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

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Abstract

Chronic sleep deprivation is a significant and understudied public health issue. Using 1.9 million survey responses from the United States and an administrative census of 160 million hospital admissions from Germany, we study the relationship between sleep and health. Our empirical approach exploits the end of Daylight Saving Time in a quasi-experimental setting on a daily basis. First, we show that setting clocks back by one hour in the middle of the night significantly extends people's sleep duration. Second, using an event study approach, we show that this nighttime extension reduces the share of people who involuntarily fall asleep during the day. In addition, we find significant health benefits via sharp reductions in hospital admissions. For example, hospitalizations due to cardiovascular diseases decrease by ten per day, per one million population. This effect persists for four days after the time shift. Admissions due to heart attacks and injuries also exhibit the same characteristic four-day decrease. We provide a series of checks to rule out alternative mechanisms, and show that increasing sleep produces the human capital improvements. Finally, we discuss the benefits of additional sleep for the sleep-deprived as well as policy implications for nudging people to sleep more. Our findings illustrate the importance of public policies that target sleep deprivation.

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

JEL codes: H41, I18, I31

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1. INTRODUCTION

Despite the abundance of human capital studies (e.g. Cunha and Heckman, 2007), the one single activity that humans spend most of their time doing—sleeping—has received very little attention in the public policy and economics literature. At the same time, there is ample but largely overlooked evidence that "Insufficient Sleep Is a Public Health Epidemic" (CDC 2014). The Centers for Disease Control and Prevention (CDC) recommend at least 7 hours of sleep per night to avoid increased risks of high blood pressure, coronary heart disease, stroke, frequent mental distress, and all-cause mortality. However, in a recent report, the CDC finds that a third of the U.S. population does not get a healthy amount of sleep, especially blacks and multiracial respondents (Liu et al. 2014). Knutson et al. (2010) report a significant increase of "short-sleepers" (less than six hours) between 1975 and 2006 in the United States.

As Chicago-Economist 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. As another piece of evidence, the global sleep-aid market is growing rapidly and is estimated to be worth $80 billion by 2020. The United States alone provides 40 million sleeping pill prescriptions per year and about 2800 "sleep labs" exist (CDC, 2013; Persistence Market Research, 2015; DiSalvo, 2015). This paper contributes to the social science literature by examining the role of sleep for human capital. It is one of the first papers in the public policy and economics literature to investigate in a real world setting how sleep can causally affect one integral component of human capital: health.

In one of the few sleep studies in economics, Biddle and Hamermesh (1990) show that higher (instrumented) wages 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. Similarly, Gibson and Schrader (2018) exploit geographic variation in sunset timing to identify positive wage returns to sleep. And Billari et al. (2018) 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). 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).[1] 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 optimum 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. Today, all countries in the European Union, the great majority of the U.S. states and Canadian provinces, as well as 40 other countries such as Mexico, Chile, Israel, and Iran observe DST.

[1] In one of the few randomized controlled trials involving 48 healthy young adults, Van Dongen et al. (2003) find significantly reduced cognitive performance after restricting sleep periods to 4-6 hours per night over two weeks (also see Carskadon et al. 1981 or Drake et al. 2001). In another experimental study, Spaeth et al. (2013) find that sleep-restricted subjects consume more calories and gain more weight than the control group. Using natural experiments, Carrell et al. (2011) find that later school start times increase test scores.

Our identification strategy focuses on the time shift in the fall when the clocks "fall back" in the middle of the night, and thus extend the nighttime by one hour. The additional hour generated by the time shift creates a plausibly exogenous extension of sleep duration, which we exploit to estimate the causal effect of getting more sleep on population health. Using the Behavioral Risk Factor Surveillance System (BRFSS)—a large U.S. survey—we find a significant increase in sleep durations as a result of the nighttime extension. In addition, reinforcing our identification strategy, we find a significant decrease in the share of people who unintentionally fell asleep during the day.[2]

We then use the German Hospital Census to estimate the impact of the fall DST transition on health. Exploiting all 160 million hospitalizations that occurred in Germany between 2000 and 2008 allows us to comprehensively control for seasonal and weekday confounders while maintaining enough statistical power to precisely identify health effects at a daily level. Our findings show significant, sharp decreases in hospital admissions as a result of the nighttime extension. Hospitalizations due to cardiovascular diseases decrease by ten per one million population, per day. This decrease lasts for four days. Moreover, the results are consistent across different disease categories and robust to multiple specifications.

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 link with current sleep

[2] Our findings are broadly consistent with the medical and psychology literature on DST transitions and sleep (cf. Lahti et al, 2008; Barnes and Wagner, 2009; Janszky et al. 2012; Jiddou et al. 2013). Interestingly, we do not find that people sleep significantly less when the clocks "spring forward" in spring and offer a few possible explanations. First, those who are most affected by the spring time shift may be less likely to respond to the survey and thus be underrepresented in our data. Second, to the extent that vulnerable people follow salient medical advice in the media, they may adjust their bedtime schedules to ensure that they sleep enough. Showing this is beyond the scope of this paper, which focuses on the time shift in the fall.

(such as receiving a flu shot in the previous year) and find no effect, providing additional support of the sleep mechanism. Moreover, we discuss alternative mechanisms through which the DST transition might affect health, and show that most of these mechanisms go in the opposite direction of the sleep mechanism. In the last part of the paper, we categorize and monetize the economic benefits of getting more sleep for the sleep deprived. Our findings have important implications for public health programs and policies to reduce sleeplessness and sleep deprivation.

The next section briefly describes the data. Section 3 outlines the empirical methodology. Section 4 presents and discusses the findings and Section 5 concludes.

2. DATASETS

We employ a two-step approach in our analyses. First, we use a large U.S. survey to test if people sleep more when the night extends by one hour through the DST transition. Second, we utilize administrative hospital data from Germany to test for the impact of increased sleep on hospitalizations across various disease categories.

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

The BRFSS is a large annual telephone survey of U.S. adults aged 18 and above, which is 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. It is, by design, representative of state populations. In 2009, several states have started to include questions on sleep duration in the survey; this question expanded to all states between 2013 and 2016.

We focus on this period from 2013 to 2016, which includes 1.9 million survey responses in total. As shown in Figure 1, we extract six weeks around the time shift to ensure that the responses are conducted at a similar time of the year, such that seasonalities are not a major concern. Doing that, we obtain 174,503 survey responses in the main sample. Further, we include a robust set of time controls in our analysis, including month and day-of-week fixed effects.

Construction of Main Dependent Variables

The sleep duration question reads: *"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."* The answers are integers between 0 and 24. People on average report 7 hours of sleep, with a standard deviation of 1.5 (Table A1, Appendix). 32% report having slept 6 or fewer hours. This illustrates an alarming level of sleep deprivation in the U.S.

However, the sleep question does not explicitly ask for the duration of sleep *last night.* Given the phrasing "24-hour period" and the emphasis on thinking about the time actually spent sleeping, the answers will likely be a weighted average of subjects' sleep duration in the very recent past, with significant weight given to the previous night's sleep. This measurement error will downward bias our estimates, as the true effect of the DST transition on sleep will only partially be reflected in the subjects' responses. Put differently, our estimates will be a lower bound of the true effect.

We supplement the estimation with another survey question about tiredness during the day. Between 2009 and 2010, the BRFSS asked: "*During the past 30 days, for about how many days did you find yourself unintentionally falling asleep during the day?*" The responses are integers between 0 and 30. We convert them into a binary variable of whether the subject has unintentionally fallen asleep in the past 30 days. On average, 35% of people report having unintentionally fallen asleep in the past 30 days (Table A1, Appendix).

