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

**Lawrence Jin*** **Nicolas R. Ziebarth****

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**Abstract**

This paper is one of the first to test for a causal relationship between sleep and human capital. We exploit a plausibly exogenous extension of night sleep triggered when Daylight Saving Time (DST) ends. In the fall, the clocks "fall back" and add one additional hour of night time. Our empirical findings are based on the universe of 160 million hospital admissions from Germany and up to 3.4 million BRFSS survey responses from the US over one decade. We find that setting clocks back by one hour significantly extends night's sleep and reduces self-reported tiredness for four days following the time shift. In turn, self-reported health improves and hospital admissions decrease significantly for about four days. Finally, we categorize and monetize various economic benefits of getting more sleep.

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

**JEL codes:** H41, I18, I31

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*Cornell University, Department of Economics, e-mail: ljj9@cornell.edu

**Corresponding author: Cornell University, Policy Analysis and Management (PAM), 106 Martha van Rensselaer Hall, Ithaca, NY 14853, DIW Berlin, and IZA Bonn, e-mail: nrz2@cornell.edu, Phone: +1-(607) 255-1180, Fax: +1-(607) 255-4071.

# 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.

In addition to human capital life cycle models and their empirical applications (Cervelatti and Sunde, 2005; Low and Pistaferri, 2015), important economic studies test for the short and long-run health effects of risky behavior (Kenkel, 1991; Schultz, 2002; Cawley, 2015), ambient air pollution (Graff Zivin and Neidell, 2013; Currie et al., 2014) or *in utero* conditions (Almond and Currie, 2011; Conti et el. 2012; Wilde et al., 2017). 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, adverse environmental shocks, or education.

This paper contributes to the human capital literature by examining the role of sleep and its (short-term) effects on health. 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 studies investigating human capital, 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 overall economic costs of sleeplessness at almost one percent of GDP. It is also worth noting that the global sleep-aid market is growing strongly with an estimated size of $80 billion in 2020. In the US alone, 40 million sleeping pill prescriptions are dispensed every year and about 2800 "sleep labs" exist (CDC 2013, DiSalvo, 2015, Persistence Market Research, 2015)

In one of the few economic sleep studies, Biddle and Hamermesh (1990) show that more labor market activities reduce hours of sleep. Moreover, sleep follows countercyclical pattern (Brochu et al.

2012), which may explain why economic booms are positively correlated with mortality (Ruhm, 2000). Carrell et al. (2011) find that later school start times increase test scores. 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 US. Giuntella and Mazzona (2016) also exploit US time zones to show in a geographic Regression Discontinuity Design that sleep deprivation can lead to poor health and obesity. And in another recent paper, Gibson and Schrader (2017) identify positive wage returns to sleep.

In addition to these scarce economic studies, the medical literature documents that a substantial share of people in industrialized countries are permanently sleep deprived (Moore et. al., 2002; Roenneberg et al., 2007). Knutson et al. (2010) report a significant increase (to 9.3%) in the share of "short-sleepers" (less than six hours) between 1975 and 2006. Whereas correlation studies generally find a link between sleep deprivation and bad health or cognitive ability in large population samples, it remains unclear to what degree this link represents a causal relationship (Taheri et al., 2004; Mullington, et al., 2009; Killgore, 2010; Haack et al., 2013).[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 optimal 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.[2] Today, all countries in the European Union, the great

[1] In one of the few randomized controlled trials involving 48 healthy young adults, Van Dongen et al. (2003) find a significantly reduced cognitive performance after restricting sleep periods to 4-6 hours per night over two weeks (also see Carskadon, Mary and Dement, 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.

[2] There is a debate regarding the effectiveness of DST in conserving energy. In 2007, the US extended the DST period by four weeks with the explicit goal to reduce energy consumption (EPA, 2005). However, several recent studies find that energy consumption may actually (slightly) increase, mostly because the savings in electricity for electric light are overcompensated by increases for heating and other electronic devices such as air conditioning (Kellogg and Wolff, 2008; Momani et al. 2009; Krarti and Hajiah, 2011; Kotchen and Grant, 2011; Sexton et al. 2014). Doleac and Sanders (2015) identify significant

majority of the US states and Canadian provinces, as well as 40 other countries such as Mexico, Chile, Israel, and Iran set their clock one hour forward in spring and one hour back in fall.

The large majority of DST studies have focused on spring DST, which has been shown to increase fatal accidents (Smith, 2016) or crime rates through the shift in ambient light (Doleac and Sanders, 2015). Although we provide evidence for spring as well, our identification strategy focuses on the time shift in fall when the clocks "fall back" and add one additional hour of night time. One main reason to focus on fall DST is that the exogenous extension of night sleep is very unlikely to be confounded by alternative mechanisms. To the extent that vulnerable people follow the salient medical advice in the media to adjust their sleep and take extra care, such behavior is very likely to happen during spring, not fall, DST.

This paper identifies short-term changes in our health outcome measures at the daily level comparing health outcomes in the week of DST to health outcomes in the two weeks before and after while netting out a rich array of month-year and month-day-of-week fixed effects. We consider the yearly one-time extension of sleep in the fall to be exogenous as it plausibly (solely) extends sleep for the sleep deprived and is unlikely to affect health through other channels. We discuss and exclude such alternative channels extensively and run a series of placebo tests including the impact on having received flu shots in the past year, and permutation tests for all remaining weeks of the year. To the extent that alternative mechanism exist, we show that their potential effect sizes are unlikely to confound the main mechanism, which operates through DST-induced variation in night sleep. Alternative mechanisms would also mostly predict negative health effects following fall DST. For example, as evenings get dark sooner, crime rates and traffic accidents are likely to increase, not decrease. These alternative mechanisms would thus downward bias our sleep-health estimates and provide us with a lower bound. While this paper focuses on the causal link between sleep and health using two large population samples from two continents, it

decreases in robberies due the additional evening hour in daylight. One obvious disadvantage of DST is the organizational effort (Hamermesh et al. 2008). Kountouris and Remoundou (2014) and Kuehnle and Wunder (2016) use SOEP as well as BHPS data and find negative well-being and mood effects when focusing on DST in spring and comparing the weeks after to the weeks before DST changes.

also provides a clean evaluation of the health effects of Daylight Saving Time, a policy that affects one billion people around the globe.

This study uses two very large datasets that complement each other: *(a)* The US Behavioral Risk Factor Surveillance System (BRFSS), which elicits self-reported health and allows us to study mild and subjective health effects. And *(b)* The German Hospital Census, which measures adverse and objective health effects requiring inpatient stays. Both datasets together provide evidence from the most populous American and European country in the first decade of the new millennium, from the mildest human-capital effects of sleep 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. This property is crucial to control for seasonal confounding factors while maintaining enough statistical power to identify health effects at a daily level.

First we show that, *on average*, people sleep a highly significant 11 minutes more at night during the week of fall DST.[3] Moreover, we find a significant reduction in the share of people who report having unintentionally fallen asleep during the day. Next, we move on to demonstrate clear and consistent evidence that subjective and objective health significantly improves for about four days following the extension of night sleep, using both the German hospital and the US BRFSS survey data. The BRFSS data show that the share of US citizens in excellent health increases from 19 to 20% between days 1 to 4 after the fall transition. We also find decreases in the share of people in bad (subjective) health. Objective health benefits of longer sleep are identified for the at-risk population in Germany: We find sharp decreases in hospital admissions across several disease categories on days 1 to 4 after the transition. To corroborate our main findings, we run permutation tests using all non-DST weeks during the year, and carry out falsification tests using health outcomes that have no theoretical link with sleep (e.g. flu shots in the previous year). In the last part of the paper, we attempt to categorize and monetize the various

[3] Our findings are broadly consistent with the medical and psychology literature that find DST transitions significantly influence sleep duration and efficacy (cf. Lahti et al, 2008; Barnes and Wagner, 2009).

economic benefits of getting more sleep for the sleep deprived. We assess the average value of feeling rested as a result of sufficient sleep at $55 per day.

The next section briefly describes the data. More details about the data can also be found in the Appendix. Section 3 outlines the empirical methodology. Section 4 presents and discusses the findings and Section 5 concludes.

## 2. DATASETS

### 2.1 The US Behavioral Risk Factor Surveillance System (BRFSS)

Our first dataset measures sleep duration and subjective health effects in the general population. The Behavioral Risk Factor Surveillance System (BRFSS) is a large, annual telephone survey of US adults aged 18 or above, administered by the *Centers for Disease Control and Prevention (CDC)*. The survey began in 1984 with fifteen participating states; by 1996, all 51 US states participated in the survey. It covers an extensive set of self-reported health and also sleep measures and is, by design, representative of state populations. There are more than 3.4 million observations over the period 2001-2010.

As illustrated by Figure 1, our main sample extracts six weeks around the time shift; it counts 421,101 observations. Table A1 reports descriptive statistics of this subsample. The BRFSS includes demographic variables such as age, sex, race, and marital status, as well as the level of education and employment status.

**[Insert Figure 1 about here]**

**Construction of Main Dependent Variables**

First, we focus on people's responses to the standard self-assessed health (SAH) question: *"Would you say that in general your health is ___?"* Table A2 shows the distribution of the five answer categories: excellent, very good, good, fair, and poor. The majority of respondents report their general health to be either very good (32%) or good (30%), and about 19% report excellent general health. Less than 6% of the population report poor general health. From this, we construct two binary dependent variables of interest: *(a) Excellent health*, and *(b) fair or poor health*. Appendix C discusses in detail issues related to measurement errors in these self-reported health measures.

Second, we use two sleep-related measures.[4] We use responses to the following question: *"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."* Responses are integers between 0 and 24. We interpret this as a good measure of sleep duration in the recent past, in particular because the question specifically asks to think about the time spent sleeping. 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 for the impact of DST on sleep duration is likely to be downward biased. It thus provides a conservative test to verify that our exogenous treatment (DST transition) works.

In addition, we use the following question to elicit tiredness during the day: *"During the past 30 days, for about how many days did you find yourself unintentionally falling asleep during the day?"* We converted the responses into a binary variable indicating the share of people who unintentionally fell asleep. This applies to 35% of the US population (Table B1).

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

**Daylight Saving Time in the US**

In the US, DST ends on the first Sunday in November. Time change occurs at 2am, where the clocks are moved back to 1am, effectively extending night hours by one hour. Table 1 shows the various dates

[4] In 2009, six states began to include questions about sleep, which was expanded to nine states in 2010. The six states are: Georgia, Hawaii, Illinois, Louisiana, Minnesota, and Wyoming. The nine states are: Arkansas, Connecticut, Delaware, District of Columbia, Hawaii, Minnesota, Missouri, Nevada, and Oregon.

of DST from 2000-2010.[5]

**[Insert Table 1 about here]**

DST is observed by most states in the US. As of 2017, the states that do not observe DST are Arizona, Hawaii, and overseas territories. Indiana only began to observe DST in 2006. We include observations from non-observing states in our analysis as controls.

### 2.2 German Hospital Admissions Census (2000-2008)

Our second dataset measures severe and objective health effects of a small at-risk population. This second dataset comprises all German hospital admissions from 2000 to 2008. By law, German hospitals are required to submit depersonalized information on every single hospital admission. The 16 German states collect these information and the *German Federal Statistical Office* provides restricted data access for researchers. We hypothesize that health effects identified by these data are primarily triggered by sleep deprived people on the margin to getting hospitalized.

Germany has about 82 million inhabitants and about 17 million hospital admission per year. To obtain the working dataset, we aggregate the individual-level data on the daily county level and then normalize admissions per 100,000 population.[6]

As seen in Appendix B, 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 10th revision of the *International Statistical Classification of Diseases and Related Health Problems (ICD-10)* code.

