Sleep, Health, and Human Capital: Evidence from Daylight Saving Time

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Abstract

This paper studies the relationship between sleep and human capital using a census of 160 million hospital admissions from Germany and 3.4 million survey responses from the U.S. over one decade. Our approach exploits the exogenous extension of sleep when Daylight Saving Time ends. We show that setting clocks back by one hour significantly extends night's sleep and reduces the likelihood to unintentionally fall asleep for four days. Moreover, hospital admissions decrease significantly for four days. We find ten fewer hospitalizations due to cardiovascular diseases per day per one million population. Finally, we categorize and monetize economic benefits of additional sleep.

Keywords: sleep deprivation, health, human capital, hospital admissions, BRFSS, Daylight Saving Time (DST)

JEL codes: H41, I18, I31

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1. INTRODUCTION

Since the seminal contributions by Becker (1964), Grossmann (1972) and more recently by Heckman (e.g. Cunha and Heckman, 2007), large strands of the economic literature have theoretically modeled and empirically tested for "human capital" effects. Human capital is a broadly defined concept that refers to the stock of health, ability, or personality. Important economic studies test for the short and long-run effects of risky healthy behavior (Kenkel, 1991; Cawley, 2015), ambient air pollution (Graff Zivin and Neidell, 2013; Currie et al., 2014) or *in utero* conditions (Almond and Currie, 2011). The human capital outcome measures vary from birth outcomes to specific diseases, health care utilization, labor market and social outcomes. The human capital input measures are likewise plentiful and include health behaviors, environmental shocks, or education.

This paper contributes to the human capital literature by examining the role of sleep. It is one of the first papers in the economic literature to investigate how sleep can affect one integral component of human capital: human health. Despite the abundance of human capital studies, the one single activity that humans spend most of their time doing—sleeping—has received very little attention in the economics literature. As Sendhil Mullainathan (2014) puts it: "The economic consequences of inadequate sleep are surely huge." Hillman et al. (2006) estimate the economic costs of sleeplessness at almost one percent of GDP. The global sleep-aid market is growing rapidly with an estimated size of $80 billion in 2020. The United States alone counts 40 million sleeping pill prescriptions per year and about 2800 "sleep labs" exist (CDC, 2013; Persistence Market Research, 2015; DiSalvo, 2015).

In one of the few sleep studies in economics, Biddle and Hamermesh (1990) show that more labor market activities reduce sleep duration. In another causal effects study, Hamermesh et al. (2008) exploit television schedules and time use data to demonstrate how time zones affect market work and sleep in the U.S. Giuntella and Mazzona (2017) also exploit U.S. time zones to show in a geographic Regression Discontinuity Design that sleep deprivation can lead to poor health and obesity. Moreover, Gibson and

Schrader (2018) identify positive wage returns to sleep. Billari et al. (2017) exploit the rollout of high-speed internet access in Germany to show that DSL access reduces sleep duration and sleep satisfaction.

The medical literature on sleep is richer. It documents that about ten percent of the population are permanently sleep deprived: Knutson et al. (2010) report a significant increase of "short-sleepers" (less than six hours) between 1975 and 2006 in the U.S. Whereas correlation studies generally find a link between sleep deprivation, poor health, and cognitive ability in population samples, it remains unclear whether this link represents a causal relationship (Moore et. al., 2002; Taheri et al., 2004; Mullington, et al., 2009; Killgore, 2010). Banks and Dinges (2007) provide a comprehensive review of the behavioral and physiological effects of inadequate sleep, including experimental evidence with healthy adult laboratory participants. They conclude that restricting sleep below an individual's optimal level could cause a range of neurobehavioral deficits.

This paper exploits the quasi-experimental nature of a regulation that has been affecting the sleep pattern of more than one billion people in 70 countries around the globe: Daylight Saving Time (DST). It is the practice of setting clocks forward by one hour in spring and backward by one hour in fall. The original DST rationale was to save energy. Today, all countries in the European Union, the great majority of the U.S. states and Canadian provinces, as well as 40 other countries observe DST.

Our identification strategy focuses on the time shift in the *fall* when the clocks "fall back" and exogenously add one additional hour at night. One reason to focus on the fall transition is the absence of possible confounding factors such as crime (Doleac and Sanders, 2015) or accidents (Smith 2016), as has been established by recent economic studies. The main idea is that the night time extension induces people to sleep more. Indeed, using a large U.S. survey, we find that people report sleeping significantly more following the time shift. Moreover, we find a significant reduction in the share of people who report having unintentionally fallen asleep during the day. We then exploit administrative data on hospital admissions to identify health effects of increased sleep as a result of the time shift.

In total, we use two large datasets that complement each other: (a) The U.S. Behavioral Risk Factor Surveillance System (BRFSS), which records sleep and self-reported tiredness and allows us to study sleep and mild human capital effects. (b) The German Hospital Census, which records all hospitalizations in Germany and allows us to study objective health effects. Both datasets together provide evidence from the most populous American and European country over a decade, from mild human capital effects across the entire population to hospitalizations among the at-risk population. In addition, both datasets carry very large numbers of observations—3.4 million interviews from the U.S. and 160 million hospital admissions from Germany. The sample size is crucial to control for seasonal and weekday confounders while maintaining enough statistical power to precisely identify health effects at a daily level.

Our findings show consistent evidence that subjectively reported human capital ("unintentionally falling asleep") and objectively measured health (hospital admissions) significantly improve for about four days following fall DST. The BRFSS data show that the likelihood to unintentionally fall asleep decreases by almost 30% in the four days after the time shift. In addition, hospital admissions decrease significantly—also for about four days. For example, hospitalizations due to cardiovascular diseases decrease by ten admissions per day per one million population (-10%) for about four days.

We corroborate our main findings with permutation tests using all non-DST transition weeks during the year. We also run falsification tests using outcomes that have no theoretical link with current sleep, e.g., having received flu shot in the *previous* year. Moreover, we discuss alternative mechanisms through which the DST transition might affect health—and show that these are unlikely to drive our findings.

In the last part of the paper, we categorize and monetize the economic benefits of getting more sleep for the sleep deprived.

2. DATASETS

2.1 The U.S. Behavioral Risk Factor Surveillance System (BRFSS)

The first dataset measures sleep duration and human capital ("unintentionally falling asleep") in the general population. The Behavioral Risk Factor Surveillance System (BRFSS) is a large, annual telephone survey of U.S. adults aged 18 and above, administered by the *Centers for Disease Control and Prevention* (CDC). The survey began in 1984 with fifteen participating states; by 1996, all 51 U.S. states participated in the survey. We focus on the period from 2001 to 2010, which includes more than 3.4 million survey responses in total. However, as illustrated by Figure A1 (Appendix), our main sample extracts six weeks around the time shift and counts 421,101 observations. We use the full sample in robustness checks.

Table A1 reports descriptive statistics of this main sample. Our control variables contain demographics such as age, sex, race, and marital status, as well as education and employment status.

Construction of Main Dependent Variables

First, we measure sleep duration. Responses to the following question are integers between 0 and 24: *"On average, how many hours of sleep do you get in a 24-hour period? Think about the time you actually spend sleeping or napping, not just the amount of sleep you think you should get."* We interpret the answers as a good measure of actual sleep duration. It is worth noting that the question does not explicitly ask for the duration of sleep *last night*, but instead the responses will reflect average sleep in the recent past. Hence, our estimate on sleep duration is likely downward biased and a lower bound. Thus, it provides a conservative test of whether people sleep more when clocks are set back in fall.

Second, we measure human capital during the day by using responses to the following question: *"During the past 30 days, for about how many days did you find yourself unintentionally falling asleep during the day?"* We convert the responses into a binary variable indicating the share of people who unintentionally fell asleep. On average, 35% of the U.S. population report unintentionally falling asleep (Table A1). Appendix C discusses issues related to measurement errors in these outcome measures.

Finally, in robustness checks and falsification tests, we use information on whether respondents received a flu shot in the past calendar year, and whether they exercise.

Daylight Saving Time in the U.S.

In the United States, DST ends on the first Sunday in November. The time change occurs at 2am, where the clocks are set back to 1am, effectively extending the night by one hour. DST is observed by most states in the U.S. Our empirical strategy only uses states that observe DST. [1]

2.2 German Hospital Admissions Census

The second dataset provides objective health measures. The dataset comprises all German hospital admissions from 2000 to 2008. The 16 German states collect these information and the *German Federal Statistical Office* provides restricted data access for researchers. Germany has about 82 million inhabitants and about 17 million hospital admission per year. To obtain the working dataset, we aggregate the admission-level data on the daily county level and then normalize admissions per 100,000 population.

Table B1 reports the descriptive statistics. As seen, the data include information on age and gender, the day of admission, the county of residence as well as the diagnosis in form of the ICD-10 code.

As with BRFSS, our working dataset focuses on the six weeks centered around the time shift (Figure A1). This main sample has 336,604 county-day observations over 9 years.[2]

[1] There was a structural change to extend DST in 2007; prior to 2007, DST ended in October. We experimented with exploiting this policy change to identify effects but lack statistical power (results are available upon request).