If the nighttime extension induces people to sleep more, we should see an increase in the sleep duration and a similar decrease in people falling unintentionally asleep. We discuss issues related to measurement errors of both variables in Appendix B.

Daylight Saving Time in the United States

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. Our empirical strategy only uses states that observe DST.

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. 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 the BRFSS, our working dataset focuses on the six weeks around the time shift (Figure 1). This main sample has 336,604 county-day observations.[3] We leave the data at the county-level and do not further aggregate up to the national level for a few reasons. This allows us to stratify the effects by county characteristics. Another reason is that we lose statistical power when aggregating up to a time series at the national level.

[3] 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.

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 A1, 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 A1). 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)* as well as the *respiratory* (J00-J99), *metabolic* (E00-E90), *neoplastic* (C00-D48), and *infectious admission rate* (A00-B99). We also test for changes in *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 a plausibly exogenous extension of night sleep created by the nighttime extension through the end of DST in the fall. The transitions occur on different dates each year. Our large datasets allow us to comprehensively control for seasonal confounders, weekday effects, and yet still precisely estimate the 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 outcome variable. For example, using the German Hospital Census it stands for admission rates in county *i* on day *d*. *DST* is a vector of fifteen daily dummies around the end of DST.

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 Halloween.[4]

Due to the relevance of day-of-the-week (DOW) effects, 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 ends 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

[4] In Germany, official school vacations vary at the level of the 16 states by date, and also in lengths. In spring, they are typically around Easter but vary from early March until the end of April. They vary in length from one up to three weeks, depending on the state. Fall vacations lie between the beginning of October and mid-November, and vary by state, both in term of time and length. In the U.S., we include a dummy for Halloween, which occurs on October 31st each year. Halloween is only a very recent phenomenon in Germany and has no tradition. However, the German findings are robust to including Halloween fixed effects.

or individual-level socio-demographics ($X_{id}'\gamma$) and persistent differences across states or counties (μ_s).

Because it is unlikely that county-level 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 cross-sectional BRFSS, we cluster standard errors only at the date level (as it is no panel). All BRFSS regressions are probability weighted.

3.2 Identification

The key idea of our identification strategy is that DST transitions create plausibly exogenous variations in people's sleep duration by extending the nighttime by one hour. These transitions are arguably exogenous to individuals. 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 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 temperatures and hours of sunshine.

Sample Selection and Definition of Treatment and Control Groups

As illustrated by Figure 1, we restrict our main sample to three weeks before and three weeks after the time shift. However, the results are robust to including all 52 weeks of the year as we will show. 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 (cf.

Hausman and Rapson, 2017), where the post-treatment outcomes are compared to that of the pre-treatment, conditional on all covariates shown in Equation (1), see for example Doleac and Sanders (2015); the results are available upon request.

[Insert Figure 1 about here]

Comparing the covariate means for the week of the DST transition—our "treatment week"—to the "control weeks" before and after the treatment week yields no evidence for imbalances. 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 (see Jin and Ziebarth, 2018).

4. RESULTS

4.1 Effects of the Nighttime Extension on Sleep Duration

This section uses the BRFSS data to estimate the impact of the nighttime extension on sleep duration. Figure 2 compares reported sleep for those who were interviewed between Thursday and Wednesday around the DST transition, to similar respondents who were interviewed a week earlier and later. Figure 2 plots the results by day of the week. The black vertical line represents the DST transition that occurs for the dark blue line. As seen, the sleep duration for the control group remains relatively stable across the seven days of the week. For the treatment group, the sleep duration exhibits a similar trend prior to the DST transition. However, reported sleep sharply increases on the Sunday of the nighttime extension and lasts for three days. In particular, the difference on Monday is statistically significant at the 1% level. On Tuesday, it is significant at the 5% level.

[Insert Figure 2 about here]

While statistically significant, one may notice that the differences are relatively small. This is likely due to measurement errors that downward bias our results (Section 2.1 and Appendix B).

[Insert Figure 3 about here]

Next, we test if the results are robust to the inclusion of seasonal controls as shown in Equation (1). Figure 3a plots the regression coefficients of the fifteen daily dummies in Equation (1), with hours of sleep as dependent variable. The *x*-axis represents the days relative to the nighttime extension (0 is the Sunday of the transition), and the *y*-axis shows the effect on sleep duration. Again, we see a sharp increase in self-reported sleep on the Monday following the transition. This effect persists for several days and are highly consistent with Figure 2. Together they provide evidence that people indeed sleep more when clocks fall back in fall in the middle of the night.

We further corroborate these findings with a few more tests. Figure 3b plots the daily dummies of Equation (1) with *unintentionally fell asleep* as dependent variable. Consistent with our hypothesis, we observe a sharp decline in the share of people who unintentionally fell asleep during the day after the nighttime extension. The decline is immediate and largest on the Sunday of the transition; it persists for about four days before dissipating. The relatively clear pattern is even more reassuring when put into context because the measure is again a weighted average of the participants' reported tiredness in the past 30 days, creating a downward bias in our estimates. The sample is also relatively small due to the limited data availability. Despite these challenges, we find this significant decrease in tiredness following the nighttime extension, providing strong support of our first stage.

[Insert Table 1 about here]

Finally, Table 1 estimates the effect on sleep at the *weekly* level. That is, we run Equation (1) but replace the daily dummies with a binary *week of Transition* indicator that equals one for the week of the transition (from Sunday of the transition until the Saturday after). According to column (1), on average, people sleep an additional 0.026 hours per night during this week. This estimate is statistically significant at the 1% level. Note that this is a population average, which is also averaged over the entire DST week. Figures 2 and 3 suggest that the effect is concentrated during the first days after the transition; it is likely also concentrated among the sleep deprived.

Column (2) turns to our measure of tiredness during the day. People are, on average, 4.4 percentage points less likely to report having unintentionally fallen asleep during the treatment week, which equals a 13% decrease. The estimated weekly effect is statistically significant at the 5% level.

To summarize, while our sleep measures are self-reported, the results consistently provide evidence in support of our first stage, namely that the nighttime extension effectively extends people's sleep. Our sleep variables are likely downward-biased, but we can still precisely estimate the treatment effects due to large sample size. Next, we investigate the second-stage, namely the reductions in hospital admissions subsequent to the nighttime extension.

4.2 Effects of the Nighttime Extension on Hospital Admissions

Table 2 shows weekly admission estimates by disease groups for Germany. Each column is one model as in Equation (1) but 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 highly 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.

[Insert Figures 4 and 5 about here]

Next, we zoom in and plot the daily estimates of Equation (1) in event study graphs. Figure 4a shows *all cause admissions* per 100,000 population and Figure 4b *cardiovascular admissions* per 100,000 population. Despite conservative two-way clustering, we are able to identify even daily effects in a very precise manner. Please note that the event study graphs do *not* represent simple descriptive graphs but compare the effects in the treatment group relative to the control group (Figure 1) after having netted out of seasonal and weekday confounders as formalized by Equation (1).

The two event study graphs 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 A1, Appendix) as well as *heart attacks* and *injuries* (Figure 5). The consistency of these patterns for even heart attacks is reassuring.[5]

Finally, we examine hospital admissions due to *drug overdosing*, which arguably has a weaker theoretical link with sleep. Illicit drugs are highly addictive, which limits the extent to which additional sleep can help prevent those who are on the margin of overdose from being hospitalized. As such, we do not expect to see a strong effect on drug-related hospital admissions. Indeed we find no effect, as shown in Figure 6. This reinforces the notion that additional sleep is responsible for the reductions in hospital admissions that we observe for the other disease categories.