As with the BRFSS below, we choose a bandwidth of six weeks centered around the time change in the fall (Figure 1). In robustness checks, we make use of the entire 52 weeks of the year. The restricted main sample contains 336,604 county-day observations, whereas the full sample contains 1,429,196

[5] Note that there was a structural change to extend DST in 2007; prior to 2007, DST ended in October.

[6] Note that both nominator and denominator refer to the county of residence. The data excludes military hospitals and hospitals in prisons

county-day observations over 9 years.[7] We leave the data at the county-level and do not further aggregate up to the national level for various reasons: One is that this allows us to stratify the effects by county characteristics and weather and pollution conditions (these data are also available at the county-day level). Another is that we would lose statistical power when aggregating up to a time series at the national level.

**Construction of Main Dependent Variables**

Using the information on the primary diagnosis, we generate the following dependent variables: *(a)* The *All cause admission rate* by aggregating over the total numbers of admissions on a given day in a given county and normalizing per 100,000 population. On a given day, we observe 59.77 hospital admissions per 100,000 population (see Appendix Table B1).[8] However, the rate varies substantially at the daily county level and the standard deviation is 25.73. Note that the county refers to the county of residence of the patient—hence we observe on a daily basis how many of each county's citizens are hospitalized per 100,000 population.

*(b)* By extracting the ICD-10 codes I00-I99—diseases of the circulatory system—we generate the variable *Cardiovascular admission rate*. This is the single most important subgroup of admissions—9.53 admissions per 100,000 population account for 16% of all admissions (Table B1).

*(c)* Extracting the codes I20 and I21, the variable *Heart attack rate* shows that, on a given day, 1.59 people per 100,000 population are hospitalized due to heart attacks.

*(d)* Finally, we generate the indicators *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 daily DST related changes in *suicide attempts* (T14) and *drug overdosing* (T40) per 1 million population.

[7] 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.

[8] Note that German data protection laws prohibit us from reporting min. and max. values.

**Daylight Saving Time in Germany**

In Germany, the clocks are set back on the same day—the last Sunday of October—in all German counties (Table 1). The change in clocks always occurs between the night of Saturday to the morning of Sunday from 3am to 2am. Again, for the main analysis, we restrict our sample to six weeks around the daylight savings time change (see Figure 1).

## 3. EMPIRICAL SPECIFICATION

We use two different model specifications and two very large datasets to test for potential health effects of additional sleep induced by the DST transition. First, our preferred specification identifies the effects at a very fine level, the daily level. Second, we also estimate models at the weekly level which should capture medium-term and potential intertemporal substitution effects. Because we can show that the effects only persist for up to four days, aggregating at the monthly level yields models that lack the statistical power to identify effects that only persist for few days (available upon request).

Furthermore, because the DST transition always occurs on a Sunday around similar times of the year, a regression specification without sufficient seasonal controls may provide spurious estimates by picking up seasonal or weekday effects.

### 3.1 Main Specification: Daily-level Effects

Our preferred specification employs daily dummies around the DST time shift in the fall:[9]

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

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

[9] The results are robust to running probit models and reporting marginal effects for the BRFSS when we employ binary outcome variables.

Equation (1) lists sets of controls that net out seasonal and weekday confounders which are highly relevant when using high frequency data within the DST context. For example, hospital admissions sharply decrease on Sundays and also on national holidays (Witte et al., 2005). $Vacation_d$ controls for public holidays and Halloween.[10]

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 (and are) systematically different from Sundays in May. 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 day-of-the-week effects by month of the year.

As seen, the list of control variables 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. Equation (1) also corrects for county-level or individual-level socio-demographics ($X_{id}'\gamma$) (Table A1 and B1) and persistent differences across states or counties ($\mu_s$).

Because it is unlikely that hospital admission rates are either independent over time or across space, we correct the standard errors, $\varepsilon_{id}$, 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.

[10] German school vacations vary by the 16 states and by date and lengths. In spring, they are typically scheduled around Easter but could vary from early March until the end of April. They vary in length from one up to three weeks, depending on the state. In the US, we also include a dummy for Halloween (Oct 31st ). Halloween is a very recent phenomenon in Germany. The German findings are robust to including Halloween fixed effects. An alternative approach would be to exclude all observations that fall on holidays as in Smith (2016) or omit observations on weekends as in Doleac and Sanders (2015). However, we decide to keep all these observations but control for them flexibly.

### 3.2 Regression Discontinuity Design

As a complementary robustness check, we adopt a regression discontinuity approach, similar to Doleac and Sanders (2015) and Smith (2016):

$$y_{id} = ß_0 + ß_1\, DST_{id} + ß_2\, DaysToDST_{id} + ß_3\, DST_{id} * DaysToDST_{id} + DOW_{id} + \mu_s * \delta_t + \varepsilon_{id} \qquad (2)$$

Where $DST_{id}$ equals 1 if day $d$ falls under DST (i.e., in the summer months), and $DaysToDST_{id}$ is a running variable that counts days to the DST fall transition. Following Doleac and Sanders (2015), we include state-year and day-of-week fixed effects. An important difference between the two specifications is that in Equation (1), we can readily observe lagged health effects of additional sleep, whereas Equation (2) estimates a single effect from the discontinuity. Hence Equation (1) is our preferred specification.

### 3.3 Identification

The key idea of our identification strategy is that the running variable is time, and the treatment is represented by exogenous DST transition dates. Time is arguably exogenous to individuals because humans cannot influence time. This is a variant of an RD approach with time as the running variable and follows the identification strategies in Doleac and Sanders (2015) and Smith (2016). However, our main specification is even more saturated because it de-trends the outcome variables using DOW-month and month-year fixed effects in addition to socio-demographic controls. The richness of our data allows us to still obtain precise estimates at the daily level.

The setting allows us to disentangle: *(i)* The day-to-day short-term and immediate impact of changing the clocks from the *(ii)* net impact on a weekly basis. Moreover, we disentangle important confounding factors such as *(iii)* weekday effects or *(iv)* general seasonal effects as well as specific seasonal effects such as vacation day effects. In effect heterogeneity specifications that test for behavioral mechanisms, we *(v)* stratify the results by ambient climatic conditions such as temperatures, hours of sunshine, and pollution.

**Sample Selection and Definition of Treatment and Control Groups**

Our preferred specification restricts the sample to three weeks before and three weeks after the fall DST transition, as illustrated in Figure 1. This is analogous to the bandwidth of 21 days in Doleac and Sanders (2015). Robustness checks show that the results hold up when all 52 weeks of the year are included in the analysis. The findings are also robust to assigning all three weeks after the transition to the "treatment group." Doing this yields results that are similar to the standard RD design where the post-treatment outcomes are compared to that of the pre-treatment, conditional on all covariates shown in Equation (2).

Table A3 compares the mean covariate values for the week of fall DST—our "treatment week"—to the control weeks prior and post the treatment week (Figure 1). As seen, the mean values are very similar. The normalized difference proposed by Imbens and Wooldridge (2009) shows that no single value is above the critical sensitivity value of 0.25 and all are very close to zero in size. Also when comparing the treatment week mean values to the values of all other weeks of the year (not just the ones around DST) we find balanced covariates. Figure A1 only shows a slight increase in the sample size of the ten years under consideration. This just implies that more recent years get slightly larger weights. Figure A2 shows that the BRFSS has very balanced sample sizes over the 12 calendar months.

Inspecting the observable characteristics of respondents on the DST *Sunday* in fall yields no evidence that respondents systematically react to DST by being more or less likely to participate in the BRFSS (detailed results available upon request).

# 4. RESULTS

## 4.1 The Impact of Fall DST on Sleep Duration

We use BRFSS measures on sleep to provide first-stage evidence that the extension of night hours in the fall increases the average sleep duration. Clocks are set back by one hour from 2am to 1am on the first Sunday of November in the US, extending night time by an hour (in Germany clocks falls back from 3am to 2am on the last Sunday of October, Table 1).

Table 2 shows the results when we estimate Equation (1) using the BRFSS sleep measures as outcome variables. The first two columns use the self-reported *hours of sleep* as dependent variable; the last two columns use *unintentionally fell asleep* as dependent variable. As discussed (Section 2.1), our measures not only capture sleep last night but also from the recent past, which likely implies downward biased estimates and a conservative test of the first-stage effect.

Column (1) suggests that, on average, people sleep an additional 0.27 hours (or about 16 minutes) on the night of DST transition. This estimate is statistically significant at the 1% level. The regression includes the same set of controls as our preferred model in Equation (1), comprehensively netting out seasonal effects. We interpret this as evidence of a strong first stage treatment effect of fall DST on sleep. Note that this is an average (conservative) effect across the entire population. People who get the optimal amount of night sleep are unlikely to drive this effect and would simply wake up one hour earlier when clocks are set back by one hour. Conversely, this increase in sleep is very 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 on the *week* of DST transition (Figure 1). Again, we find that people sleep a statistically highly significant 0.18 hours (or 11 minutes, 2.5%) more in the week of fall DST when night sleep is extended.

**[Insert Table 2 about here]**

We also find consistent evidence when we turn to the self-reported measure of tiredness in columns (3) and (4). The estimated daily effect in column (3) is negative but not statistically significant. This may be due to the noisy nature of this survey question, which asks for tiredness in the past 30 days. 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 report having fallen asleep in the week of fall DST transition. This estimate is statistically significant at the 5% level.

**[Insert Figure 2 about here]**

Figure 2 goes one step further and plots the daily dummies of the vector $EndDST_{id}$ in Equation (1) using *respondent fell unintentionally asleep as outcome measure*. Figure 2 is an event study-type figure and plots the daily dummies -7, -6,…,0,…, 6, 7 of the fall DST week and the week prior to fall DST. As seen, we find evidence very consistent with the findings above: Despite the noise nature of the self-reported measure, one observes a distinct four-day decrease in tiredness following the DST transition. As we discuss below, we find very similar four-day health improvements using various measures from the US and Germany, such as self-reported health or hospital admissions for various causes. We interpret these consistent pattern as strong reinforcing evidence for the credibility of our identification strategy.

### 4.2 The Impact of Fall DST on Hospital Admissions

Next, we study whether hospital admissions vary significantly in the days following the fall transition of DST. Table 2 shows that people sleep significantly longer on the day and in the week of fall DST. As studies have consistently found that about ten percent of the population are permanently sleep deprived (e.g. Knutson et al. 2010), we expect the night sleep extension effects to be particularly concentrated among the sleep deprived.

We start with weekly admission estimates for Germany by disease groups. These are reported in Table 3 where each column is one model as in Equation (1). The main regressor of interest is a dummy indicating the week of fall DST (Figure 1).

**[Insert Table 3 about here]**

Except for drug overdosing, all model 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 relatively similar 7.5% for *cardiovascular admissions* (column (2)). *Injuries* decrease by almost 5% or about 2.7 per 1 million residents. Maybe surprisingly, but consistent with the medical literature (Berk et al. 2008), even *suicide attempts* decrease by almost a third or 2.76 per 100 million residents, illustrating how powerful our data are.

**[Insert Figure 3 about here]**

Next we zoom in and plot the daily dummy estimates of Equation (1). Figure 3a shows *all cause admissions* per 100,000 population (mean 59.8) and Figure 3b *cardiovascular admissions* per 100,000 population (mean 9.5). Despite conservative two-way clustering on the date and county-level, the universe of all hospital admissions allows us to assess even daily effects in a very precise manner.