[2] We leave the data at the county-level and do not further aggregate up to the national level for a few reasons. First, this allows us to stratify the effects weather conditions and control for county characteristics. Another reason is that we lose statistical power when aggregating up to a time series at the national level.

Construction of Main Dependent Variables

First, we generate *all cause admission rate.* On a given day, we observe 59.77 hospital admissions per 100,000 population (Table B1, Appendix).

Next, by extracting the ICD-10 codes I00-I99, we generate *cardiovascular admission rate,* the single most important subgroup of admissions (9.53 admissions per 100,000 population, Table B1). Extracting the codes I20 and I21, the *heart attack rate* is 1.59 admissions per 100,000 population.

Finally, we generate the *injury rate (V01-X59).* We also test for changes in *suicide attempts* (T14) and *drug overdosing* (T40) per 1 million population.

Daylight Saving Time in Germany

In Germany, DST ends on the last Sunday of October in all German states. The time change occurs on 3am, where the clocks are set back to 2am.

3. EMPIRICAL SPECIFICATION

Our identification strategy relies on sleep extensions created by DST transitions in the fall. These occur on different dates each year. Our large datasets allow us to comprehensively control for seasonal confounders, weekday effects, and yet to precisely estimate health effects. Our preferred empirical specification identifies the effects at the daily level. We also estimate models at the weekly level to capture medium-term and potential intertemporal substitution effects.

3.1 Main Specification

Our preferred specification employs daily dummies around the DST time shift in the fall:

$$y_{id} = ß_0 + ß_1\, DST_{id} + X_{id}'\gamma + Vacation_d + \phi_m * \delta_t + DOW * \phi_m + t + t^2 + \mu_s + \varepsilon_{id} \quad (1)$$

Where y_{id} is the health outcome variable using the German Hospital Census (BRFSS), for county (individual) i on day d. *DST* is a vector containing fifteen daily dummies around the DST time shift in fall.

Equation (1) includes controls that net out seasonal and weekday confounders. These are crucial when using high-frequency data within the DST context. For example, hospital admissions decrease on Sundays and on national holidays (Witte et al., 2005). $Vacation_d$ controls for public holidays and the Halloween.

Due to the relevance of day-of-the-week (DOW) effects in hospitalizations, we additionally interact DOW with month fixed effects ($DOW*\phi_m$). This is important as Sundays in November may be systematically different from Sundays in September. For example, in our data, relative to Sundays, hospital admissions almost double on Mondays and this effect varies over the months of a year. Because DST transitions are always on Sundays, it is crucial to net out DOW effects by month of the year.

Our model also includes month-year fixed effects ($\phi_m*\delta_t$) and linear and quadratic time trends ($t + t^2$). However, the findings are robust to replacing month-year fixed effects with separate month and year fixed effects and omitting time trends. In addition, Equation (1) corrects for county-level or individual-level socio-demographics ($X_{id}'\gamma$) and persistent differences across states or counties (μ_s).

Because it is unlikely that admission rates are either independent over time or across space, we correct the standard errors, ε_{id}, by applying two-way clustering across counties and over time (Cameron et al., 2011). When using the independently drawn and representative observations of the BRFSS, we cluster standard errors at the date level. All BRFSS regressions are probability weighted.

3.2 Identification

The key idea of our identification strategy is that the running variable is time, and that the DST transition generates the treatment. DST transitions are arguably exogenous to individuals because humans cannot influence time. Our main specification de-trends the outcome variables using DOW-month and month-year fixed effects, in addition to socio-demographic controls. We also disentangle weekday and seasonal effects from specific events such as vacation days or national holidays. The richness of our data still allows us to obtain precise estimates at the daily level. However, we also compare the day-to-day short-term effect of the change in time to the net effect on a weekly basis. Moreover, in effect heterogeneity specifications that test for behavioral mechanisms, we stratify the results by ambient climatic conditions such as temperature and hours of sunshine.

Sample Selection and Definition of Treatment and Control Groups

As mentioned, we restrict our main sample to three weeks before and three weeks after the time shift (Figure A1). However, the results are robust to including all 52 weeks of the year. The findings are also robust to assigning all three post-transition weeks to the "treatment group." Doing this yields results that are similar to a standard Regression Discontinuity design where the post-treatment outcomes are compared to that of the pre-treatment, conditional on all covariates shown in Equation (2), see for example Doleac and Sanders (2015).

Table A2 compares the mean covariate values for the week of DST transition—our "treatment week"—to the control weeks prior and post the treatment week. The normalized difference proposed by Imbens and Wooldridge (2009) shows that no single value is above the threshold of 0.25 and are all very close to zero in size. Figure A2 shows that the BRFSS has very balanced sample sizes over the 12 calendar months.[3]

4. RESULTS

4.1 Effects of the Time Shift on Sleep and Staying Awake

First, we use BRFSS measures on sleep to provide first-stage evidence that the fall DST time shift increases the average sleep duration and human capital (in the form of staying awake) in the population.

Table 1 shows the results when we estimate Equation (1) using the BRFSS sleep measures as outcome variables. The first two columns use self-reported *hours of sleep* as the dependent variable; the last two columns use *unintentionally fell asleep* as dependent variable. The variables capture sleep in the "recent past," not just from last night (Section 2.1). The estimates are therefore likely downward-biased; for us, they provide a crude affirmative test that people do sleep more when clocks fall back.

According to column (1), on average, people sleep an additional 0.27 hours (or about 16 minutes). This estimate is statistically significant at the 1% level. Note that this is an *average effect* across the entire population. It is likely driven by the sleep deprived. In fact, a 16-minute increase in sleep across the entire population is consistent with a quarter of the population sleeping one hour more.

Column (2) uses a model that measures the effect at the weekly level. Here we include a dummy variable that equals 1 for the entire the *week* of the DST transition. Again, we find that people sleep a statistically highly significant 0.18 hours (or 11 minutes, 2.5%) more per night (for seven nights) on the week of DST transition.

[Insert Table 1 about here]

[3] Inspecting the observables on the transition day also yields no evidence that BRFSS respondents of the fall DST transition differ significantly (detailed results available upon request).

We find corroborating evidence when we turn to the model with self-reported tiredness in columns (3) and (4). The estimated daily effect in column (3) is negative but imprecisely estimated, which may be due to the noisy nature of this survey question. In column (4), where we estimate the weekly model, we find that people are 4.4 percentage points (ppts) or 12.6% less likely to fall asleep in the week of time shift. This estimate is statistically significant at the 5% level.

[Insert Figure 1 about here]

Figure 1 plots the daily dummies of the vector DST_{id} in Equation (1) using *unintentionally fell asleep* as outcome measure. Figure 1 is an event study-type graph and plots estimates for -7, -6,…,0,…, 6, 7 days relative to the time shift. Note that this is *not* a simple descriptive graph but compares the effect in the treatment group relative to the control group (Figure A1), after having netted out of seasonal and weekday confounders (Equation (1)).

In Figure 1, despite the noisy nature of the self-reported measure, following the DST fall transition one observes a distinct four-day decrease in the probability of unintentionally falling asleep of about -0.1ppt, or -29%. Below, we find similar four-day pattern using hospital admissions from Germany over a decade. We interpret these consistent pattern as reinforcing evidence for the credibility of our identification strategy.

4.2 Effects of the Time Shift on Hospital Admissions

Table 2 shows weekly admission estimates by disease groups for Germany. Each column is one model as in Equation (1). The main regressor of interest is a dummy indicating the week of DST transition.

[Insert Table 2 about here]

Except for drug overdosing, all estimates are negative and statistically significant, mostly at the 1% level. The weekly decreases in daily admissions range from 8.3% for the *all cause admission rate* (column (1)) to a similar 7.5% for *cardiovascular admissions* (column (2)). *Injuries* decrease by almost 5% or about 2.7 per 1 million population. Consistent with the medical literature (Berk et al. 2008), even *suicide attempts* appear to decrease slightly by 2.76 per 100 million residents (although we consider these findings as suggestive, see Figure B3a).

[Insert Figure 2 about here]

Next we zoom in and plot the daily estimates of Equation (1) in event study graphs. Figure 2a shows *all cause admissions* per 100,000 population and Figure 2b shows *cardiovascular admissions* per 100,000 population. Despite conservative two-way clustering, we are able to identify even daily effects in a very precise manner.

The two event study graphs in Figure 2 show a characteristic four-day pattern of decreases in admissions: We observe significant decreases in overall and cardiovascular admissions on days one to four after the time shift. The effect is strongest on the Monday after the clocks are set back, and it decreases smoothly over the next three days before it disappears on day five. The decrease for cardiovascular admissions equals about 1 avoided admission per 100,000 population for four days, or about a 10% decrease for four days.

In robustness checks, we obtain exactly the same pattern using the full sample (Figure B1), *heart attacks,* and *injuries* (Figure B2). The consistency of these patterns for even heart attacks suggests that the decrease in admissions is not due to voluntary behavioral responses.[4]

We interpret the similarity of these four-day patterns as strong support for our identification strategy. The implication is that additional sleep leads to immediate health improvements across disease groups

[4] Note that the German data do not allow us to distinguish between emergency room visits, elective visits and other type of admission.

for people who are *on the margin* of being hospitalized. This find is plausible as the medical advice for people on the margin of being hospitalized is certainly to get sufficient rest.