[Insert Figure 6 about here]

Moreover, 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 for people who are *on the margin* of being hospitalized. This finding is very consistent with, and underscores, the medical advice that people on the margin of having acute heart failure should get sufficient bed rest (Millane et al. 2000).

4.3 Could Alternative Mechanisms Explain the Health Effects?

Now we discuss alternative mechanisms that could explain the health 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

[5] Note that the German data do not allow us to distinguish between emergency room visits, elective visits and other type of admission. We solely see the primary diagnosis and know that the patient stayed overnight, which excludes ambulatory elective surgeries.

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 7a. In line with Giuntella and Mazzona (2017), we find no evidence that exercising changes as a result of the time change.

[Insert Figure 7 about here]

Next, we stratify the effects by weather conditions using the German Hospital Census. As we explain in Appendix A, 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 A2 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 (Figure 7a), 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 can also affect traffic accidents. This could potentially explain the significant reduction in admissions due to injuries. However, traffic accidents cannot explain why we observe consistent reductions in admissions that are not related to accidents, such as admissions for cardiovascular diseases.

Another potential confounding factor is crime. Doleac and Sanders (2015) show that robberies decrease in the days following the DST transition in spring (when evenings get dark later). They find no significant effects on crime rates in fall. If there was a significant robbery effect, robberies would likely *increase* following the time shift in the fall (because it gets dark sooner), and thus have adverse health effects, opposite what we find.[6]

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, it will result in more admissions, opposite our findings. This mechanism also cannot explain the persistent health effects that we find over four days.

Finally, we estimate placebo regressions. Our first placebo test, using 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 7b shows no impact on this outcome.

[Insert Figure 8 about here]

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 1. 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 until end of the year.[7] As such, we obtain 23 weekly placebo estimates. Figure 8 plots the distribution of these weekly placebo estimates along

[6] 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. According to Doleac and Sanders (2015), in spring, the number of avoided robberies decrease by about 2 per 10 million people. Smith (2016) finds that the spring change leads to 30 more deaths for the entire U.S. These numbers certainly would not bias the survey estimates for the US. As for the hospital admission data, our "Injury Admissions per 1 Million Population" outcome category should capture these effects.

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

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

When considering policies to tackle the growing sleep-deprivation problem, it is important to quantify the potential benefits of encouraging people to sleep more. In this section, we monetize the economic benefits of avoided hospital admissions. We also estimate other benefits based on studies that show improved work productivity (Gibson and Schrader, 2018) and avoided traffic fatalities (Smith, 2016). These calculations are based on several assumptions, but provide a basic framework for such an exercise.

Table 3 shows the estimates. We first monetize the value of avoided hospital admissions from the end of DST in the fall—a policy that generates a one-time extension of sleep every year. Figure 4a implies 100 fewer admissions per 1 million population over four days. Columns (1) to (3) in Table 3 show that the benefits of avoided hospital stays can be decomposed into a per person €2000 for medical costs, €450 for lost labor as well as €550 for lost quality of life during hospital stays.

One can also assess 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 US, this translates into $10 over four days. Assuming that these gains only applies to the ten percent sleep deprived full-time employed Americans, it would sum to $500 thousand per 1 million population (column (4), Table 3).

Finally, Smith (2016) quantifies the number of avoided traffic fatalities with 30 for the entire U.S. (0.09 per 1 million population). Evaluated at $5 million per life saved (Kniesner et al. 2010), we obtain values for saved statistical lives of around $450 thousand per 1 million population (column (5), Table 3).

The total welfare benefits sum to about $1.3 million per 1 million population.

5. CONCLUSION

This paper exploits the quasi-experimental nature of Daylight Saving Time (DST) to assess the health benefits of encouraging people to sleep more. Our findings present an unintended consequence of a policy to save energy. We find that humans sleep significantly more in the short-run when they gain an additional hour at night during the DST transition in fall. Also, the share of people who unintentionally fall asleep during the day drops significantly for four days. Moreover, we find that hospital admissions drop sharply for four days as well. For example, cardiovascular admissions decrease by ten per one million population. We find no effect for placebo outcomes, which have weaker or no theoretical links to sleep, such as drug overdosing or having received a flu shot.

Because exogenous shifters of sleep are very rare in real world settings, our study is one of very few causal studies on the health benefits of sleep. To identify effects, we use a large survey dataset from the U.S. and the census of hospital admissions from Germany. Properly investigating the impact of the nighttime extension on health *on a daily level* requires powerful and representative data. These are crucial to estimate rich econometric specifications that consider weekday effects in addition to general and specific seasonal adjusters.

Our findings have important implications for public policy. Sleep deprivation is becoming a widespread problem in many developed countries—the CDC has recently declared it a “public health epidemic” (CDC 2004). 32% of Americans report sleeping 6 or fewer hours, significantly less than the CDC-recommended minimum of 7 hours. The findings from our study provide compelling evidence of the need to devise policies to reverse the trend of growing sleep deprivation in recent years.

The evidence presented in this paper is also bolstered by other recent economic studies that identify work productivity effects as a result of more sleep (Gibson and Schrader, 2018), decreases in obesity (Giuntella and Mazzonna, 2017), better cognitive skills (Giuntella et al. 2017) or fewer traffic fatalities (Smith, 2016). In the last part of the paper, we attempt to categorize, standardize, and monetize the various benefits that this paper and companion research in economics identifies. 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 benefits can be decomposed into work productivity, hospitalization, and mortality effects.

The main objective of this paper is to provide evidence of a causal relationship between Daylight Saving Time transitions, sleep and health. 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

Figure 1: **Sample Selection of Main Models—Extracting 6 Weeks around DST Transition**