The two event study graphs in Figure 3 show very clearly the characteristic four-day pattern of improved health after the fall night time sleep extension: We observe a distinct decrease in overall and cardiovascular admissions on days one to four after the fall DST change. The effect is most pronounced on the Monday after the clocks are turned back by one hour, 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, one obtains exactly the same pattern using the full sample (Figure B1), *heart attacks* and *injuries* (Figure B2), *respiratory* and *metabolic admissions* (Figure B3) and *suicide attempts* (Figure B4). There is little room for interpretation whether these patterns could be due to voluntary behavioral responses when we see the same pattern for, for example, heart attacks.[11]

We interpret the similarity of these four-day pattern across different disease groups (and two continents) as strong support for our identification strategy. The implication is that additional sleep leads

[11] 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 in the data and know that the patient stayed overnight, which excludes ambulatory elective surgeries.

to immediate short-term health improvements across a broad range of disease groups for people who are *on the margin* to getting hospitalized. The medical advice for most people on the margin to getting hospitalized is certainly to lay down and rest, which is essentially what additional sleep represents.

### 4.3 The Impact of Fall DST on Self-Reported Health

So far, we have shown significant reductions in hospital admissions following the DST transition, which extends night hours by one hour. This is compelling evidence that an extension of night sleep has positive health effects for the at-risk population.

However, does more sleep also make people *feel* better? To address this question, next, we make again use of the US BRFSS data. Using the share of respondents reporting *excellent health* as outcome measure, Figure 4a plots the estimated daily coefficients of Equation (1). As above, all point estimates are plotted along with 90% confidence intervals.

Following the night sleep extension, the share of people reporting *excellent health* increases by a statistically significant 1ppt on Monday after the transition, and the effect persists until Thursday. Although the estimates are noisier (than hospital admissions) due to being self-reported, we can again identify the characteristic four-day pattern of positive health effects. In fact, it is surprisingly similar to the pattern that we find for hospital admissions in Germany (Section 4.2) and our measure of tiredness (Section 4.1). The size of the probability-weighted coefficients would translate into about 2.5 million marginal Americans who would report "excellent" instead of "very good" health for four days.

**[Insert Figure 4 about here]**

The distinct four day health improvement pattern also hold up in robustness checks. Figure A3 shows the same pattern when we use all 52 weeks of the year and do not weight the regressions.[12] The pattern

[12] Note that this does not equal simply extending the bandwidth in a classical RD design because, in the standard approach, we only use the first DST week as the treatment week and all other weeks before and after as control weeks (Figure 1).

also remain robust when we explore movements from SAH category three (good health) to category two (very good health). Detailed results are available upon request.

However, the flat development of Figure 4b where we use *fair or poor health* as outcome measure may appear to be inconsistent with the change in hospital admissions. It is not but requires an explanation. First, the SAH measures are very noisy. In addition, most Americans are optimistic about their health and only 6% report poor health. Second, people at the lower health margins may not be able to complete the survey (because of hospitalizations), and thus may not appear in the data. Third, when using the RD approach in the next subsection, we do find significant decreases in the share of people reporting bad health. Lastly, using Equation (1) but ignoring people who sleep more than 8 hours on a regular basis, we also find evidence that *fair or poor health* decreases significantly by 3.3ppt over the fall DST transition (available upon request).

### 4.4 Regression Discontinuity Estimates

The regression discontinuity approach of Equation (2) yields results that are in line with the findings above. Table A4 (Appendix) reports the estimated health effects for four binary self-reported health outcomes: excellent health; very good or excellent health; fair or poor health; and poor health.

Accordingly, the night sleep extension reduces the share of people reporting either *fair or poor health* by about 2ppt, implying that 13% of people in this category are pushed up to the "good health" category. Similarly, the share of people reporting *poor health* falls by about 1.3ppt. The coefficient estimates are statistically significant at the 1% level and, as above, one percentage point change in these models equals about 2.5 million marginal Americans.

### 4.5 Could Changes in Ambient Light or Alternative Mechanisms Trigger the Health Effects?

Now we investigate effect heterogeneity by weather conditions using the German Hospital Census. As we explain in Appendix B, we use data from more than one thousand ambient German weather monitors and measure weather conditions in every German county on a daily basis from 2000 to 2008.

Through the stratification via ambient conditions, we hope to learn more about the underlying behavioral mechanisms. 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.

First, we use a BRFSS measure on exercising and plot the daily effects in Figure A4a. In line with Giuntella and Mazzona (2016), we find no evidence that exercising changes following the time shift.

Second, the first four columns of Table B2 stratify the DST effects by *(i)* temperature, *(ii)* rainfall, *(iii)* sunshine, and *(iv)* cloudiness. The underlying hypothesis here is that weather conditions determine how and where individuals spend their time (Gebhart and Noland, 2014); better outdoor conditions should also indicate whether exercising may play a confounding role. Methodologically, we run our standard model, control for weather conditions and interact *EndDST* with the weather measures in the column headers. As seen, consistent with Figure A4a and the absence of structural changes in exercising, the first four models of Table B2 show no evidence that ambient conditions matter in fall. None of the interaction terms between the four weather measures and *End DST* are statistically significant.

As a final check to exclude that exercising produces the identified health effects, we run a robustness check adding region-calendar-day fixed effects for our US model. The idea here is that the daily fixed effects net out daily changes in sunrise and sunset times. Controlling for daily changes in ambient light produces robust main estimates (available upon request).

A shift in ambient light can also impact traffic accidents. Smith (2016) focuses on the spring transition and identifies an increase of traffic fatalities. He shows that changes in ambient light only *reallocates* fatalities, whereas sleep is the driving mechanism.

As a last argument, all potential changes in behavior induced by the shift of sunlight from the morning to the evening hours would be *permanently* induced, and not just last for four days. This also speaks against changes in sunlight triggering the health effects.

Another potential confounding factor could be crime. Doleac and Sanders (2015) show that robberies decrease in the days following spring DST (when evenings get dark later). However, they find no significant impact on crime rates in fall. Even if there was a significant robbery effect, this mechanism would imply that robberies would *increase* following the fall DST change—because evenings become dark sooner—and thus have adverse health effects, opposite the prediction of our sleep mechanism.[13]

Finally, we rule out alternative explanations using placebo tests. First, by construction, the DST transition increases the length of the day 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, opposite to what we find. In addition, although our model nets out seasonal effects, it is theoretically possible that we are still picking up leftover seasonalities following the Sunday of the transition. If these alternative mechanisms were true, they would generate the same four-day improvements in health for *all* types of hospital admissions (or survey questions), even for those unrelated to sleep. To exclude this possibility, we estimate placebo regressions.

Our first placebo test, using the BRFSS, is having received a flu shot in the *past year*. This outcome is, by construction, unrelated to getting additional sleep during the DST transition. As expected, Figure A4b shows no impact of the DST transition on this outcome.

As a second placebo test, using the German hospital data, we conduct the following permutation test: We start in July of each year and select a six-week window of data as illustrated in Figure 1. Then we

[13] 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 US. These numbers certainly would not bias the survey estimates for the US. As for the hospital admission data, these effects would be captured by our separate "Injury Admissions per 1 Million Population" outcome category.

run our standard model with aggregated effects at the weekly level, pretending that the fourth week was the fall DST week. Next, we move the six week window one week further into February and repeat the approach. We permute until week six of our selected sample hits the true DST week and continue with six week windows post fall DST until end of the year.[14] As such, we obtain a total of 23 placebo weekly DST estimates. Figure B5 plots the distributions of the weekly coefficient estimates along with the true DST fall estimate. Figure B5 shows that the decrease in hospital admissions following fall DST does not fall within the statistical placebo estimate distribution during the second half of the calendar year.

### 4.6 Pollution and Socio-Demographics as Mediators

Because pollution has also been shown to have a direct effect on hospital admissions (Schlenker and Walker, 2016), we hypothesize that pollution may operate in interaction with changes in sleep. Columns (5) to (8) of Table B2 makes use of daily pollution measures in Germany (see Appendix B for details). As above with the weather conditions, we add an interaction term between *EndDST* and the pollutant in the column header to our standard model (in addition to controlling for pollution in levels).

As seen, admissions increase when pollution conditions worsen, independent of the fall time change. Pollution seems to be always bad for humans on the margin to getting hospitalized. As the interaction terms show, in the week of fall DST, admissions increase with higher air pollution; the plain *EndDST* coefficient is consistently negative and highly significant. However, when pollution is high in the week of fall DST, part of this general decrease in admissions is offset, as indicated by the interaction term.

Next, using the socio-demographics in the BRFSS, we test whether the effects differ by socio-economic status. We hypothesize that the effects may differ depending on how time-constraint people are (results available upon request). Again, we use our standard model and *Excellent health* as well as *fair or poor health* as outcome measures. Then we add the dummies *male*, *age<50*, *retired*, and *married*

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

in levels and interacted with *EndDST* to the model.

The observed pattern largely confirm the hypothesis above, although the models lack statistical power. For example, we find that the increase in self-reported excellent health is driven by young people under the age of 50. There is also evidence that the effects for males are stronger and that retired respondents are less effected; however, these latter two estimates are not statistically significant.

**4.7 Evidence from the Spring Transition**

Most existing empirical studies focus on spring DST; maybe as a result of the many media reports which regularly warn about the negative potential health effects of the spring transition due to a "mini-jetlag." Our fall findings above are entirely consistent with the existing evidence for spring. However, we argue that the fall transition is a cleaner natural experiment as it simply extends the night sleep; behavioral adjustments due to media reports (which would confound the estimates) are less likely to occur over the fall transition. These priors of ours have been confirmed by our data.

When we run our main models for the spring DST transition, we find evidence that is consistent with fall DST, but that is less distinct: Unlike existing studies—mostly medical studies which often rely on simple before-after comparisons—Figure B6a does not provide much evidence that the heart attack rate increases significantly over spring DST. Our main explanation is that these estimates are confounded by preventive behavior and behavioral adjustments of vulnerable population subgroups: many people with health problems likely follow the medical advice voiced in news reports around spring DST and take extra care.

Figure B6b shows the findings for admissions due to injuries. As seen, distinct effects are largely absent, but one observes a significant uptick in the injury rate shortly after the time change. A similar one-day increase in the admission rate is observable for most other diseases, e.g., metabolic admissions or suicide attempts (available upon request). Given the hundreds of fixed effects that take out substantial variation in the outcome variables, it remains unclear whether these singular increases in admissions are picked up by the majority of studies reporting negative health effects.

### 4.8 Quantifying the Economic Benefits of One Additional Hour of Sleep

There is anecdotal evidence of humans who believe that cutting back on sleep would free up time for daily activities. Such a static view rests on the assumption that cutting back on sleep has no negative impact on work productivity or happiness. Even when we are aware that less sleep makes us less productive or grumpy, we may still not go to bed early enough due to hyperbolic discounting or other cognitive biases, as some behavioral economists argue (Mullainathan, 2014). However, it could also simply be that many obligations leave no time for sufficient sleep. Independent of the exact reasons for a lack of sleep (which should be the objective of future research) this subsection intends to quantify the economic benefits of additional sleep based on our empirical findings. Table 4 provides an attempt to categorize and monetize these benefits. Obviously, these calculations are based on several assumptions but can nevertheless provide a basic framework for such an exercise.

The first column of Table 4 monetizes the benefits of the (subjective) improvement in health for 2.5 million marginal Americans in the four days following fall DST (Figure 4). One Quality Adjusted Life Year (QALY) is typically valued with about $100,000 or about $270 per day (Kniesner et al. 2010). Assuming that the shift in subjective health equals an increase in QALYs by 20% (one category better on the 5-categorical SAH scale), one would obtain a monetary equivalent of about $55 per day or $220 over four days. Summing over all 2.5 million affected Americans results in a population-based estimate of $550 million, or $1.7 million per one million population.