4.4 Could Alternative Mechanisms Explain the Health Effects?

Now we investigate whether alternative mechanisms could explain the effects that we find. For example, an alternative mechanism that could theoretically produce the health benefits is the shift in ambient light from evening to morning hours. As the clocks “fall back” by one hour, sunrise and sunset both occur at earlier times. One could hypothesize that, because mornings get brighter earlier, people are more likely to exercise in the morning following the transition (and less likely to exercise in the evening). To test for this the net effect on exercising, we use a BRFSS measure on exercising and run our standard model in Equation (1). The daily effects are in Figure A3a. In line with Giuntella and Mazzona (2017), we find no evidence that exercising changes as a result of the time change.

Next, we stratify the effects by weather conditions using the German Hospital Census. As we explain in Appendix B, we use data from more than one thousand ambient weather monitors on a daily basis from 2000 to 2008. The underlying hypothesis is that weather conditions determine how and where individuals spend their time (Gebhart and Noland, 2014); better outdoor conditions should also indicate whether changes in exercising behavior play a confounding role. Table B2 stratifies the effects by (i) temperature, (ii) rainfall, (iii) sunshine, and (iv) cloudiness. Methodologically, we run our standard model, control for weather conditions and interact DST_{id} with the weather measures in the column headers. Consistent with the absence of changes in exercising, there is no evidence that ambient conditions matter. None of the interaction terms between the four weather measures and DST_{id} is statistically significant.

A shift in ambient light could also affect traffic accidents. However, Smith (2016) does not find evidence that the time shift in the fall significantly affects fatalities. Moreover, traffic accidents cannot explain why hospital admissions drop sharply across a range of diseases, most of which are not related to accidents.

Another potential confounding factor could be crime. Doleac and Sanders (2015) show that robberies decrease in the days following the DST transition in *spring* (when evenings get dark later). However, they find no significant effects on crime rates in fall. Even if there was a significant robbery effect, robberies would then *increase* following the time shift in the fall (because it gets dark sooner), and thus have adverse health effects, opposite the prediction of our sleep mechanism. [5]

The fall DST transition increases the length of the Sunday from 24 to 25 hours. This may affect hospital admissions (or health survey responses) in ways unrelated to sleep. The most plausible hypothesis is that, because the day is longer, the total number of admissions will be *higher*, suggesting that we identify a lower bound. Moreover, this mechanism cannot explain why we find persistent health effects over four days.

Finally, we estimate placebo regressions. Our first placebo test, using the BRFSS, is having received a flu shot in the *past year* as an outcome measure. This outcome is, by construction, unrelated to getting additional sleep. As expected, Figure A3b shows no impact on this outcome. Our second placebo test uses the hospital data to conduct the following permutation test: We start in July of each year and select six-week windows of data as illustrated in Figure A1. Then, we run our standard model with aggregated effects at the weekly level, pretending that the fourth week was the week of the time shift. Next, we move the six week window one week further into August and repeat the approach. We permute until week six of our selected sample hits the true week of the time shift and continue with six-week windows

[5] While both effect sizes—on robberies and fatalities—are cleanly identified by the studies just cited, they are rather small and unlikely to confound our population health estimates. Moreover, our "Injury Admissions per 1 Million Population" outcome category should capture these effects.

until end of the year.[6] As such, we obtain 23 weekly placebo estimates. Figure B4 plots the distribution of these weekly placebo estimates along with the true estimate. Clearly, the decrease in admissions following the time shift does not fall within the statistical placebo estimate distribution.

4.5 Quantifying the Economic Benefits of One Additional Hour of Sleep

In this last subsection we attempt to categorize and monetize the benefits of additional sleep. These calculations are based on several assumptions, but provide a basic framework for such an exercise.

First, one can assesses the value of increases in work productivity when sleep-deprived employees gain more sleep. According to Gibson and Schrader (2018), the short-term wage returns for an additional hour of sleep would equal 1.1% of the wage. Given the average daily wage of $230 in the U.S. (U.S. Census Bureau, 2018), this translates into $10 over four days. Assuming that these gains only apply to the ten percent sleep deprived full-time employed Americans, it would sum to $500 thousand per one million population (Table D1, Appendix).

Second, one can monetize the value of avoided hospital admissions (cf. Dobkin et al. 2018). Figure 2a implies 100 fewer admissions per one million population over four days. Using German numbers (OECD, 2017), the benefits of one avoided four-day hospital stay can be decomposed into €2000 for medical costs, €450 for lost labor as well as €550 for lost quality of life (Table D1, Appendix).

Finally, Smith (2016) quantifies the number of avoided traffic fatalities with 30 for the entire U.S. (0.09 per 1M pop.). Evaluated at $5M per life saved (Kniesner et al. 2010), we would obtain values for saved statistical lives of around $450 thousand per one million population (Table D1, Appendix).

[6] The true DST week is never included in these placebo six week samples.

This little exercise illustrates that the individual benefits of additional sleep can be dramatic for those on the margin of having a heart attack or being hospitalized. The total welfare benefits sum to about $1.3 million per 1 million population.

5. DISCUSSION AND CONCLUSION

This paper exploits the quasi-experimental nature of Daylight Saving Time (DST) to assess whether getting more sleep during the fall transition affects human capital and population health in the short-run. Because exogenous shifters of sleep are very rare, it is one of few causal studies on this topic from the field. To identify effects, we use a large survey dataset from the U.S. and the census of hospital admissions from Germany over a decade. Properly investigating the sleep-health relationship around DST transitions on a daily level requires powerful representative data. These are necessary because it is crucial to estimate rich econometric specifications that consider weekday effects in addition to general and specific seasonal adjusters.

Our results show consistent and robust evidence for the U.S. and Germany, from mild self-reported sleep outcomes to more severe health issues that require hospitalizations. We find that respondents sleep more and that the probability to unintentionally falling asleep decreases by almost 30% for four days. Moreover, hospital data also show the same characteristic four-day drop in admissions in the days following the transition. For example, cardiovascular admissions decrease by ten admission per one million population over four days. This implies that, for people *on the margin* of having a heart attack, additional sleep and rest may prevent such health shocks. We also find very similar patterns of reduced admissions for patients with other diseases (which are not necessarily diagnosed on these days), but no changes in placebo tests and no spurious correlations in falsification tests exploiting the remaining weeks of the year. Overall, our findings show that humans' most time-consuming activity, sleep, affects their human capital. In the last part of the paper, we attempt to categorize and monetize the benefits of more sleep that have been identified by this paper and companion research in economics. Under some assumptions, we assess the total societal benefits of gaining one hour of sleep with about $1.3 million per 1 million population.

The main objective of this paper is to provide evidence for the existence of a causal relationship between sleep and human capital. We do not intend to draw conclusions about the overall welfare effects of Daylight Saving Time. We also would like to point to a caveat: our reduced-form approach is well-suited for the identification of causal and immediate intent-to-treat effects, but less suited to identify long-term effects of sleep. Based on sleep habits, sleep may affect mood, cognitive skills and health cumulatively over time in the long run. Alternatively, it is possible that the human body is able to adapt to (adverse) sleeping conditions. Field experiments have the power to find answers to these questions (cf. Tepedino at al. 2017). More research is necessary to better understand how improvements in sleep quality may improve living quality, education and labor market outcomes as well as life expectancy.

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FIGURES AND TABLES

<u>Figure 1: BRFSS:</u>
Effects of Fall DST Transition on Unintentionally Falling Asleep, 2001 2010