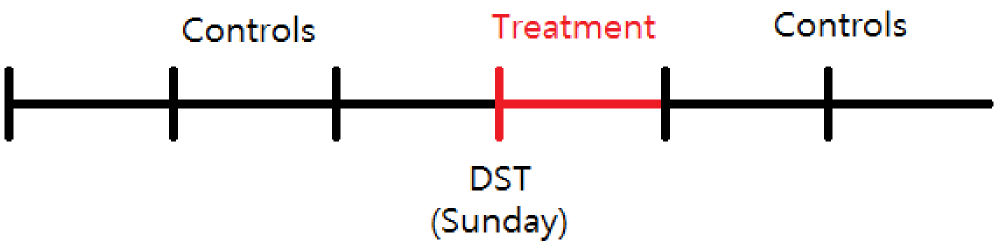
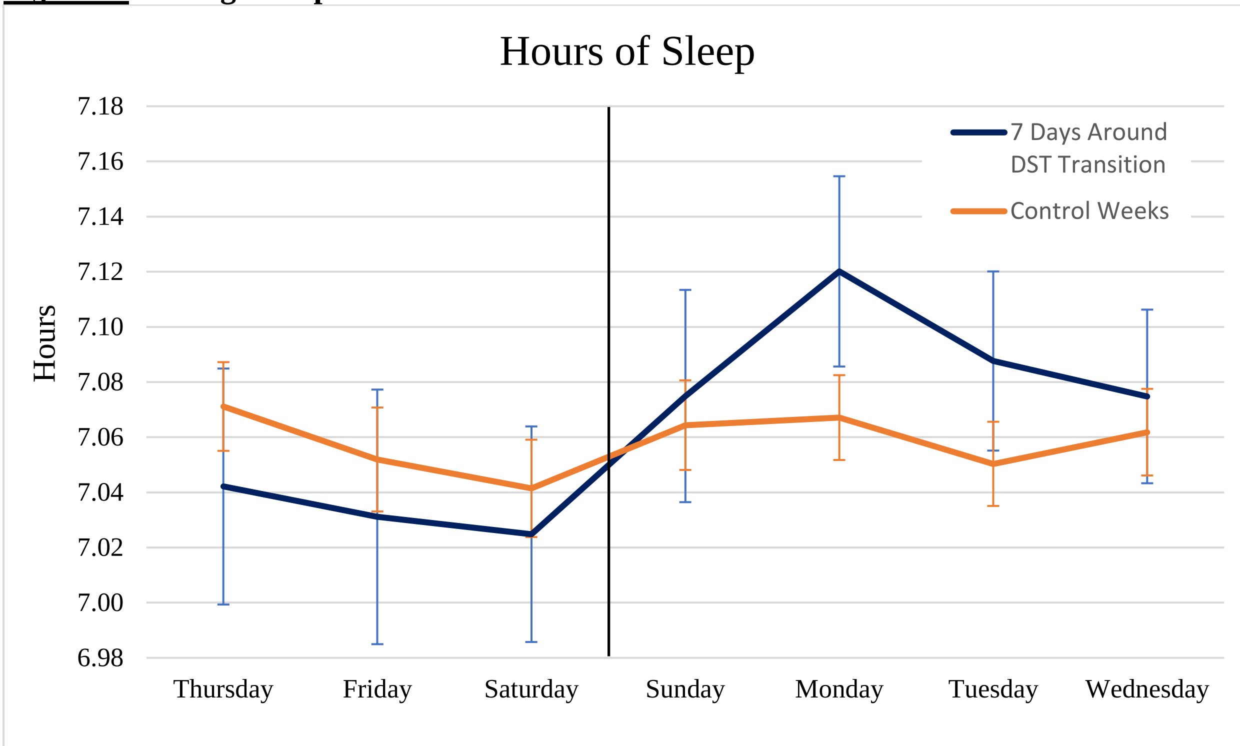
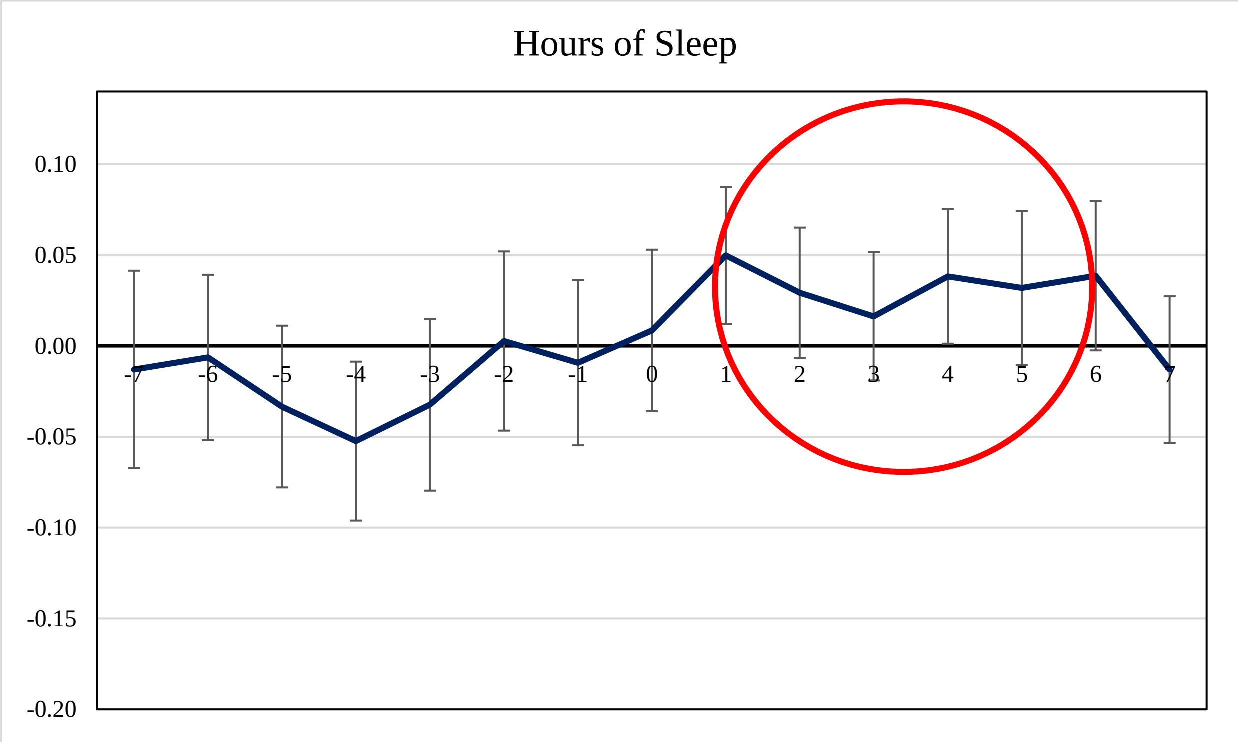


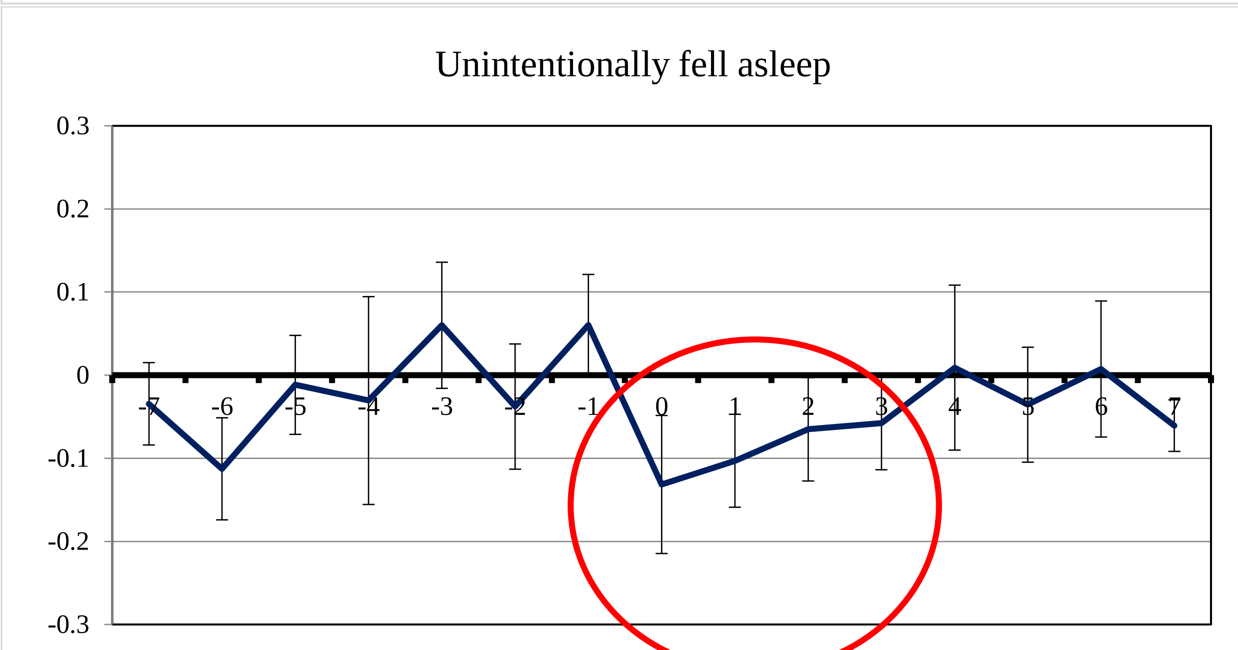
Figure 2: **Average Sleep Duration between Treatment and Control Weeks**



Source: BRFSS, 2013-2016. The red line plots the average daily sleep duration in control weeks (4-21 days before the transition, and 4-21 days after the transition). The dark blue line plots the average daily sleep duration for the 7 days around the transition. The transition occurs Sunday at 2am, represented by the black vertical line.