Colum (2) assesses the value of potential increases in work productivity when sleep-deprived employees gain more sleep and feel rested. According to Gibson and Schrader (2017), the short-term wage returns for an additional hour of sleep would equal 1.1% of the wage. For the average American daily wage of $230, this would translate into $10 over four days. Assuming that this productivity gain only applies to the 10% sleep deprived full-time employed Americans, it would sum to $500 thousand per one million population.[15]

**[Insert Table 4 about here]**

The next three columns of Table 4 monetize the value of avoided hospital admissions. Figure 3a suggests that additional sleep can prevent inpatient stays for people on the margin to getting hospitalized; to be specific, we count about 100 fewer admissions per one million population over four days. Column (3) assumes that these 100 people would have been hospitalized for four days and indicates avoided total hospital-related costs of €200,000 per one million population. Avoided costs of lost labor are evaluated using the average daily wage in Germany and assuming that only a third of these 100 people would have been active in the labor market. When evaluating the utility loss for a four-day hospital stay, we assume a 50% loss in daily QALYs. The benefits per avoided hospital stay are then decomposed into €2000 for medical costs and €450 for lost labor as well as $550 for lost quality of life during hospital stays.

Finally, Smith (2016) quantifies the number of avoided traffic fatalities with 30 for the entire US (0.09 per 1M pop.). Evaluated at $5M per life saved, we would obtain sums for saved statistical lives of around $450,000 per one million population.

This little exercise illustrates that the individual benefits of additional sleep can be dramatic for those on the margin to getting a heart attack or being hospitalized. However, even milder quality-of-life effects such as being well-rested due to enough sleep sum to substantial amounts on the population level.

[15] If the increase in work productivity would not just apply to 10% but 20% of US full-time employees, the total monetized benefits would obviously double to one million dollars per one million population.

## 5. DISCUSSION AND CONCLUSION

This paper exploits the quasi-experimental nature of Daylight Saving Time (DST) to assess whether more sleep improves human capital, in this case human health, on the population level in the short-run. It is one of very few causal studies on this topic from the field. We exploit one decade of both a very large survey dataset from the US and the universe of all hospital admissions from Germany. To be able to properly investigate the sleep-health relationship via DST variation on a daily level, one requires powerful representative data over many years. Because the end of DST only happens once a year, and always on Sunday nights around similar times of the year, it is crucial to estimate rich econometric specifications that consider weekday effects in addition to general and specific seasonal adjusters. Our empirical models yield consistent results for the US and Germany, from mild self-reported health outcomes to more severe health issues that require hospitalizations.

Overall, the findings provide strong support for the notion that humans' most time-consuming activity, sleep, does affect their health. The fall DST change effectively implies for many people—particularly those who are sleep-deprived and time-constraint—that they can sleep up to one hour more. Broad evidence documents that millions of people in Western societies are chronically sleep deprived (cf. Knutson et al., 2010). Our results show consistent and robust evidence that health significantly improves for about four days after people gain more sleep subsequent to leaving DST in the fall. About 2.5 million more Americans consider themselves in better subjective health in self-reports, they sleep significantly more, and one observes a decrease in their probability of falling asleep unintentionally during the day. Moreover, administrative hospital admission data identify a characteristic four-day drop in admissions in the days following the transition. For example, cardiovascular admissions decrease by a significant 10 admission per one million population (or about 10%) over four days. We find very similar patterns 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.

Additional sleep can obviously lead to significant health improvements for people on the margin to a severe health shock. 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, 2017), decreases in obesity (Giuntella and Mazzonna, 2016), 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 causal benefits that have been identified by this paper and companion research in economics. Under some assumptions, we assess the societal benefits of the marginal sleep deprived person with $55 per day due to a higher quality of life when feeling rested. Moreover, we monetize an avoided hospital admission of four days with about $4000 due to avoided health care costs, loss in labor, and loss in quality of life.

The main objective of this paper was 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 approach is well-suited for the identification of causal and immediate intent-to-treat effects, but less suited to causally identify long-term effects of sleep. For example, sleep could affect mood, cognitive skills and health cumulatively over time in the long-run, based on one's long term sleep habits. Alternatively, it is imaginable that the human body is able to adapt to (adverse) sleeping conditions. 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 Change**

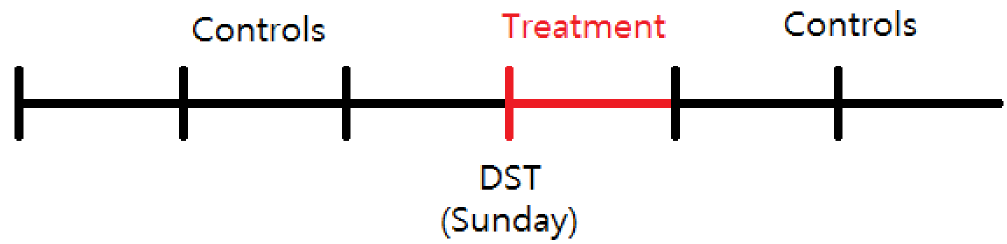


***Figure 2: BRFSS:***
**Effects of Fall DST on Unintentionally Falling Asleep at least once in Past 30 Days, 2001 2010**