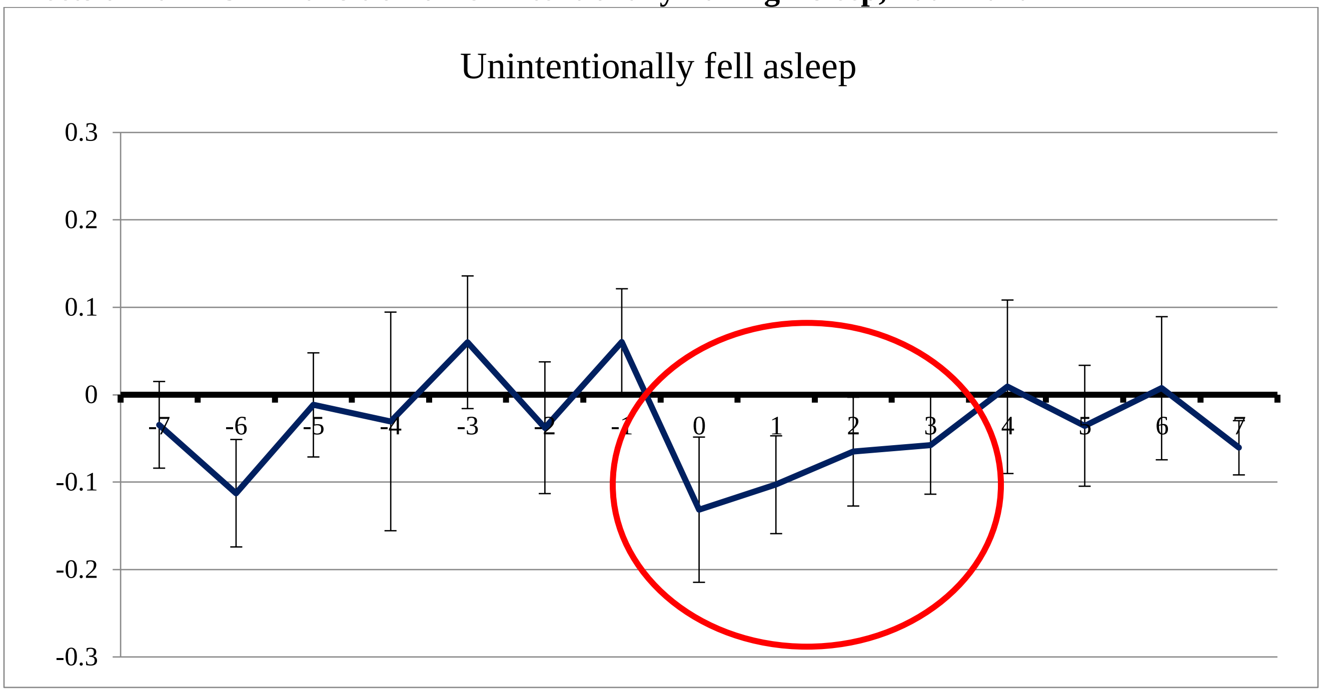
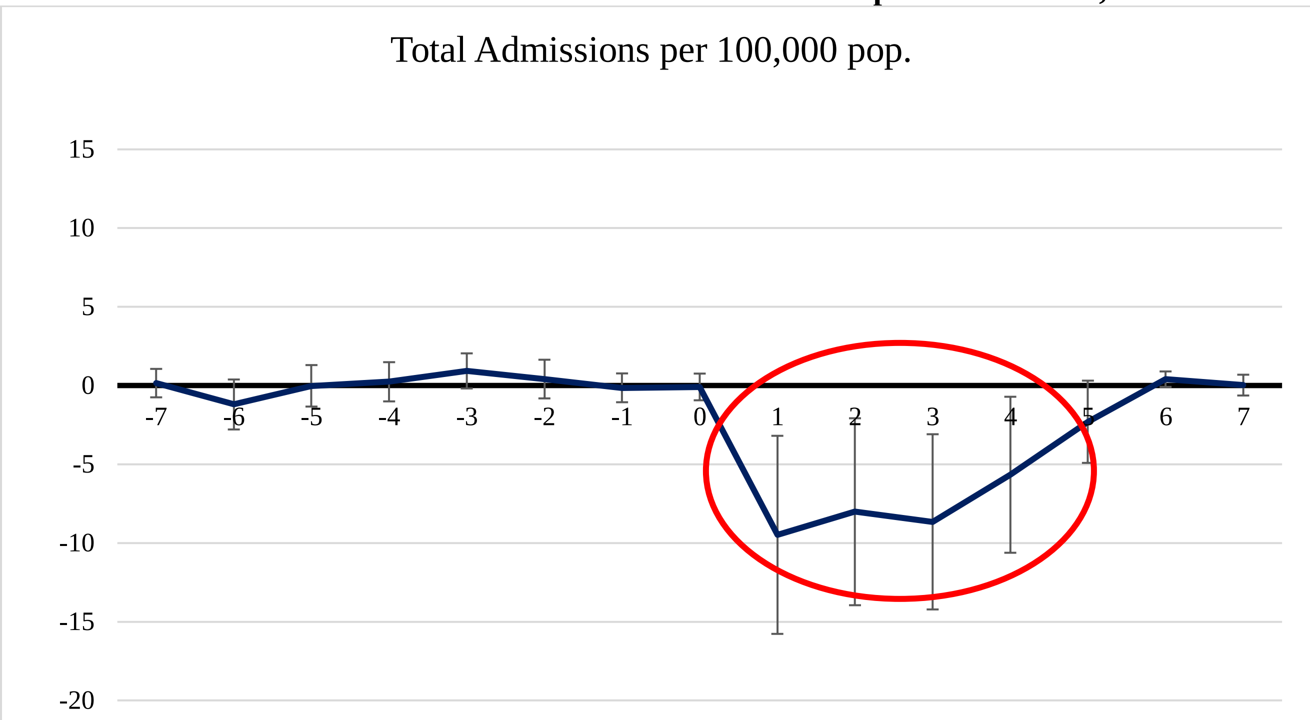
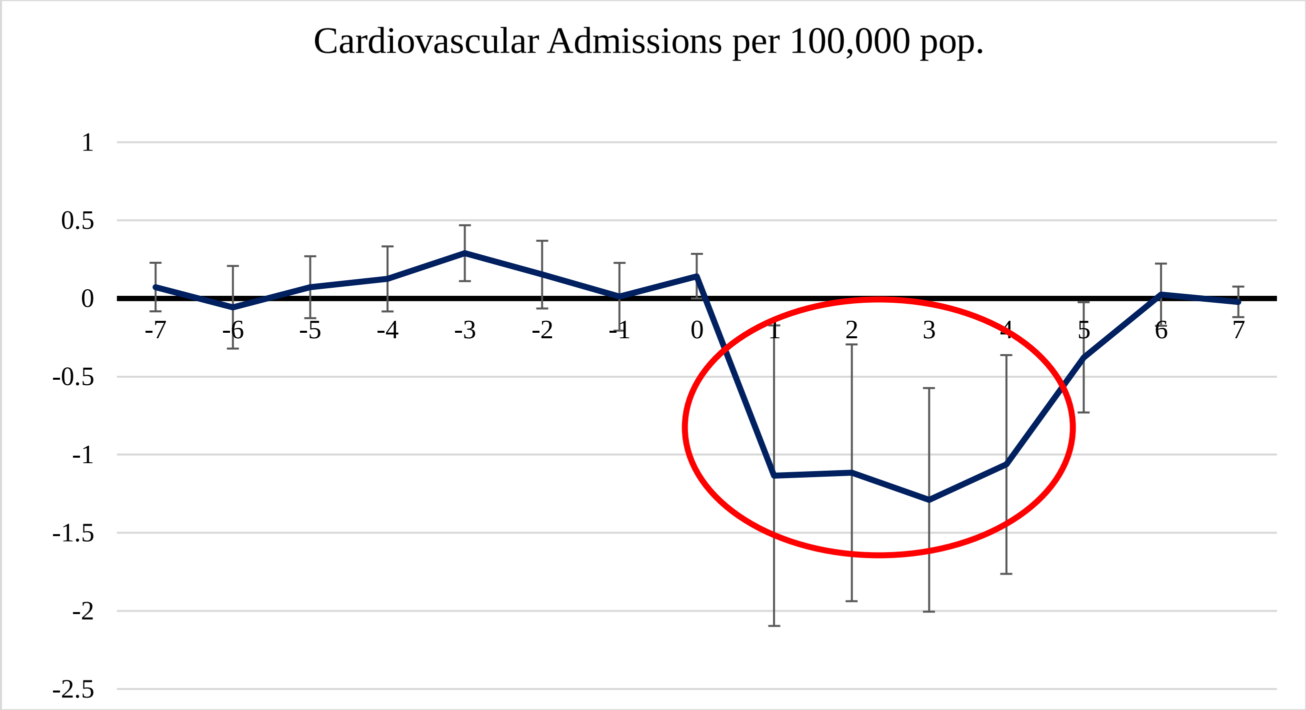


Figure 2a, b: Hospital Census:
Effects of Fall DST Transition on Total and Cardiovascular Hospital Admissions, 2000-2008





<u>Table 1: BRFSS:</u>
Effects of Fall DST Transition on Sleep and Staying Awake

	(1) Hours of Sleep	(2) Hours of Sleep	(3) At least once in past 30 days: Unintentionally fell asleep during the day	(4) At least once in past 30 days: Unintentionally fell asleep during the day
Day of Transition (End of DST)	0.265 (0.079)		-0.061 (0.054)	
Week of Transition (End of DST)		0.182 (0.069)		-0.044 (0.022)
Controls				
State FE	X	X	X	X
Halloween	X	X	X	X
Day of Week * Month FE	X	X	X	X
Month * Year FE	X	X	X	X
Linear & quad. time trend	X	X	X	X
Socioeconomic covariates	X	X	X	X
Mean of dep. Var.	*7.07*	*7.07*	*0.35*	*0.35*
R^2	0.06	0.06	0.07	0.07
Observations	10,833	10,833	10,833	10,833

Notes: Standard errors in parentheses are clustered at the date level. Regressions are probability-weighted. *Day of Transition* is an indicator variable that equals 1 if the interview is on the day of DST transition in the fall. *Week of Transition* is an indicator that equals 1 if the interview is on the Sunday of DST transition or one of the following 6 days. In 2009, six states (Georgia, Hawaii, Illinois, Louisiana, Minnesota, and Wyoming) began to include questions about sleep in the BRFSS; this expanded to nine states in 2010 (Arkansas, Connecticut, Delaware, District of Columbia, Hawaii, Minnesota, Missouri, Nevada, and Oregon). The column headers describe the dependent variables used in each column; columns (1) and (2) have values between 0 and 24; columns (3) and (4) use binary measures. The summary statistics of the dependent variables are in Table A1. Each column is one model as in Equation (1).