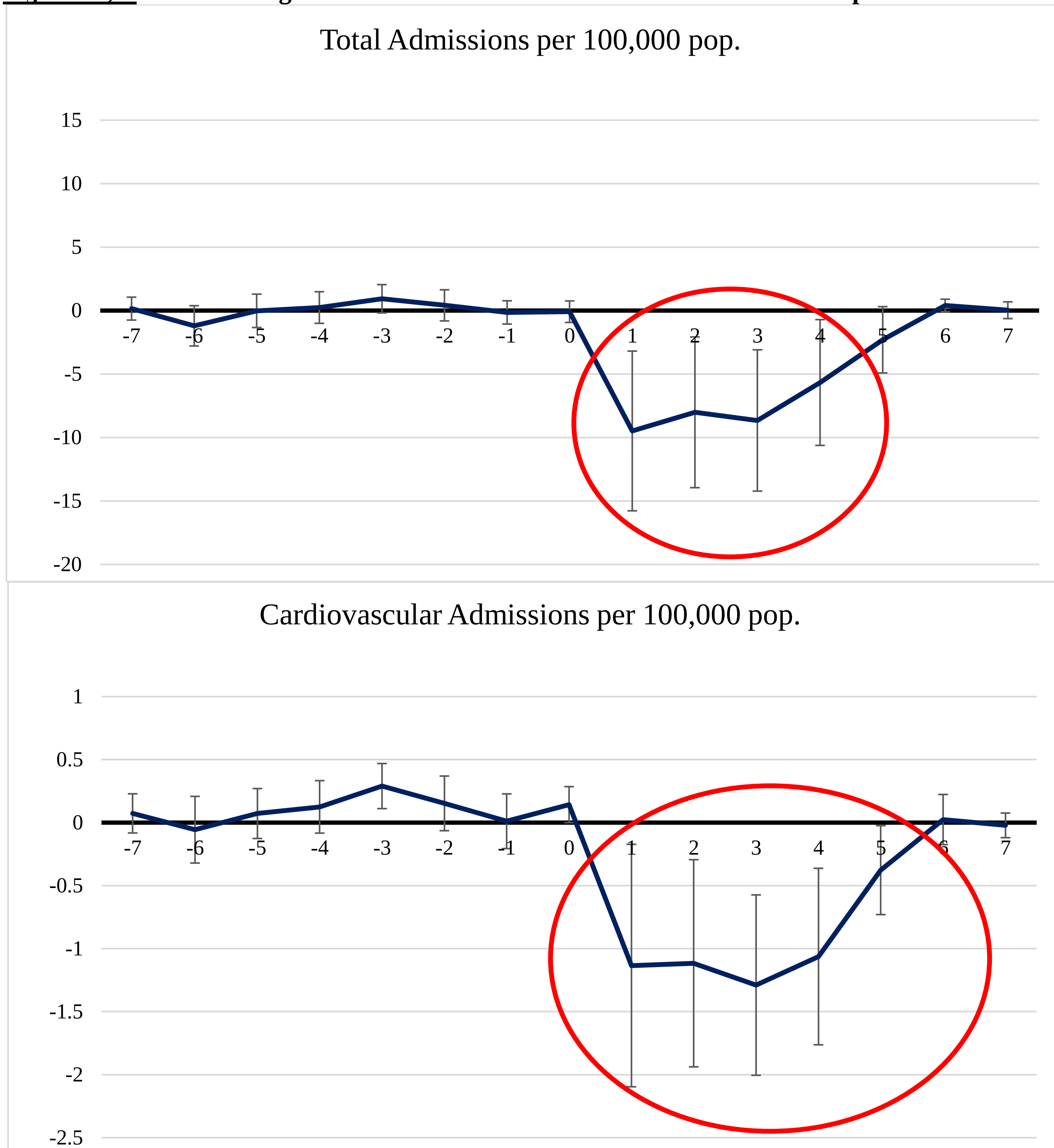
Figure 3a,b: **Effects of Nighttime Extension on Sleep and Unintentionally Falling Asleep**





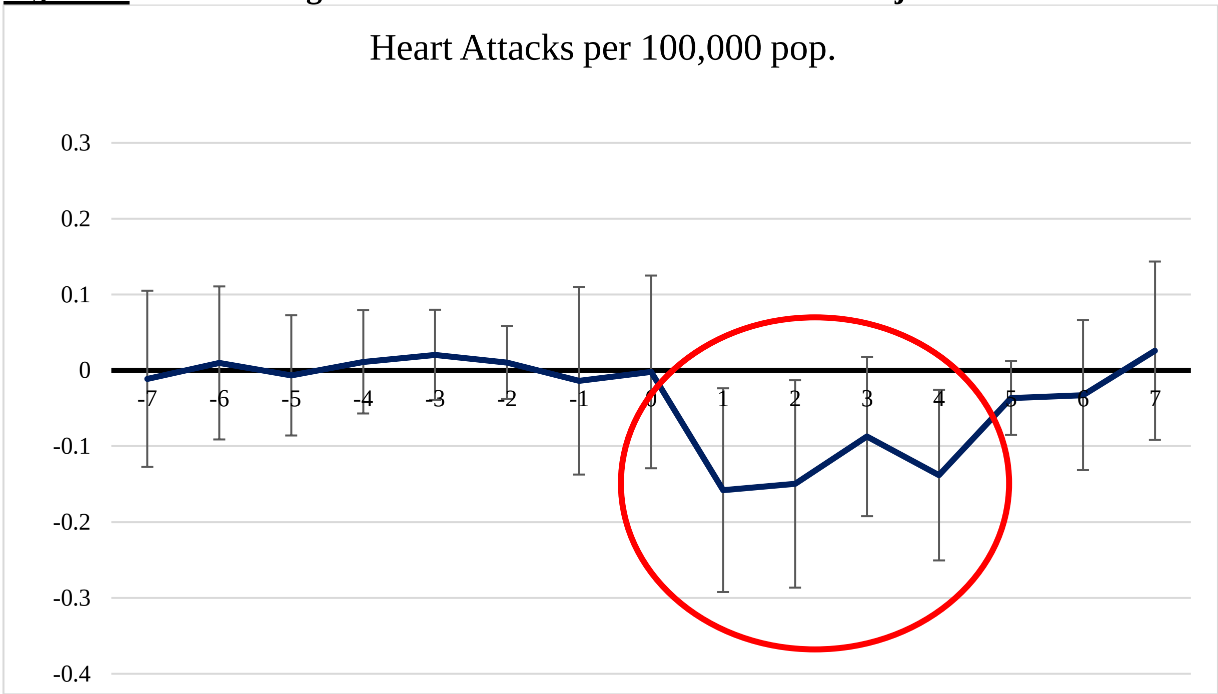
Source: BRFSS, 2009-2010. Equation (1) is estimated and daily effects plotted. Figure 1 illustrates sample selection.