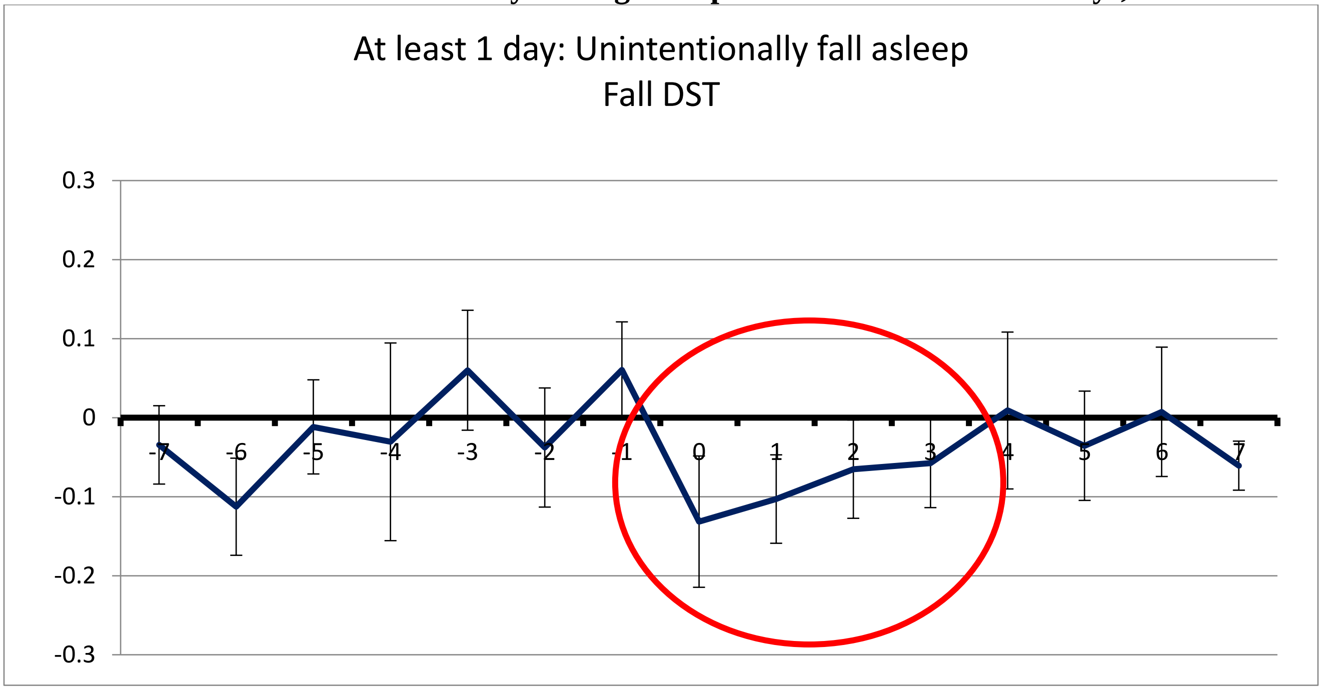


***Figure 3a, b: Hospital Census:***

**Effects of Fall DST on Total and Cardiovascular Hospital Admissions, 2000-2008**

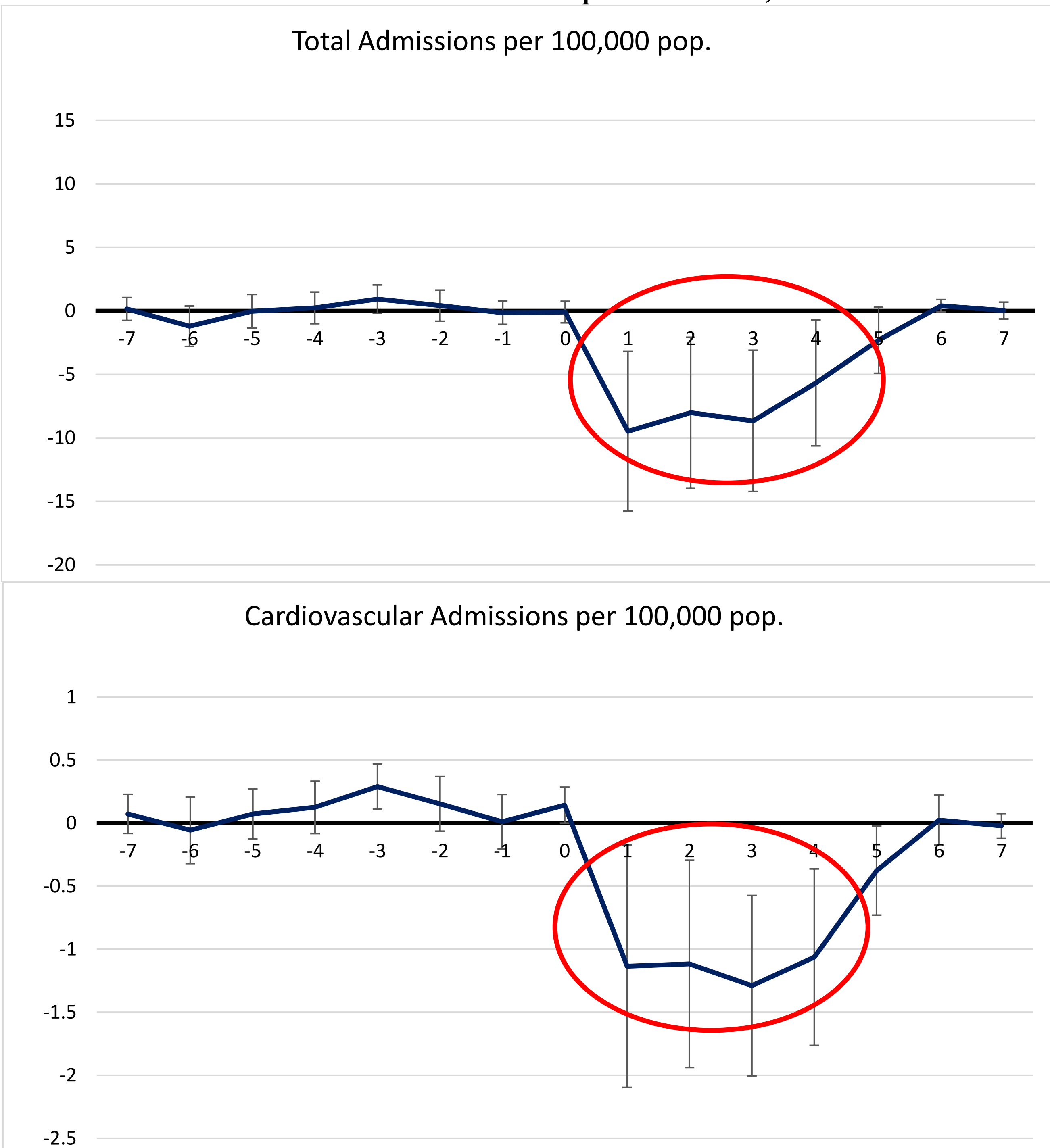


***Figure 4a, b: BRFSS:***

**Effects of Fall DST on Share of People Reporting Excellent and Poor Health, 2001-2010**

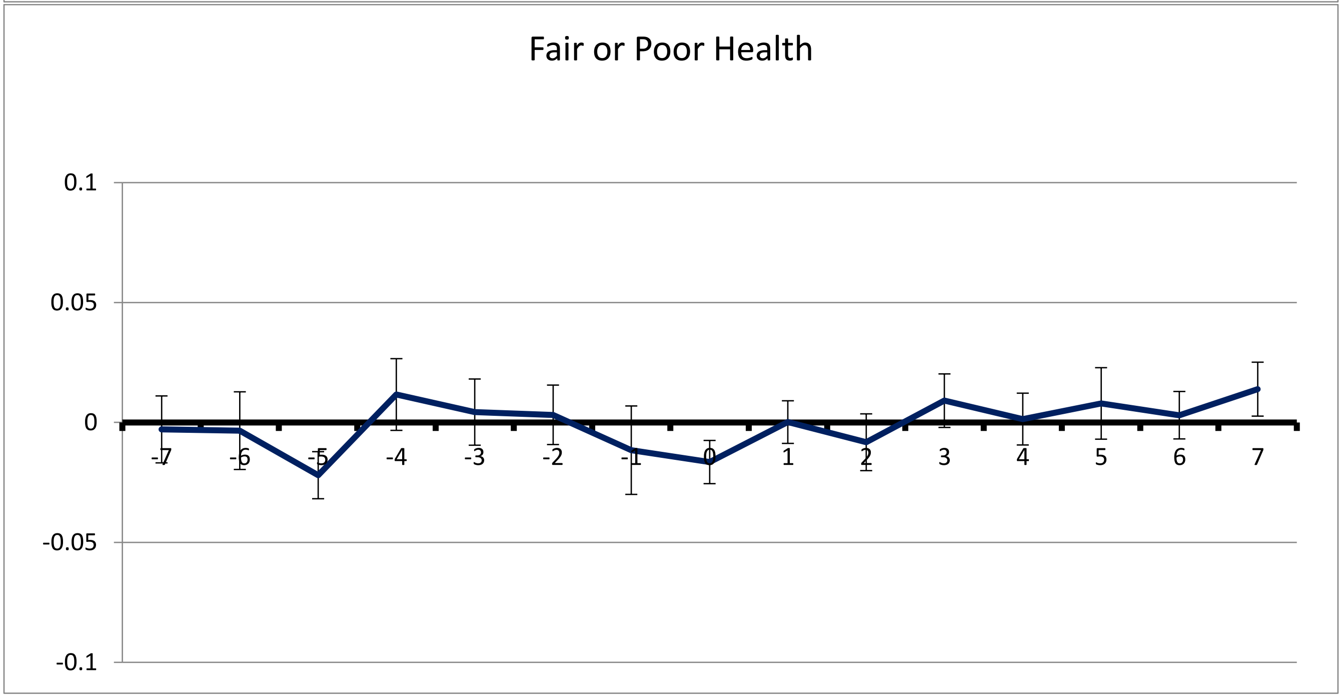



***Table 1: DST***

**End of Daylight Saving Time (DST) in Germany and the US (2000-2008)**

| Year | DST fall Germany | DST fall US |
|---|---|---|
| 2000 | 10/29/2000 | 10/29/2000 |
| 2001 | 10/28/2001 | 10/28/2001 |
| 2002 | 10/27/2002 | 10/27/2002 |
| 2003 | 10/26/2003 | 10/26/2003 |
| 2004 | 10/31/2004 | 10/31/2004 |
| 2005 | 10/30/2005 | 10/30/2005 |
| 2006 | 10/29/2006 | 10/29/2006 |
| 2007 | 10/28/2007 | 11/4/2007 |
| 2008 | 10/26/2008 | 11/2/2008 |
| 2009 | 10/25/2009 | 11/1/2009 |
| 2010 | 10/31/2010 | 11/7/2010 |

***Table 2: BRFSS:***
**The Effects of Fall DST on Sleep, 2001-2010**

| | (1)<br>Hours of Sleep | (2)<br>Hours of Sleep | (3)<br>At least once in past 30 days: Unintentionally falling asleep during day | (4)<br>At least once in past 30 days: Unintentionally falling asleep during day |
|---|---|---|---|---|
| Day of Transition<br>(End of DST) | 0.265***<br>(0.079) | | -0.061<br>(0.054) | |
| Week of Transition<br>(End of DST) | | 0.182***<br>(0.069) | | -0.044**<br>(0.022) |
| Change in percent | 3.7 | 2.5 | -17.4 | -12.6 |
| **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. *** Significant at 1% level, ** 5%, * 10%. 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 inadequacy 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, and 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).

***Table 3: Hospital Census:***
**Effects of Fall DST on Hospitalizations by Disease Type, 2000-2008**

| | **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)* |
| Change in Percent | -8.3 | -7.5 | -5.5 | -4.7 | -59 | -30 | -1.3 |
| **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. The *Week of Begin/End DST* variables are indicator variables that equal 1 if the interview date is on the DST Sunday or one of the following 6 days. Table B1 lists the dependent variables for as displayed in the column header. Each column is one model as in Equation (2). All admission rates are per 100,000 except for *Injuries*, *Suicides* and *Drug Overdosing* (per 1,000,000).

***Table 4:*** **Monetizing Health Benefits of Additional Sleep**

| | *Subjective Health Effects* | *Productivity Effects* | *Objective Severe Health Effects* | | | *Mortality Effects* |
|---|---|---|---|---|---|---|
| | BRFSS (Fig 2b, 4b; Tab. 3) | Gibson and Schrader (2017) | German Hospital Census (Fig. 5b, 6b; Tab. 4) | | | 2% - 10% Bound |
| | **Increase in Well-Being** | **Increase in Work Productivity** | **Health Care Costs** | **Labor Productivity** | **QALYs** | **Avoided Deaths** |
| | ($100K/365)*0.2*4 | Short-term increase by 1.1% at $230 daily wage (OECD, 2016)*4 | €500*4 | €150*4 | ($100K/365) *0.5*4 | Smith (2016): 30 traffic fatalities in US (0.09 per 1M pop.) |
| Benefit for marginal individual | ***=$220*** | ***=$10*** | ***=€2000*** | ***=€450*** | ***=€550*** | |
| | *2.5M/320M | *10% sleep deprived US full-time employees: (0.1*161M)/320M | *100 | *(100/3) | *100 | |
| Sum per 1M pop. | **=$1.7M** | **=$500K** | **=€200K** | **=€15K** | **= €55K** | **=€450K** |

# Appendix A: BRFSS

***Figure A1:*** **BRFSS Observations by Years**

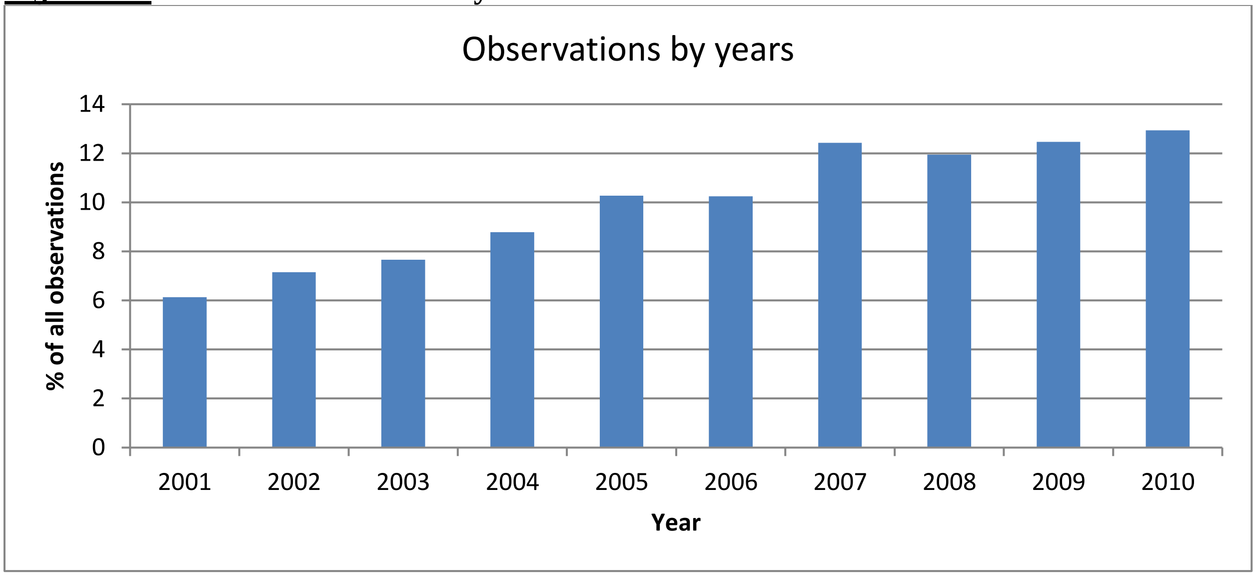



***Figure A2:*** **BRFSS Observations by Month-of-Year**

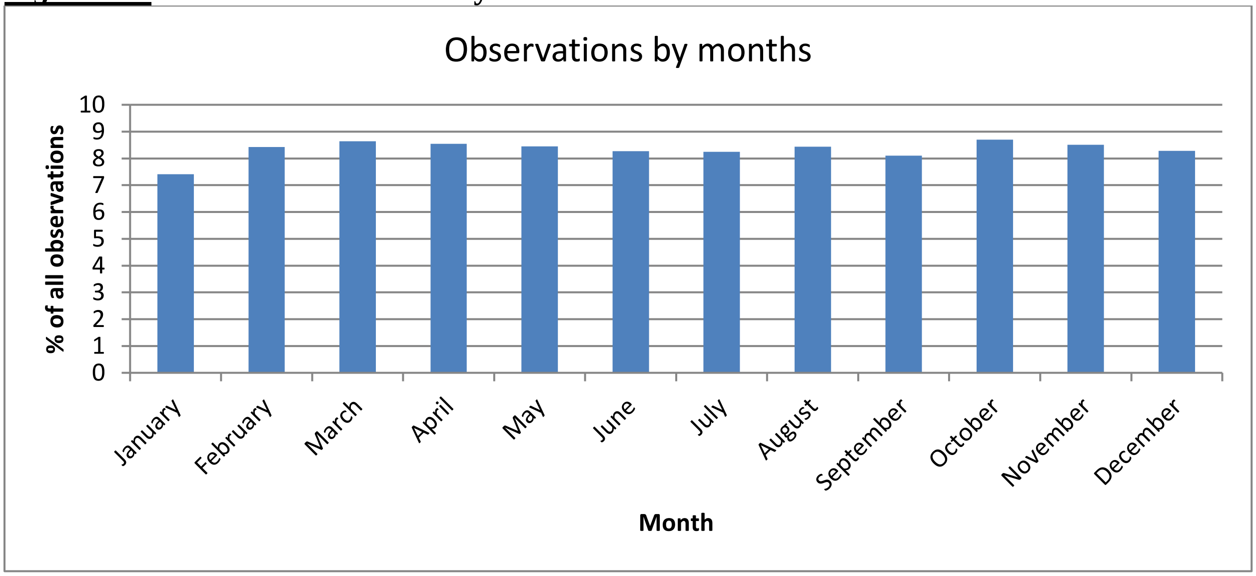
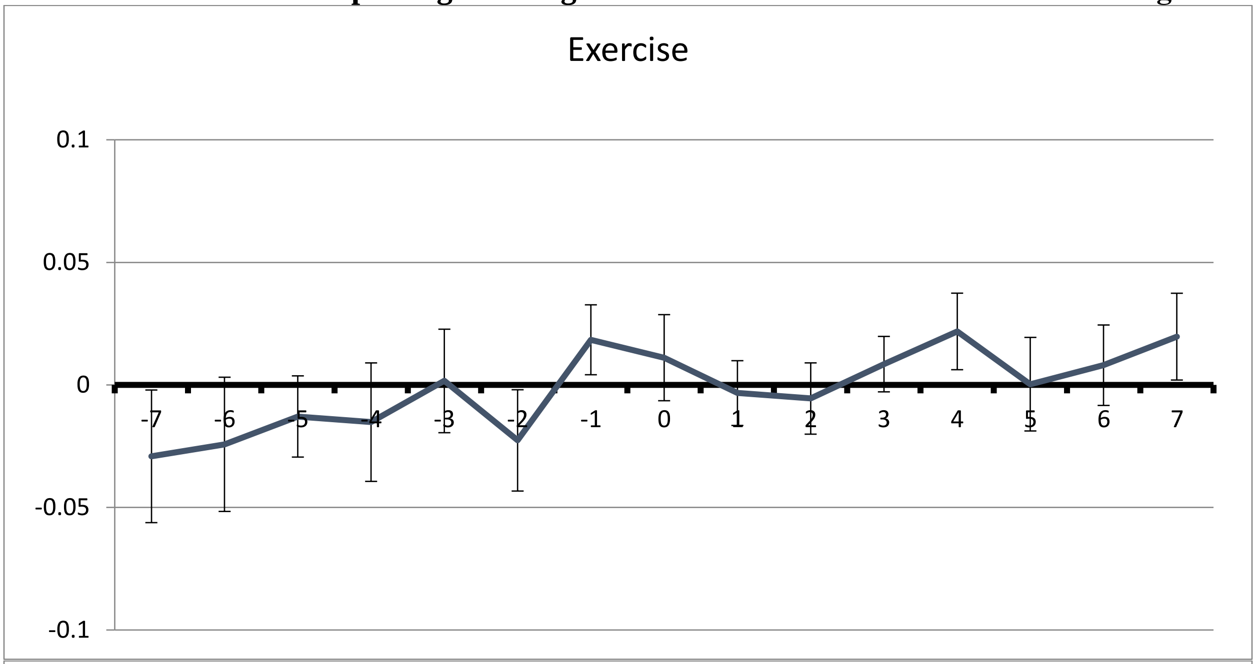