<u>Table 2: Hospital Census:</u>
Effects of Fall DST Transition on Hospitalizations by Disease Type

	All cause admission rate (1)	**Cardiovascular admission Rate (2)**	**Heart attack rate (3)**	**Injury admission rate (4)**	**Suicide attempt rate (5)**	**Drug Overdosing (6)**
Week of Transition	-4.956	-0.719	-0.088	-2.712	-0.028	-0.004
(End of DST)	*(1.1139)*	*(0.1589)*	*(0.02611)*	*(0.6869)*	*(0.0128)*	*(0.0055)*
Controls						
County FE	X	X	X	X	X	X
Easter & Vacation FE	X	X	X	X	X	X
Day of Week * Month FE	X	X	X	X	X	X
Month*Year Fixed Effects	X	X	X	X	X	X
Linear & quadr. time trend	X	X	X	X	X	X
Socioeconomic covariates	X	X	X	X	X	X
Mean of dep. variable	*59.77*	*9.53*	*1.59*	*57.56*	*0.09*	*0.32*
R^2	0.8469	0.5675	0.1510	0.2067	0.0179	0.0008
Observations	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***

Note: Standard errors are in parentheses and two-way clustered at the county and date level. *Week of Transition* is an indicator variables that equals 1 if the interview date is on the DST Sunday or one of the following 6 days. Table B1 lists the dependent variables as displayed in the column header. Each column is one model as in Equation (1). All admission rates are per 100,000 except for *Injuries*, *Suicides* and *Drug Overdosing* (per 1,000,000).

Appendix A: BRFSS

Figure A1: **Sample Selection of Main Models—Extracting 6 Weeks around DST Change**

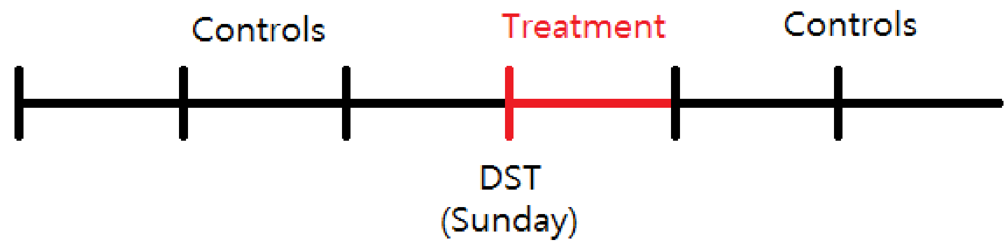


Figure A2: **BRFSS Observations by Month-of-Year**

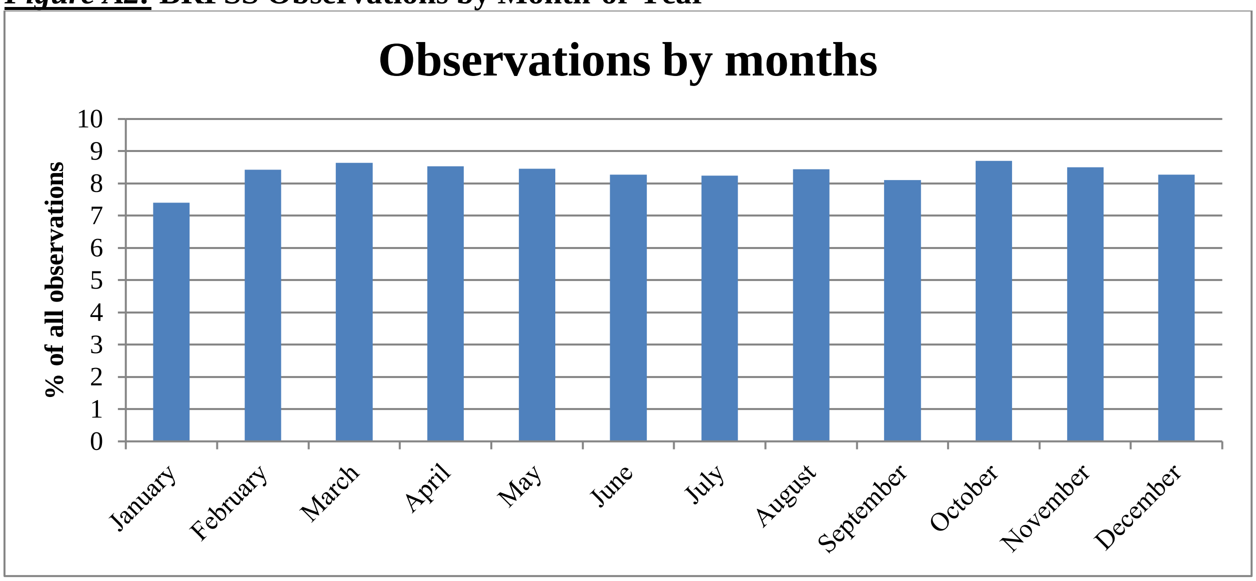
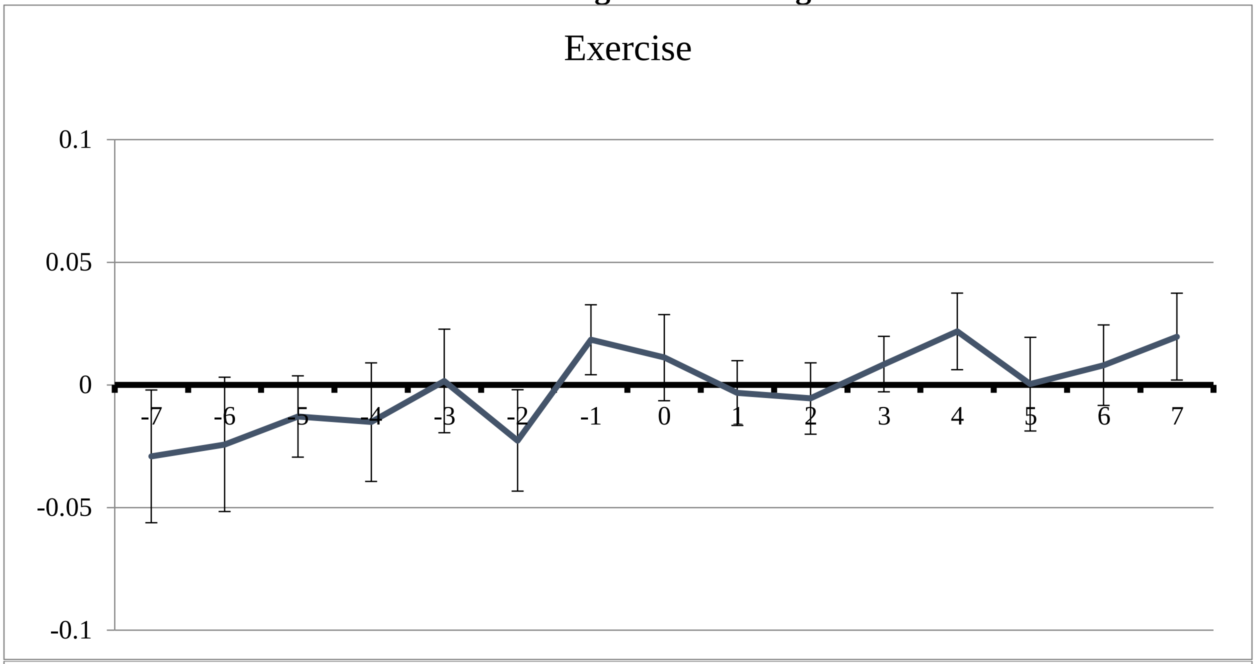


Figure A3a, b: BRFSS Placebo Test:
Effects of Fall DST Transition on Exercising and Receiving Flu Shot



