategic environments. Nevertheless, these results are quite relevant in a modern world where many individuals must perform at suboptimal times of day, either by choice or constraint. These results might also be an indication that more significant decision effects would be experienced in instances of extreme circadian mismatch (e.g., graveyard shift workers), though additional research would need to examine this claim.

4.3 Adaptation and Hypothesis 2

Though we find some evidence that strategic reasoning is significantly affected by circadian mismatch, our second hypothesis was that not all thought processes will display similar behavioral effects from a temporal depletion of cognitive resources. Summary results in Figs. 2a and 2b reveal that the convergence towards equilibrium across rounds appears largely unaffected by circadian mismatch. Following Nagel (1995), we can more specifically examine the data's conformity to a qualitative learning model that assumes subjects adjust their guess (ex post) in the direction of the optimal adjustment factor: the learning-direction theory. Nagel defines subject i 's adjustment factor and the optimal adjustment factor as

$$a_{it} = \frac{x_{it}}{(\text{mean})_{t-1}}, \quad (2)$$

$$a_{opt,t} = \frac{x_{opt,t}}{(\text{mean})_{t-1}} = \frac{p \cdot (\text{mean})_t}{(\text{mean})_{t-1}}, \quad (3)$$

where $(\text{mean})_{t-1}$ is the midpoint of the guess interval for the first round of the treatment. A “good” adjustment is where a subject changes her adjustment factor to correct the direction of the previous round's error. That is, if $a_t > a_{opt,t}$ then $a_{t+1} < a_t$ represents a good adjustment (and $a_t < a_{opt,t}$ implies $a_{t+1} > a_t$). This simple model is based on reinforcement learning. Such a model is consistent with our belief that mimicry of previous round outcomes is available in our information feedback environment—at the end of a decision round subjects are shown their own guess, others' guesses, the average group guess and that round's target number ($p * \bar{x}$), with these latter two pieces of information calculated for them.⁷ Analysis is restricted

⁷Note that in such an environment, very little cognition is required of the subjects and what stands out (if one does not have the winning guess) is whether the subject's own guess was greater or less than the winning guess. It is this feature that allows us to examine our two distinct hypotheses within the repeated Beauty Contest with feedback.

to the subsample of the data where the subject did *not* win or share in the prize in the previous round—winning rounds provide distinct feedback to subjects. We then have a sample of $N = 787$ Treatment 1 and $N = 297$ Treatment 2 guesses in response to a no-win outcome in the previous round.

We find that in the pooled data 75% of subject guesses in Treatment 1 are consistent with learning-direction theory, similar to proportions reported in Nagel (1995). In Treatment 2, the proportion is just 56%, though this is still statistically different from random adjustment factor alterations based on a coin flip (binomial test: $p = 0.03$, one-sided test against the null hypothesis of 50%). We find no statistically significant differences between the proportion of guesses conforming to the learning-direction model across the subsamples of CM versus MM subjects for either treatment (two-sample proportions test: p -value > 0.10 in both cases).

An alternative approach would be to compare OLS estimates of initial round outcomes from Table 4 to those from similar models of the rounds 2–10 data. We pool the data from rounds 2–10 for each treatment ($n = 918$) to estimate such models but find no statistically significant effect of *Mismatch* (or the trait-level cognitive ability control, *NFC*) on any of the key outcome variables in either Treatment 1 or Treatment 2.⁸ For the resultant estimates, standard errors are smaller than for the initial round estimates in Table 4, which indicates that the effect of our key independent variables on behavioral outcomes decreases with repetition in this Beauty Contest environment (as opposed to diminished significance due to larger standard errors). Thus, our data are consistent with Hypothesis 2—more automatic response mechanisms are found to be resilient to circadian mismatch.

5 Concluding remarks

Temporal variations in cognition may arise for numerous reasons. We hypothesized that circadian timing alters available cognitive resources, though other cognition risk factors certainly exist (e.g., time constraints or distractions—Wilson and Schooler 1991; Kahneman et al. 1967). While some limited evidence suggests from psychology indicates that time-of-day modulates cognition in certain aspects of high-level decision-making (Bodenhausen 1990; Kruglanski and Pierro 2008), we add to this literature by exploring the effects of circadian mismatch in a strategic decision environment. Such research is relevant given that mild cognition deficits caused by circadian mismatch are commonplace among real world decision makers.