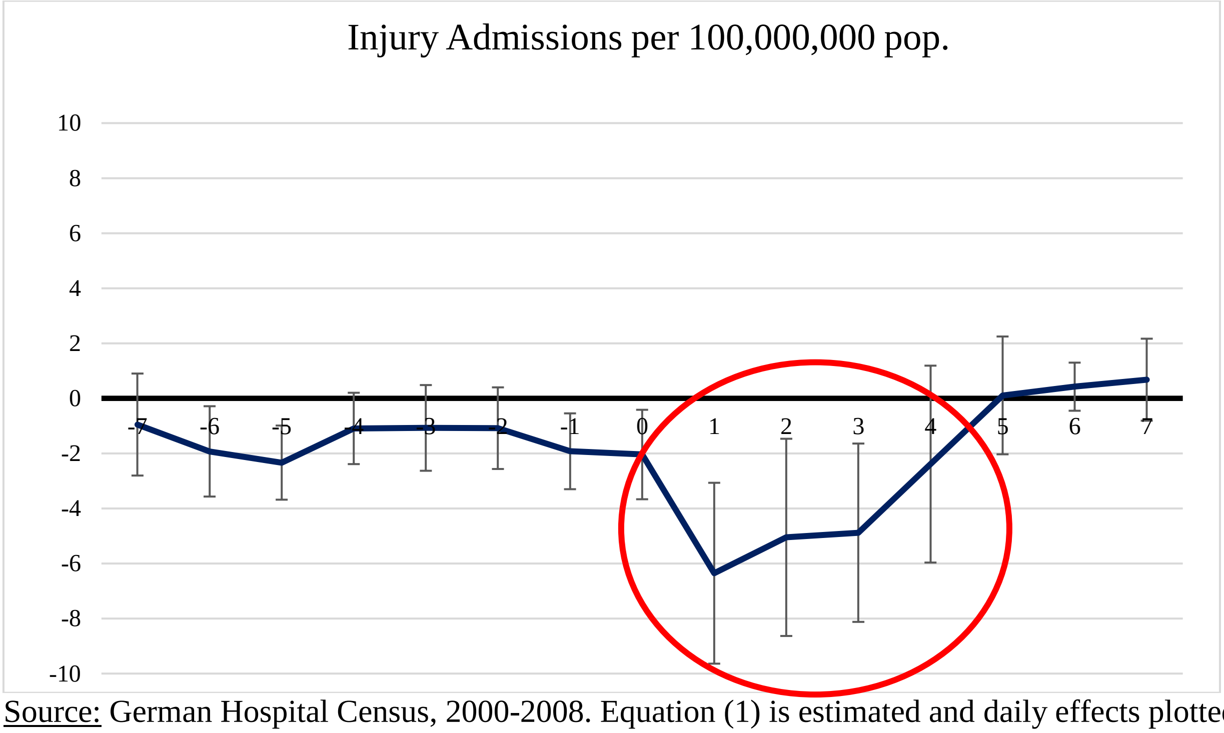
Figure 4a,b: **Effects of Nighttime Extension on Total and Cardiovascular Hospital Admissions**



Source: German Hospital Census, 2000-2008. Equation (1) is estimated and daily effects plotted. Figure 1 illustrates sample selection.

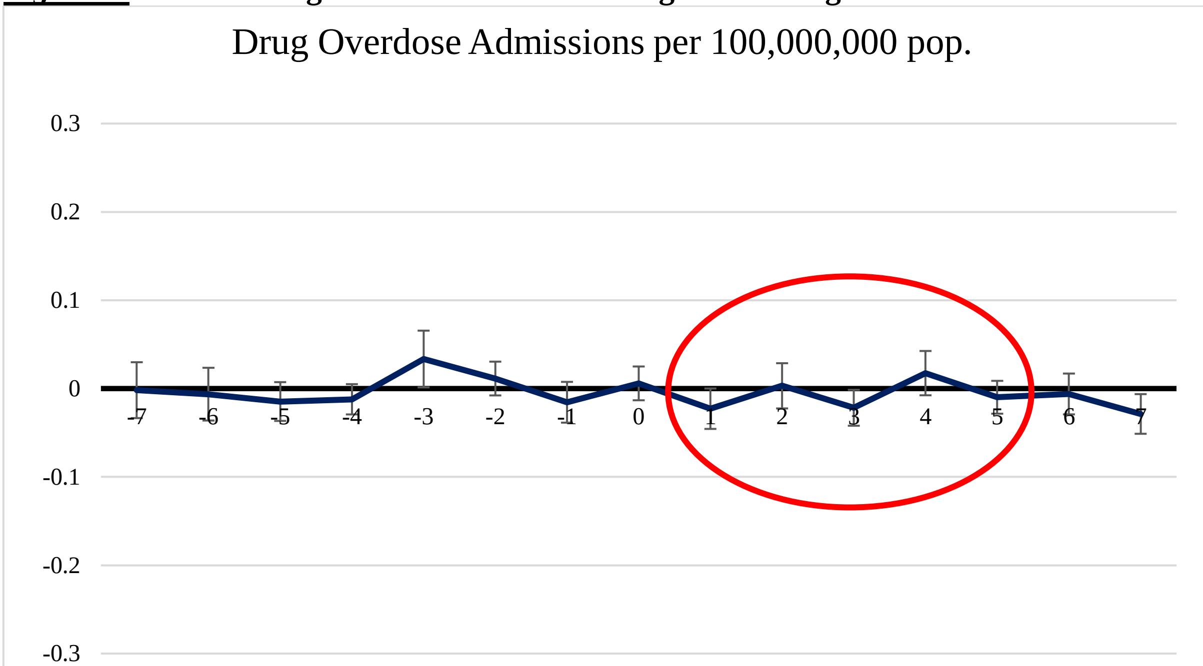
Figure 5: **Effects of Nighttime Extension on Heart Attacks and Injuries**





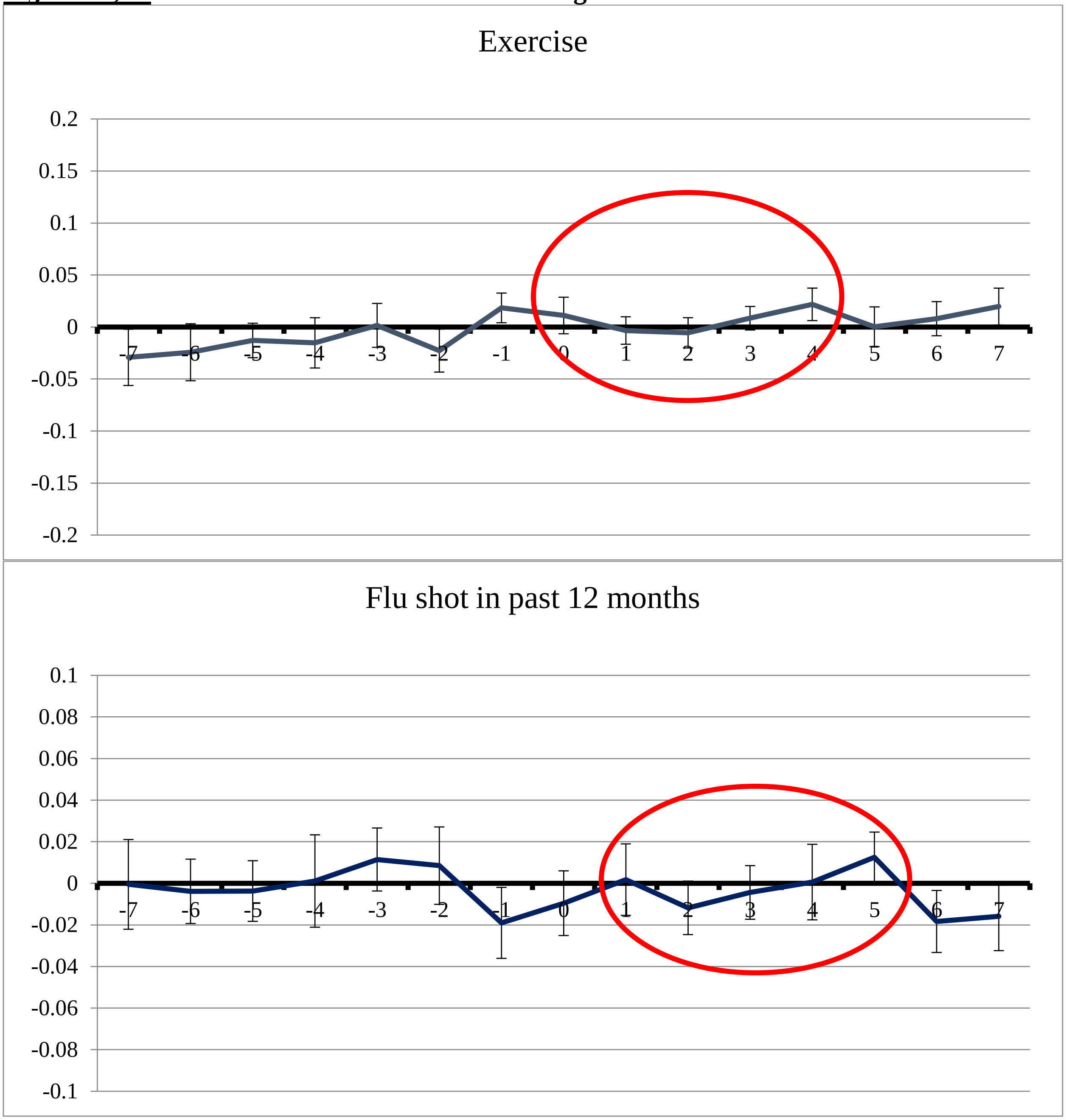
Source: German Hospital Census, 2000-2008. Equation (1) is estimated and daily effects plotted. Figure 1 illustrates sample selection.