***Figure A3: BRFSS Unweighted Full Sample:***

**Effects of Fall DST on Excellent or Very Good and Poor Health, 2001-2010**

***Figure A4a, b: BRFSS Placebo Test:***
**Effects of Fall DST on Reporting "Having Had Flu Shot in Past Year" and Exercising**



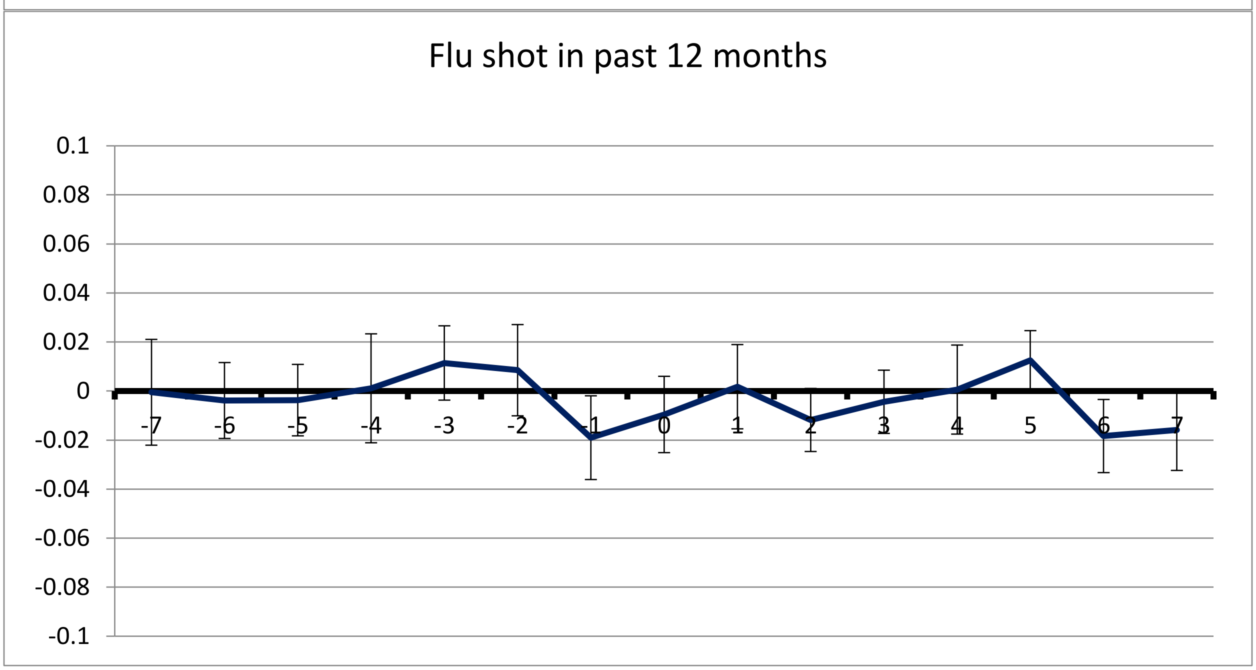



***Table A1:*** **BRFSS Descriptive Statistics**

| | *Mean* | *Std. Dev.* | *Min.* | *Max.* | *Obs.* |
|---|---|---|---|---|---|
| **Dependent Variables** | | | | | |
| General health (1=Excellent, 5=Poor) | 2.527 | 1.107 | 1 | 5 | 421,101 |
| Excellent health | 0.194 | 0.396 | 0 | 1 | 421,101 |
| Very good health | 0.323 | 0.467 | 0 | 1 | 421,101 |
| Good health | 0.300 | 0.458 | 0 | 1 | 421,101 |
| Fair health | 0.128 | 0.334 | 0 | 1 | 421,101 |
| Poor health | 0.055 | 0.228 | 0 | 1 | 421,101 |
| 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 Distribution of Self-Assessed Health (SAH), 2001-2010**

| Responses | Frequency | Percent |
|---|---|---|
| 1 Excellent | 660,207 | 19.1 |
| 2 Very good | 1,107,639 | 32.05 |
| 3 Good | 1,042,752 | 30.17 |
| 4 Fair | 450,411 | 13.03 |
| 5 Poor | 194,977 | 5.64 |
| Total | 3,455,986 | 100 |

***Table A3:*** **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. $\sigma^2$ 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).

***Table A4, BRFSS:***
**Effects of DST Fall Transition on SAH using RD Approach, 2001-2010**

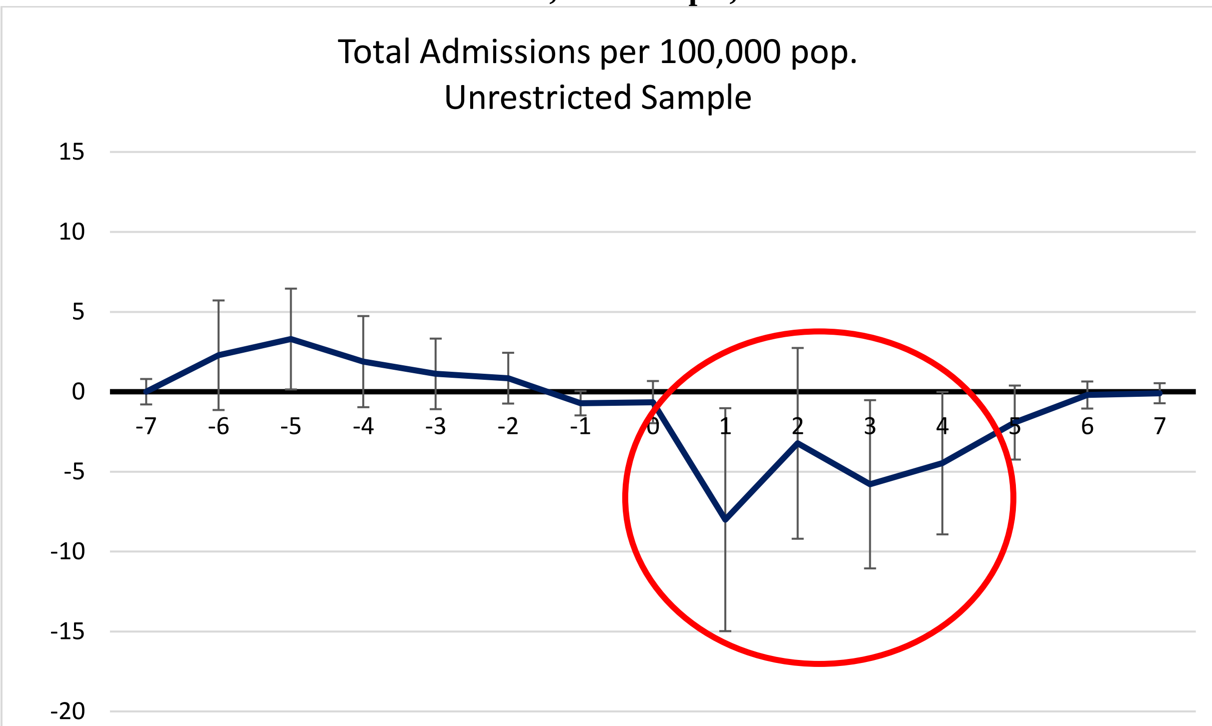
| | (1)<br>Excellent health | (2)<br>VG / Excellent | (3)<br>Fair / Poor | (4)<br>Poor health |
|---|---|---|---|---|
| Leaving DST<br>(Fall) | 0.0010<br>(0.0062) | 0.0054<br>(0.0073) | -0.0232***<br>(0.0054) | -0.0125***<br>(0.0026) |
| *Mean of dep. Var.* | *0.19* | *0.52* | *0.18* | *0.05* |
| Observations | 421,484 | 421,484 | 421,484 | 421,484 |

Notes: Standard errors in parentheses are clustered at the date level. *** Significant at 1% level, ** 5%, * 10%. Regressions are probability-weighted. Each column is one model as in equation (3).