Our first result is that the adverse state of circadian mismatch harms strategic reasoning in the $p < 1$ Beauty Contest. We find evidence to support this claim from non-parametric analysis of subject *Guesses* as well as econometric estimates of circadian mismatch effects on two of three distinct outcome measures. This is consistent with

⁸More detailed results of these additional OLS estimates are available upon request. The estimation of the *Guess* model for Treatment 1 does indicate that higher *NFC* scores predict significantly lower guesses ($p = 0.07$, two-tailed test). This effect was estimated as well for the initial round data, although it was not statistically significant in Table 4 due to higher standard errors in the initial round *Guess* estimates. Interestingly, the result that outcome measures of female subjects are consistent with lower levels of strategic reasoning is rather robust even in these later decision rounds, both in Treatments 1 and 2.

the hypothesis that even relatively mild temporal variations in cognition may harm PFC-driven strategic reasoning. In the cognitively less challenging $p > 1$ Beauty Contest, we do not find the same result, which indicates that the level of cognitive complexity in the decision task matters for this result.

Individuals may be overconfident with respect to their own behavioral susceptibility to adverse sleep states, and subjective sleepiness differences may be a poor indicator of cognitive performance differences (Van Dongen et al. 2003). Physiological manifestation of sleepiness may not appear until subjective sleepiness is extreme (Åkerstedt and Gillberg 1990). Thus, we cannot expect that individuals are self-aware of these cognitive limitations, as the tendency is to be biased against recognizing one's own weakened cognitive states. This may be particularly true when considering more elusive *decision* effects, as opposed to effects on areas such as reaction time, speech acuity, or motor function.

Our second result is that adaptation following repeated administration of the decision task with feedback was resilient to circadian mismatch. Future research will have to examine at what point more severe challenges to temporal cognition would alter this result (e.g., decision making in the middle of the night or following extended total sleep deprivation). Nevertheless, our data are consistent with a dual systems framework of cerebral function (Schneider and Shiffrin 1977; Camerer et al. 2005) because we find that adaptive or mimicry-based decisions, which should rely more on automatic/unconscious response systems, are not affected by the same circadian mismatch state that affects the PFC-driven process of anticipation and strategic reasoning.

Of course, we are not making any general comment that one neural system is superior to the other, or that it is optimal for one neural system to remain intact without the other's usual input—consider, for example, the rational-thought regulation of emotions. Loss of emotional response regulation due to cognitive stress may lead to decisions that are suboptimal, socially inappropriate, or reckless. Our results more basic: the data suggest that even transitory deficits in cognition that are not extreme can put individuals at a competitive disadvantage in decision arenas where strategic reasoning and anticipation are important.

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Appendix: Experiment instructions and methods details

A.1 Sleep data acquisition

The actigraphy devices used are accelerometers with sensitivity of 0.05 g-force, worn on the non-dominant wrist to measure its activity as a proxy for gross motor activity (model AW-64, Phillips Respironics). Data sampling is at epoch lengths

of 30-seconds. The devices are impact resistant, water-proof to 1-meter depth for 30 minutes, and can therefore be worn 24-hours a day with few exceptions. The actigraphy data were scored using the manufacturer's software which, along with complementary sleep diary data, generates an objective and validated measure of total sleep time that does not significantly differ from polysomnography-derived measure (see Kushida et al. 2001, and references therein). In our sample, nightly sleep average between the morning- and evening-types is not significantly different (Mann-Whitney test, $p > 0.10$, two-tail test), indicating no confounding correlation between diurnal preference and sleep levels.

A.2 Beauty contest experiment instructions

The Beauty Contest was administered through the open-access Veconlab web-based experiments platform at <http://veconlab.econ.virginia.edu/gg/gg.php>. Instruction pages shown below are for Treatment 1 parameters and a group of 8 individuals (group size varied from 8 to 10 individuals in our experiments). Treatment two instructions altered the allowable guess range and p value fraction. Formatting and subject ID numbers have been removed for this Appendix. A treatment is administered for 10 rounds, after which new instructions appear on screen to describe the new parameterization of the 2nd treatment. We varied the order of whether the Treatment 1 parameters were administered during the first or second block of 10 rounds.

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