Figure 6: **Effects of Nighttime Extension on Drug Overdosing**



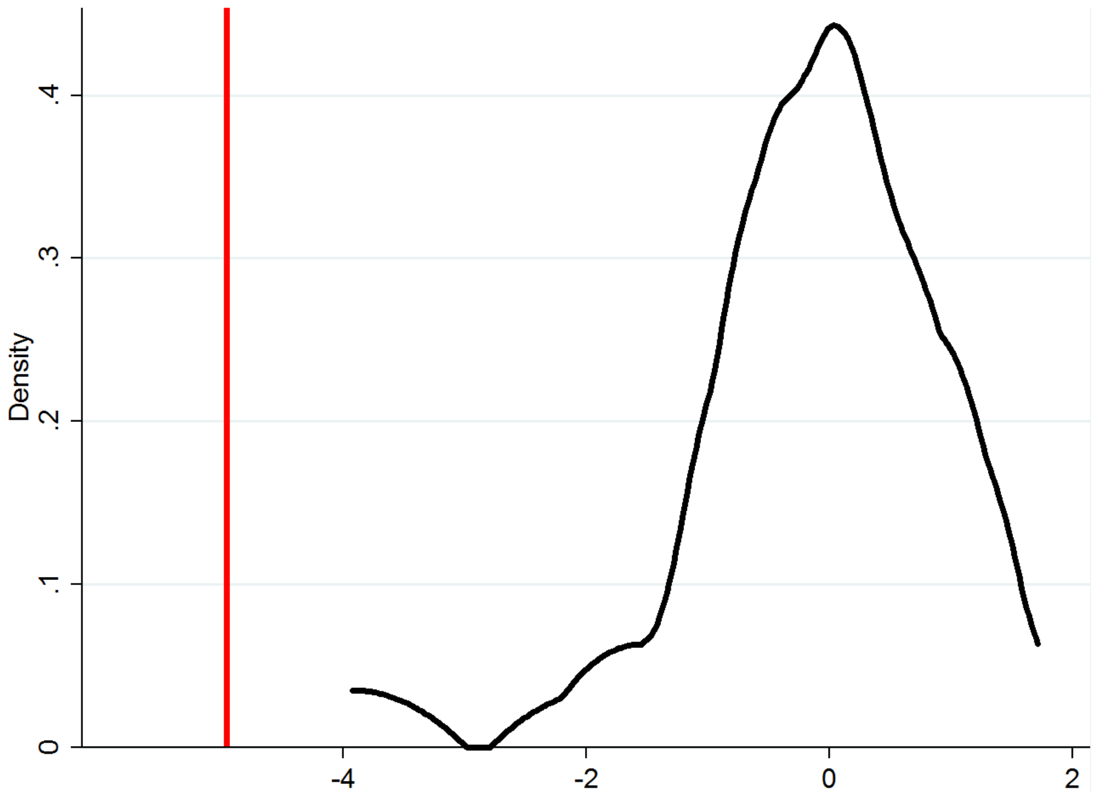
Source: German Hospital Census, 2000-2008. Equation (1) is estimated and daily effects plotted. Figure 1 illustrates sample selection.

Figure 7a, b: **Placebo Tests—Effects on Exercising and Flu Shot in the Past Year**



<u>Source:</u> BRFSS. Equation (1) is estimated and daily effects plotted. Figure 1 illustrates sample selection.

Figure 8: **Permutation Test of Winter Placebo Effects as Compared to DST Transition Week**



Source: German Hospital Census, 2000-2008.

Table 1: **The Effects of Fall DST on Sleep Duration and Tiredness**

	(1) Hours of Sleep	(2) Unintentionally fell asleep during day at least once in past 30 days
Week of Transition	0.026***	-0.044**
(End of DST)	(0.010)	(0.022)
Controls		
State FE	X	X
Halloween	X	X
Day of Week * Month FE	X	X
Month * Year FE	X	X
Linear & quad. time trend	X	X
Mean of dep. Var.	*7.06*	*0.35*
Observations	174,503	10,833

Notes: * p<0.1, ** p<0.05, *** p<0.01. The data are from BRFSS. Standard errors in parentheses are clustered at the date level. Regressions are probability-weighted. *Week of Transition* is an indicator that equals 1 if the interview is on the Sunday of DST transition or one of the subsequent six days. The column headers describe the dependent variables used in each column. Each column is one model as in Equation (1). The sample period for column (1) is 2013-2016. The sample period for column (2) is 2009-2010, as the survey question was experimented by several states at the time and discontinued since. This includes six states (Georgia, Hawaii, Illinois, Louisiana, Minnesota, and Wyoming) in 2009 and nine states in 2010 (Arkansas, Connecticut, Delaware, District of Columbia, Hawaii, Minnesota, Missouri, Nevada, and Oregon).