## Appendix B: German Hospital Census

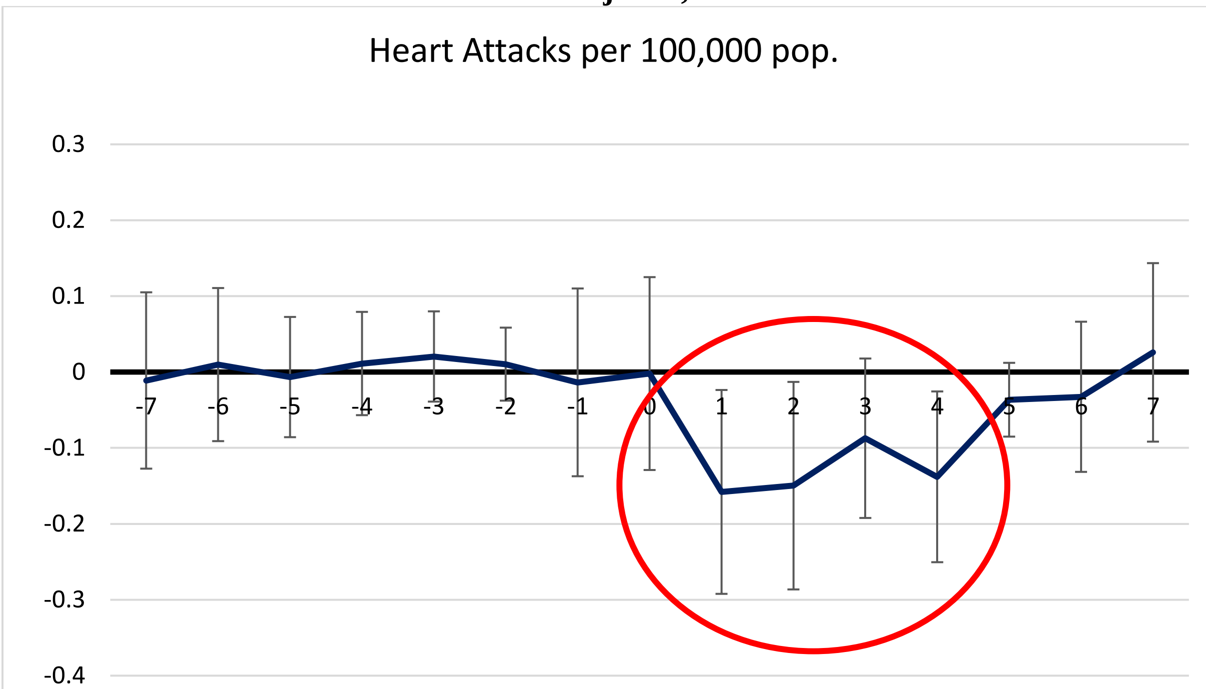
***<u>Figure B1: Hospital Census Full Sample:</u>***

**Effects of Fall DST on Total Admissions, Full Sample, 2000-2008**



***<u>Figure B2a,b: Hospital Census:</u>***

**Effects of Fall DST on Heart Attacks and Injuries, 2000-2008**



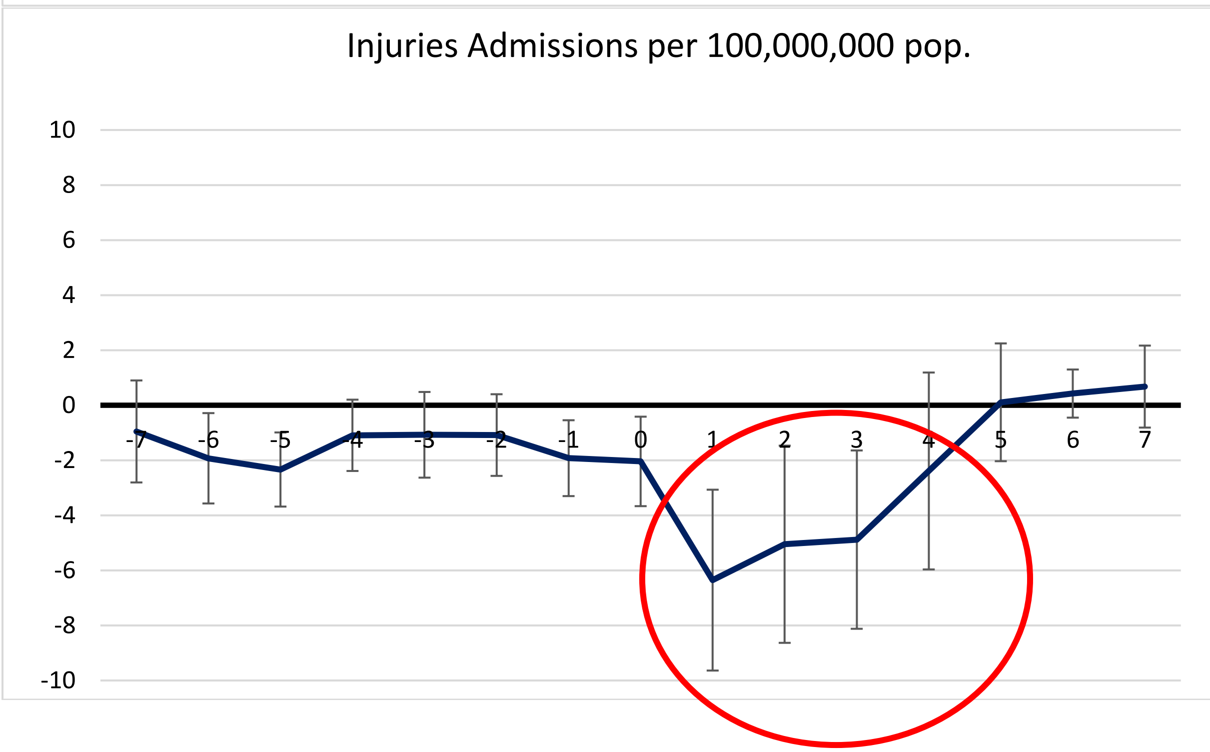
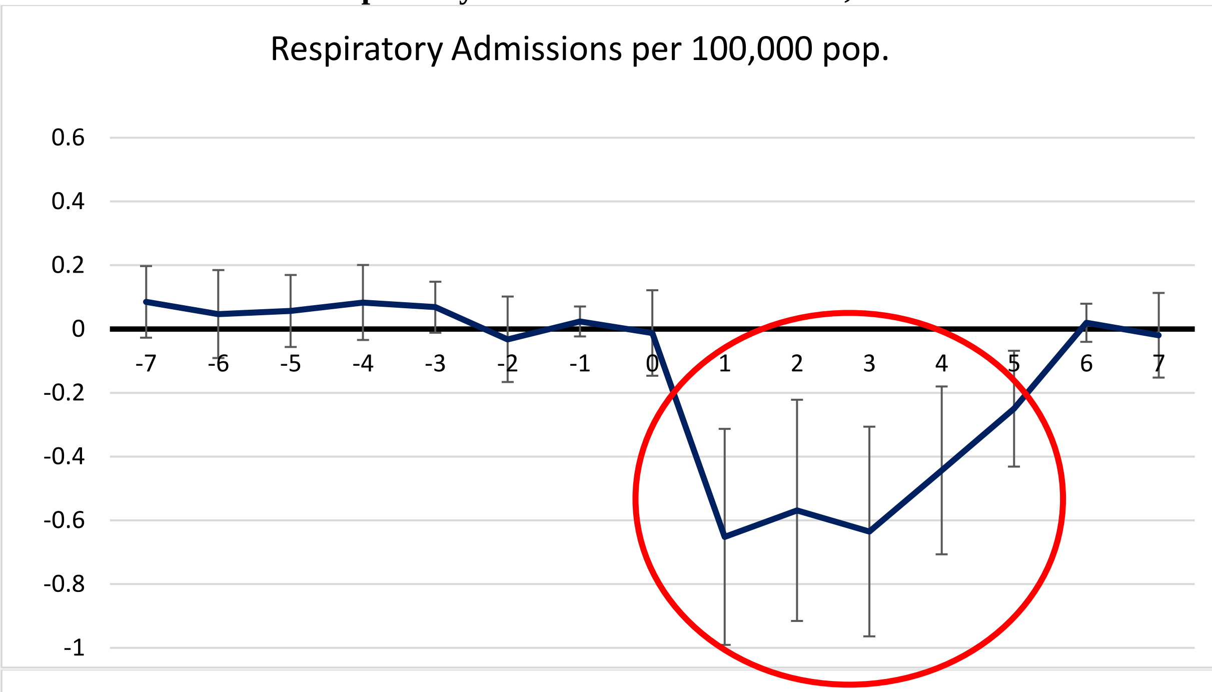



***Figure B3 a,b: Hospital Census:***

**Effects of Fall DST on Respiratory and Metabolic Admissions, 2000-2008**



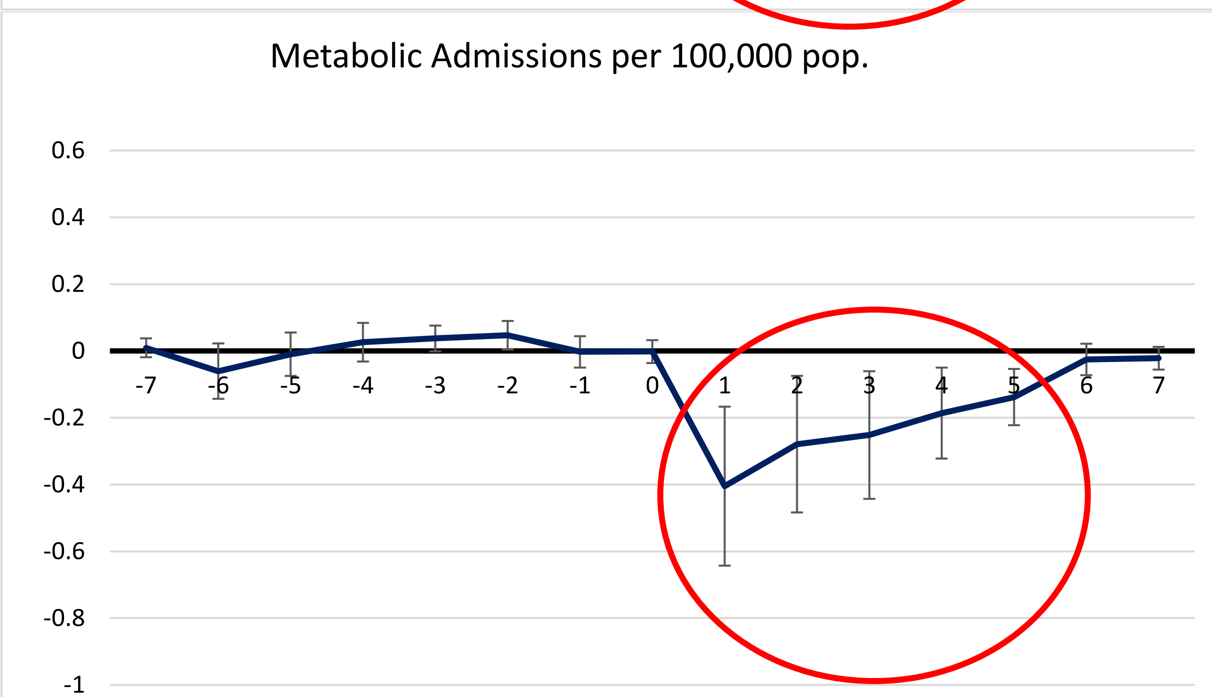
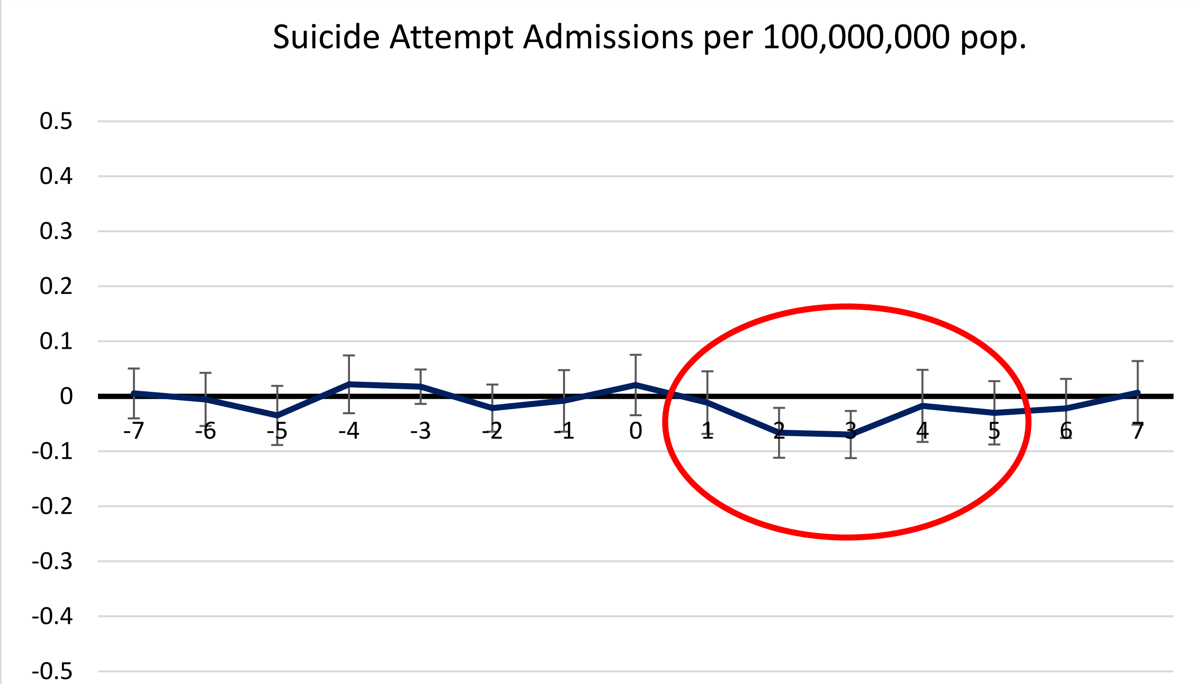