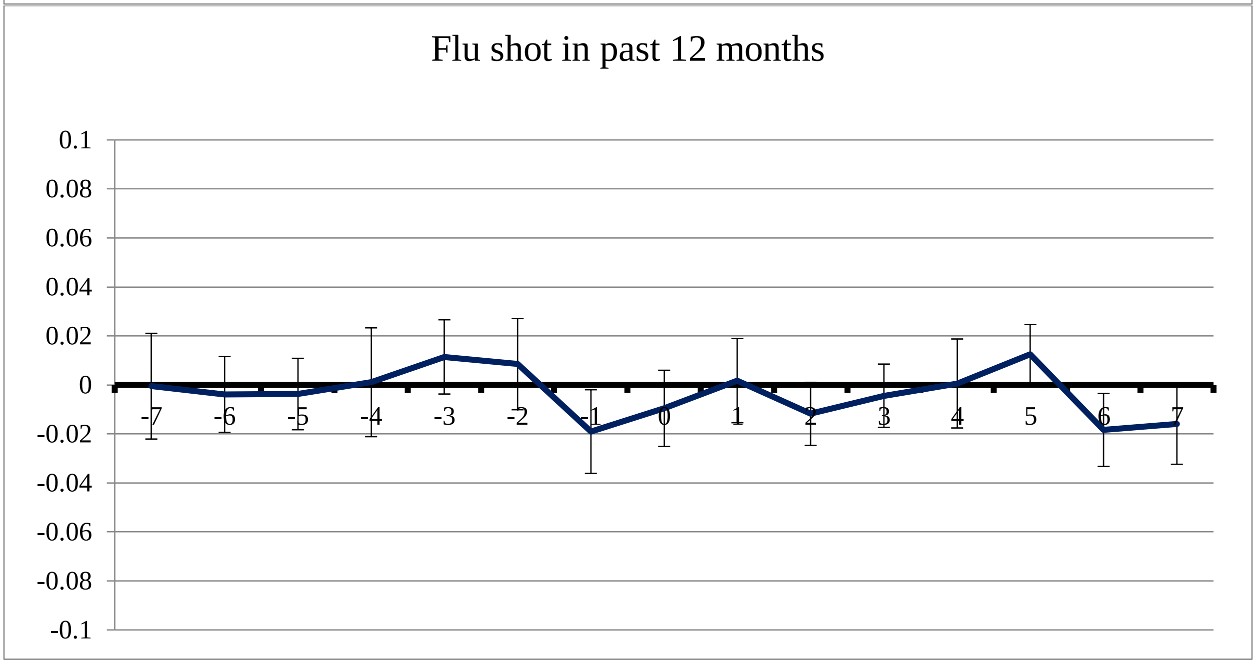


Table A1: **BRFSS Descriptive Statistics**

	Mean	***Std. Dev.***	***Min.***	***Max.***	***Obs.***
Dependent Variables					
Hours of sleep	7.072	1.389	1	24	10,833
Unintentionally fall asleep					
At least 1 day in past 30 days	0.346	0.476	0	1	10,833
Demographic Characteristics					
Age	52.4	17.4	7	99	421,101
Female	0.614	0.487	0	1	421,101
White	0.829	0.377	0	1	421,101
African American	0.087	0.282	0	1	421,101
Married	0.556	0.497	0	1	421,101
Never married	0.129	0.335	0	1	421,101
Number of Children in Household	0.622	1.076	0	24	421,101
Educational Characteristics					
Lower Than Secondary Degree	0.037	0.188	0	1	421,101
Secondary Degree	0.365	0.482	0	1	421,101
Tertiary Degree	0.596	0.491	0	1	421,101
Labor Market Characteristics					
Employed for wages	0.467	0.499	0	1	421,101
Self-employed	0.089	0.284	0	1	421,101
Unemployed	0.044	0.206	0	1	421,101
Retired	0.239	0.427	0	1	421,101

Source: BRFSS, 2001-2010, own calculations and illustration.

Table A2: **BRFSS Balancing Properties between Treatment and Control Weeks, 2001-2010**

Demographic Characteristics	***Week of DST (treatment group) Mean***	***Neighboring weeks (control group) Mean***	***Normalized Difference***
General health	2.589	2.512	0.049
Excellent health	0.183	0.197	-0.025
Fair or Poor health	0.203	0.178	0.044
Age	54.8	51.8	0.118
Female	0.633	0.610	0.034
White	0.845	0.825	0.038
African American	0.085	0.087	-0.006
Married	0.549	0.558	-0.016
Never married	0.117	0.131	-0.032
Number of Children in Household	0.560	0.636	-0.050
Educational Characteristics			
Lower Than Secondary Degree	0.037	0.036	0.004
Secondary Degree	0.379	0.362	0.024
Tertiary Degree	0.582	0.599	-0.025
Labor Market Characteristics			
Employed for wages	0.411	0.479	-0.097
Self-employed	0.084	0.090	-0.014
Unemployed	0.045	0.044	0.005
Retired	0.287	0.228	0.096
N	***79,877***	***341,224***	**-**

Note: The last column shows the normalized difference which has been calculated according to $\Delta s = (\bar{s}_1 - \bar{s}_0)/\sqrt{\sigma_1^2 + \sigma_0^2}$, with $\bar{s}_1$ and $\bar{s}_0$ denoting average covariate values for treatment and control group, respectively. σ^2 denotes the variance. As a rule of thumb, normalized differences exceeding 0.25 indicate non-balanced observables that might lead to sensitive results (Imbens and Wooldridge, 2009).

Appendix B: German Hospital Census

Figure B1: Hospital Census Full Sample:
Effects of Fall DST Transition on Total Admissions, Full Sample, 2000-2008

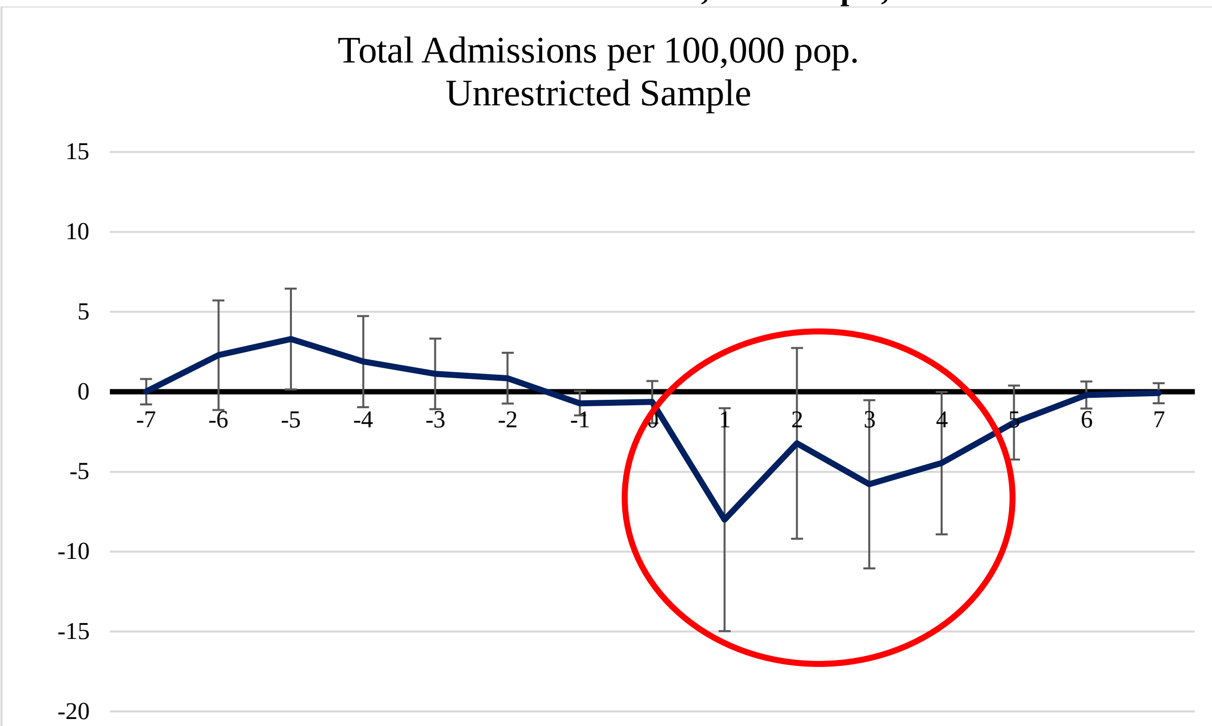


Figure B2a,b: Hospital Census:
Effects of Fall DST Transition on Heart Attacks and Injuries, 2000-2008