Table 2: **The Effects of Fall DST on Hospitalizations by Disease Type**

	All cause admission rate (1)	**Cardiovascular admission Rate (2)**	**Heart attack rate (3)**	**Injury admission rate (4)**	**Metabolic adm. rate (5)**	**Suicide attempt rate (7)**	**Drug Overdosing (8)**
Week of Transition	-4.9556***	-0.7195***	-0.0882***	-2.7121***	-0.1874***	-0.0276**	-0.0044
(End of DST)	*(1.1139)*	*(0.1589)*	*(0.02611)*	*(0.6869)*	*(0.0385)*	*(0.0128)*	*(0.0055)*
Controls							
County FE	X	X	X	X	X	X	X
Easter & Vacation FE	X	X	X	X	X	X	X
Day of Week * Month FE	X	X	X	X	X	X	X
Month*Year Fixed Effects	X	X	X	X	X	X	X
Linear & quadr. time trend	X	X	X	X	X	X	X
Socioeconomic covariates	X	X	X	X	X	X	X
Mean of dep. variable	*59.77*	*9.53*	*1.59*	*57.56*	*0.32*	*0.09*	*0.32*
R^2	0.8469	0.5675	0.1510	0.2067	0.3095	0.0179	0.0008
Observations	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***

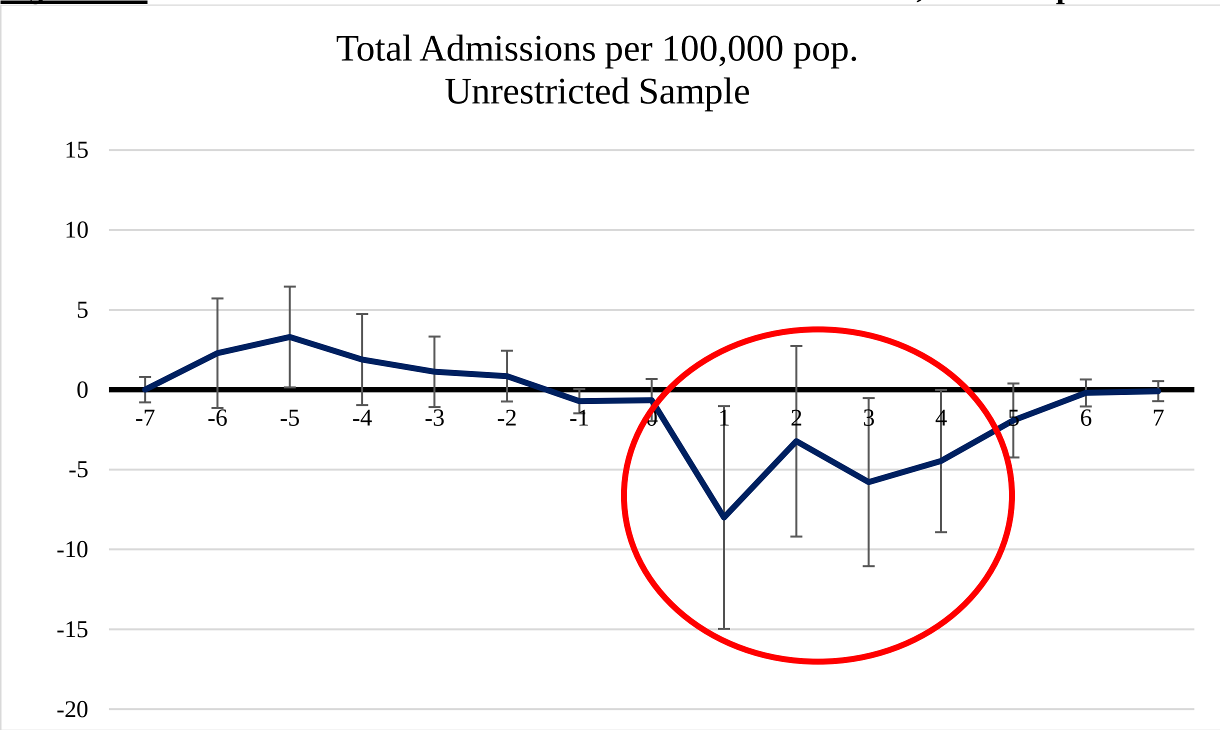
Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors are in parentheses and two-way clustered at the county and date level. *Week of Transition* is an indicator variables that equals 1 if the interview date is on the DST Sunday or one of the following six days. Table B1 lists the dependent variables for 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).

Table 3: **Decomposing and Monetizing Benefits of Additional Sleep**

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

Appendix A

Figure A1: **The Effects of Fall DST Transition on Total Admissions, Full Sample**



Source: German Hospital Census, 2000-2008. Equation (1) is estimated and daily effects plotted.

Table A1: **Descriptive Statistics**

	Mean	***Std.Dev***	***Min.***	***Max.***	***Obs.***
BRFSS					
Hours of sleep	7.06	1.46	1	24	174,503
Unintentionally fell asleep at least once in past 30 days	0.346	0.476	0	1	10,833
German Hospital Census					
Dependent Variables					
Total admission rate per 100,000	59.7681	25.7333	N/A	N/A	336,604
Cardiovascular admission rate per 100,000	9.5339	4.9525	N/A	N/A	336,604
Heart attack admission rate per 100,000	1.5909	1.4035	N/A	N/A	336,604
Injury admission rate per 1 million	56.5571	26.6603	N/A	N/A	336,604
Respiratory admission rate per 100,000	3.9595	2.5850	N/A	N/A	336,604
Metabolic admission rate per 100,000	1.7351	1.5909	N/A	N/A	336,604
Neoplastic admission rate per 100,000	6.5951	5.0857	N/A	N/A	336,604
Infectious admission rate per 100,000	1.4069	1.1953	N/A	N/A	336,604
Suicide attempt rate per 1 million	0.3219	1.6754	N/A	N/A	336,604
Drug overdosing rate per 1 million	0.0892	0.8594	N/A	N/A	336,604
Socio-Demographic Individual Controls					
Female	0.5420	0.0671	0	1	336,604
Surgery needed	0.3715	0.1478	0	1	336,604
Died in hospital	0.0249	0.0230	0	0.5	336,604
Private hospital	0.1177	0.1813	0	1	336,604
Age Group 0-2 years	0.0619	0.0416	0	0.5556	336,604
….	…	…	….	…	336,604
Age Group 65-74 years	0.0161	0.0182	0	0.3333	336,604
>74 years	0.0034	0.0082	0	0.5	336,604
Annual County-Level Controls					
Hospital per county	4.8196	5.4690	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)	0.0103	0.1011	0	1	336,604
Easter Vacation	0.1210	0.3262	0	1	336,604
Fall Vacation	0.0977	0.2969	0	1	336,604
Week Begin DST	0.0862	0.2807	0	1	336,604
Week End DST	0.0862	0. 2807	0	1	336,604

Source: Sleep variables are obtained from the Behavioral Risk Factor Surveillance System (BRFSS). *Hours of sleep* is obtained from 2013-2016, and *unintentionally fallen asleep* is obtained from 2009-2010 due to limited data availability. The hospital admission data are from the 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 A2: **Effects of Fall DST Transition on Admissions by Weather Conditions**

	All cause admission rate			
	(1)	**(2)**	**(3)**	**(4)**
	Temp.	**Rainfall**	**Sunshine**	**Cloudiness**
DST * [column header]	-0.2378	0.0991	-0.2095	0.4458
	(0.2329)	(0.1396)	(0.3431)	(0.5083)
DST	-3.1330*	-5.1829***	-4.4812***	-7.6059**
(3am → 2am in fall)	(1.8671)	(1.2059)	(1.1962)	(3.3876)
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
				s
R^2	0.8372	0.8372	0.8373	0.8373
Observations	336,604	336,604	336,604	336,604

Notes: *** Significant at 1% level, ** 5%, * 10%. 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 (1).

Appendix B: Measurement of Outcome Variables

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

First, the BRFSS tiredness measure refers to “in the last 30 days”, which may introduce measurement error and a non-straightforward interpretation when used as outcome. Assume that there was no recall bias or measurement error and everybody would provide accurate answers. Further, assume that DST would affect respondents for four days. Then, those interviewed on the day of the DST transition would report their average sleep duration on X+1 instead of X days, those interviewed on Monday X+2 instead of X days, and so on. Because our standard approach assigns respondents in weeks t+2 and t+3 to the control group status (Figure 1), 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, we use 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).