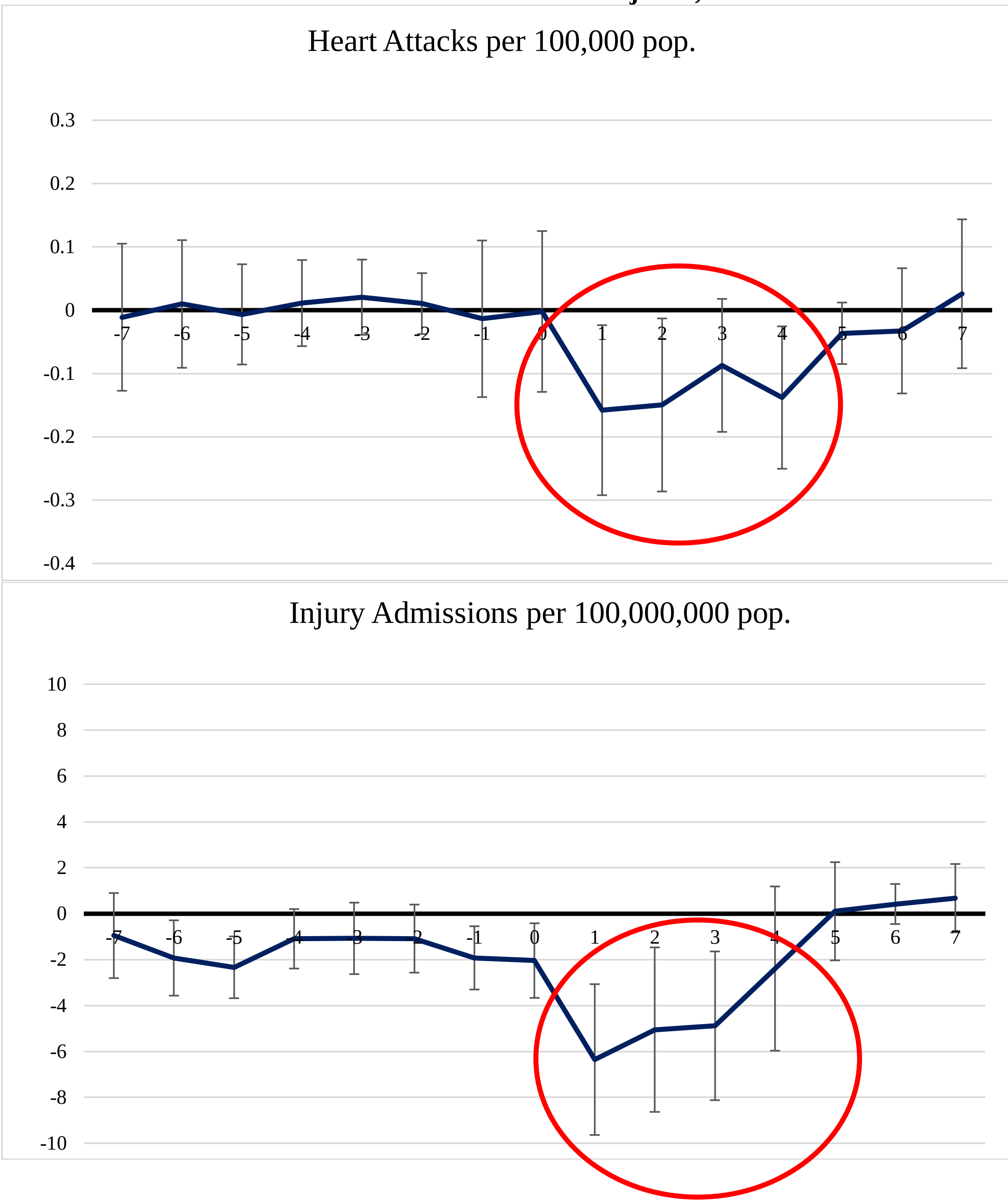
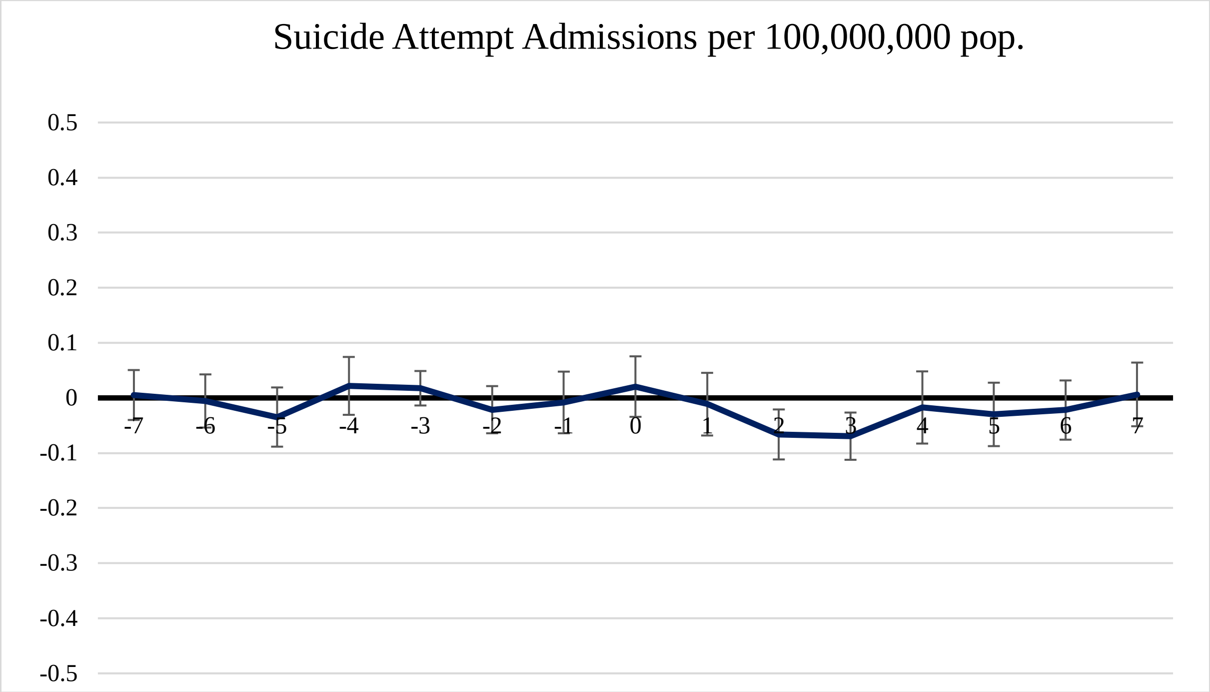


Figure B3a,b: Hospital Census:
Effects of Fall DST Transition on Suicide Attempts and Drug Overdosing, 2000-2008



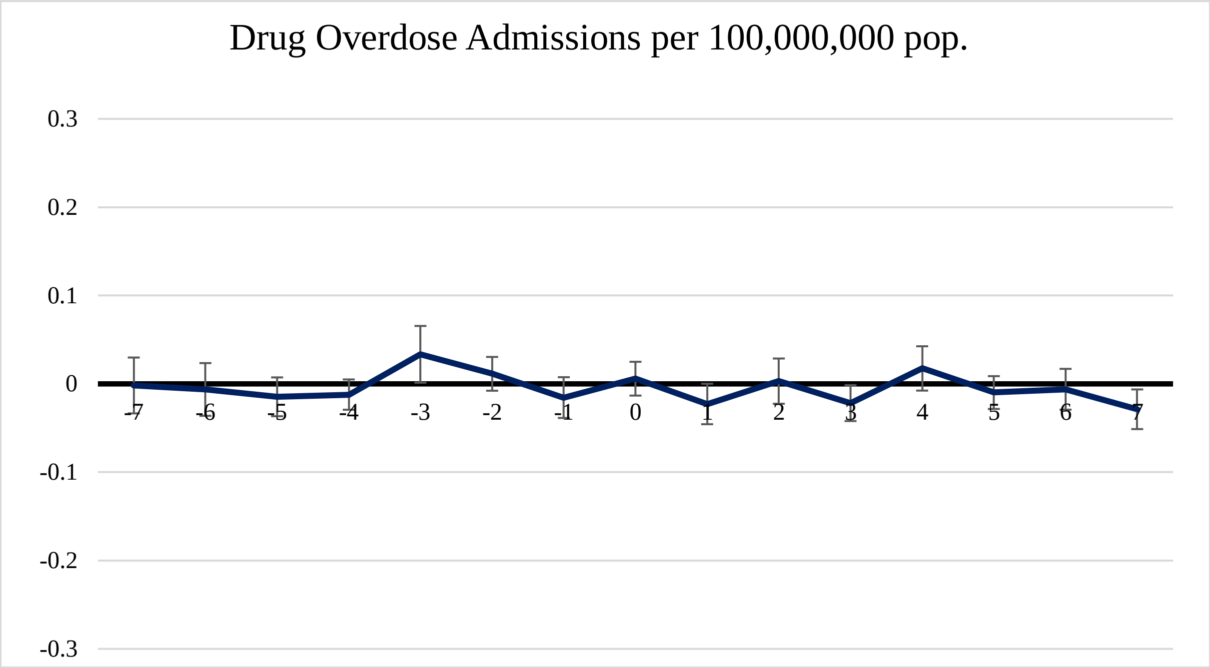


Figure B4: Hospital Census

Permutation Test of Placebo Effects as Compared to DST Week, 2000-2008

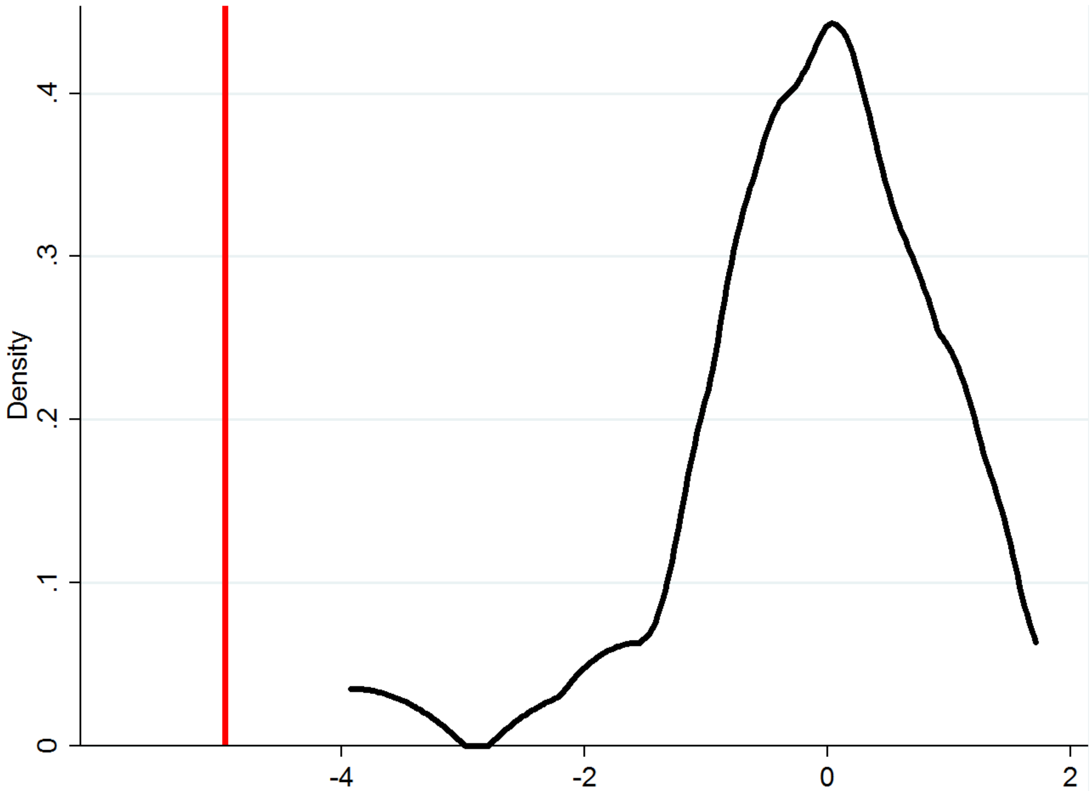


Table B1: **German Hospital Census Descriptive Statistics**

	Mean	***Std.Dev***	***Min.***	***Max.***	***Obs.***
Dependent Variables					
Total admission rate per 100,000	59.768	25.734	N/A	N/A	336,604
Cardiovascular admission rate per 100,000	9.534	4.953	N/A	N/A	336,604
Heart attack admission rate per 100,000	1.591	1.403	N/A	N/A	336,604
Injury admission rate per 1 million	56.557	26.660	N/A	N/A	336,604
Suicide attempt rate per 1 million	0.322	1.675	N/A	N/A	336,604
Drug overdosing rate per 1 million	0.089	0.859	N/A	N/A	336,604
Socio-Demographic Individual Controls					
Female	0.542	0.067	0	1	336,604
Surgery needed	0.372	0.148	0	1	336,604
Died in hospital	0.025	0.023	0	0.5	336,604
Private hospital	0.118	0.181	0	1	336,604
Age Group 0-2 years	0.062	0.047	0	0.5556	336,604
….	…	…	….	…	336,604
Age Group 65-74 years	0.016	0.018	0	0.3333	336,604
>74 years	0.003	0.008	0	0.5	336,604
Annual County-Level Controls					
Hospital per county	4.819	5.469	0	76	336,604
Hospital beds per 10,000	1204.02	1574.54	0	24,170	336,604
Unemployment rate in county	10.37	5.29	1.6	29.3	336,604
Physicians per 10,000	153.96	53.18	69	394	336,604
GPD per resident (in Euro)	25,235	10,219	11,282	86,728	336,604
Seasonal Controls					
Holy Thursday, Good Friday, Easter Sunday, Easter Monday (each)	0.010	0.101	0	1	336,604
Easter Vacation	0.121	0.326	0	1	336,604
Fall Vacation	0.098	0.297	0	1	336,604
Week Begin DST	0.086	0.281	0	1	336,604
Week End DST	0.086	0. 281	0	1	336,604

Source: German Hospital Census 2000-2008, Federal Institute for Research on Building, Urban Affairs and Spatial Development (2012). The hospital admission data are aggregated at the county-day level and normalized per 100,000 population. Note that both nominator and denominator refer to the county of residence. The data excludes military hospitals and hospitals in prisons Note that German data protection laws prohibit us from reporting min. and max. values. The socio-demographic individual controls are also aggregated at the county-day level. The seasonal controls only vary between days, not across counties. The annual county-level controls vary between the counties and over years, but not within years. Between 2000 and 2008, Germany had up to 468 different counties. Mostly, due to mergers and reforms of the administrative boundaries, the number of counties varies across years.

Linking Hospital with Official Weather Data

Weather Data. The weather data are provided by the German Meteorological Service (*Deutscher Wetterdienst (DWD)*). The DWD is a publicly funded federal institution and collects information from hundreds of ambient weather stations which are distributed all over Germany. Daily information on the average temperature, rainfall, hours of sunshine and cloudiness from up to 1,044 monitors and the years 2000 to 2008 are used.

We extrapolate the point measures into space using inverse distance weighting. This means that the measures for every county and day are the inverse distance weighted average of all ambient monitors within a radius of 60 km (37.5 miles) of the county centroid (Hanigan et al. 2006).

Socioeconomic Background Data. Because the *Hospital Admission Census* only contains gender and age, we link yearly county-level data with the hospital data. We merge in county-level information on *GDP per resident*, the *unemployment rate*, the *number of physicians per 10,000 pop.*, the *number of hospitals in county* as well as the n*umber of hospital beds per 10,000 pop*

Table B2: **Effects of Fall DST Transition on Admissions, 2000-2008, by Weather Conditions**

	All cause admission rate			
	(1)	**(2)**	**(3)**	**(4)**
	Temp.	**Rainfall**	**Sunshine**	**Cloudiness**
DST * [column header]	-0.238	0.099	-0.209	0.446
	(0.23)	(0.139)	(0.343)	(0.508)
DST	-3.133	-5.183	-4.481	-7.606
(3am → 2am in fall)	(1.867)	(1.206)	(1.196)	(3.388)
Controls				
Easter, Halloween, Vacation FE	X	X	X	X
Day of Week * Month FE	X	X	X	X
Month * Year FE	X	X	X	X
Linear & quadratic trend	X	X	X	X
Socioecon. covariates	X	X	X	X
Weather and pollution controls	X	X	X	X
R^2	0.837	0.837	0.837	0.837
Observations	336,604	336,604	336,604	336,604

Notes: Standard errors in parentheses are two-way clustered at the date and county level. *DST* are indicator variables equal to 1 if the interview is on the DST Sunday or one of the following 6 days. The dependent variable is the all cause hospital admission rate per 100,000 pop. at the daily county level (Appendix, Table B1). Appendix B describes the weather measures and how they are linked to the Hospital Census on a daily county-level basis. Each column is one model as in Equation (2).

Appendix C: Outcome Variables and Measurement

This paper uses self-reported measures on sleep and tiredness from the BRFSS along with administrative hospital admission data from Germany. Together these represent a broad set of outcome measures from different countries to validate our findings.

First, some BRFSS outcome measures refer to "in the last 30 days", which may introduce measurement error and a non-straightforward interpretation. Because our standard approach assigns respondents in weeks t+2 and t+3 to the control group status (Figure A1), our estimates would be downward biased as the retrospective 30-day responses would be affected by DST as well. In practice, however, we expect recall biases and that respondents overweigh days closer to the interview day. In robustness checks, we assign respondents in weeks t+2 and t+3 to the treatment group and the results hold up.

Second, with respect to the administrative hospitalization data: German geography, combined with the institutional setting of the German health care system, makes it very plausible that variations in hospitalizations represent serious population health effects. Germany has 82 million residents living in an area which has roughly the size of the U.S. state Montana. Thus, the average German population density is seven times higher than the U.S. population density and 231 vs. 32 people per km^2 (U.S. Census Bureau, 2012; German Federal Statistical Office, 2017). The hospital bed density is also much higher. Per 100,000 population, Germany has 824 hospital beds, while the U.S. has 304 beds (OECD, 2017). Hence, geographic hospital access barriers, such as travel distances, are low in Germany. Moreover, the German uninsurance rate is below 0.5%. The public health care system covers 90% of the population and copayment rates in the public scheme are uniform and low. The overwhelming majority of hospitals can be accessed independently of insurance status and free choice of providers exist (no provider networks).

Appendix D: Monetizing Health

Table D1: **Decomposing and Monetizing Benefits of Additional Sleep**

	Productivity Effects	**Health Effects**			**Mortality Effects**
	Gibson and Schrader (2018)	German Hospital Census (Fig. 2a, Tab. 2)			Smith (2016)
	Increase in Work Productivity	**Health Care Costs**	**Labor Productivity**	**QALYs**	**Avoided Deaths**
	+1.1% at $230 daily wage * 4 days	€500 per day *4 days	€150 per day *4 days	($100K/365) *0.5 *4 days	30 fatalities in U.S. (0.09 per 1M pop.)
Benefit for individual	***=$10***	***=€2000***	***=€450***	***=€550***	*$5M per VSL
	*10% sleep deprived employees: (0.1*161M)/320M	*100	*(100/3)	*100	
per 1M pop.	**=$500K**	**=€200K**	**=€15K**	**= €55K**	**=$450K**