***Figure B4a,b: Hospital Census:***
**Effects of Fall DST on Suicide Attempts and Drug Overdosing, 2000-2008**



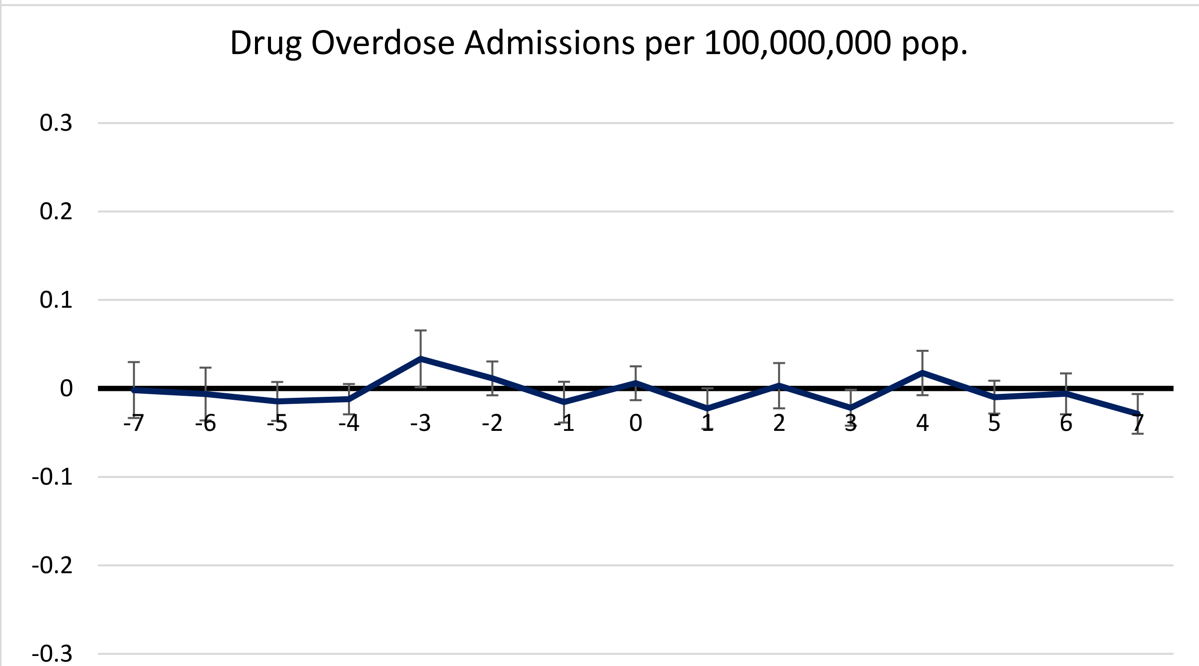



***Figure B5: Hospital Census***

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

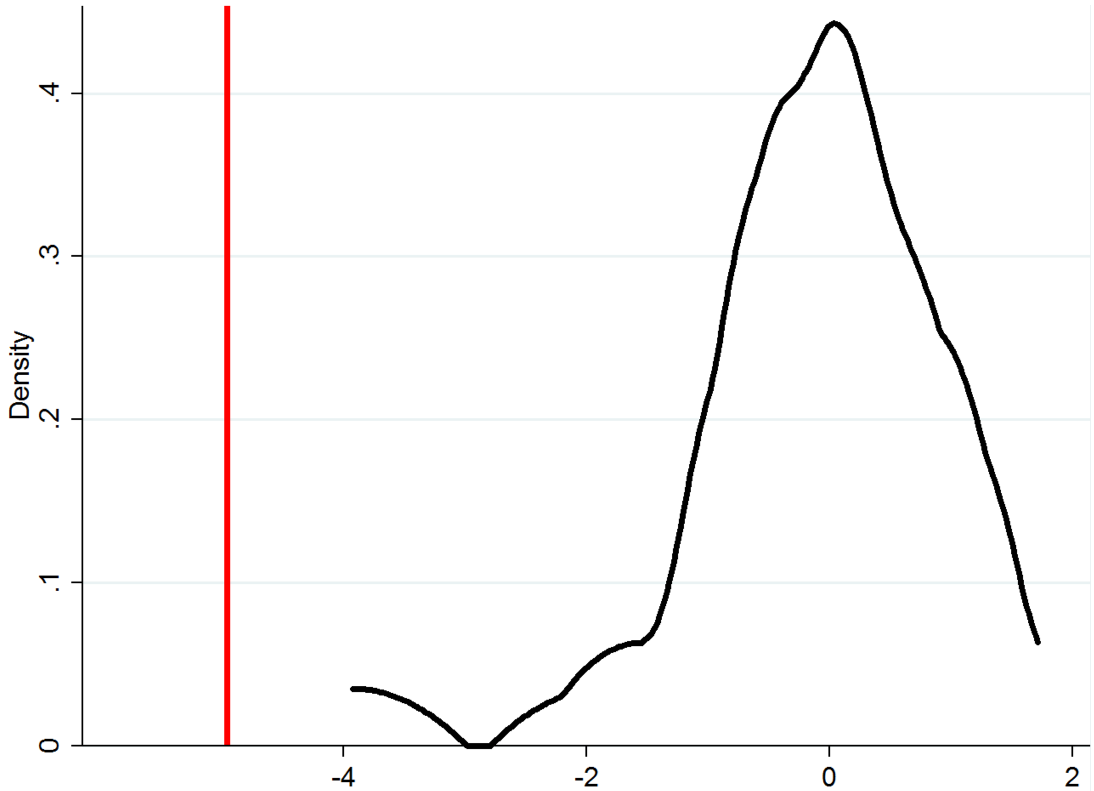
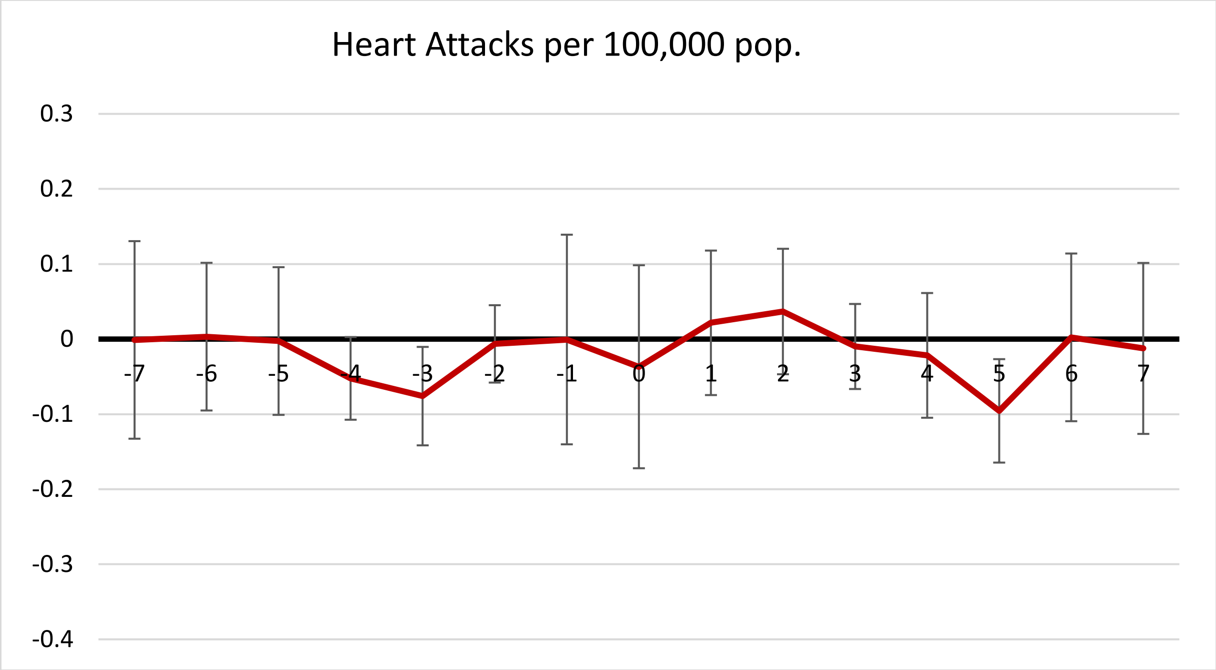


***Figure B6a, b: Hospital Census:***
**Effects of Spring DST on Heart Attacks and Injuries, 2000-2008**



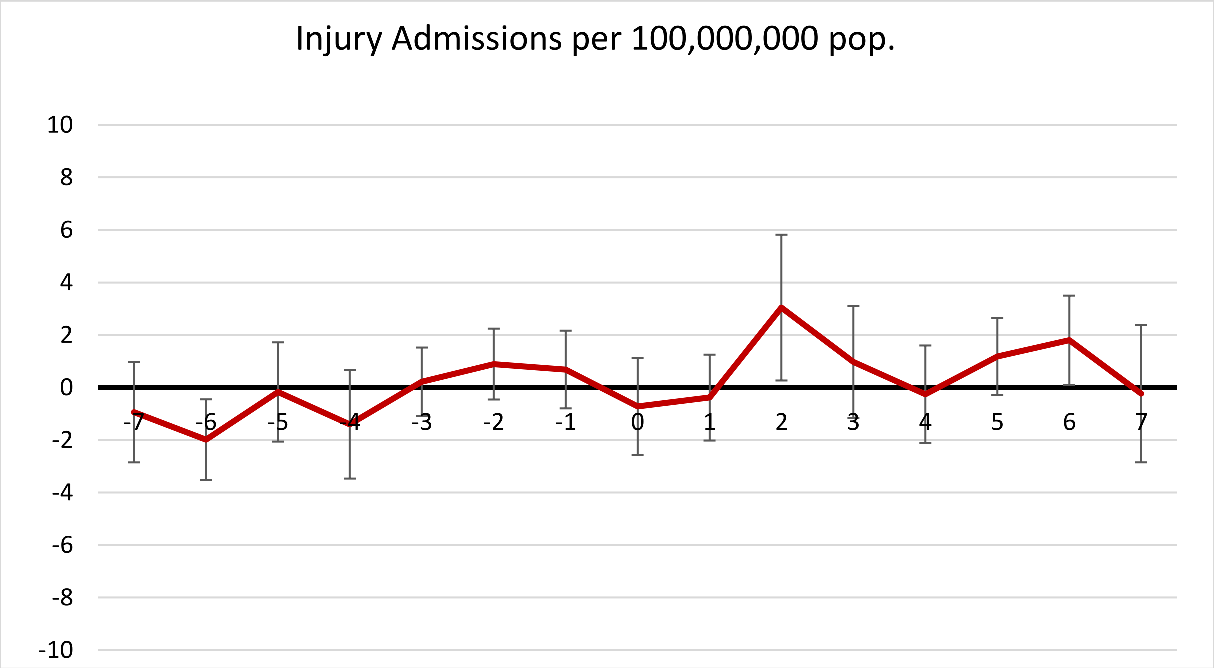



***Table B1:*** **German Hospital Census Descriptive Statistics**

| | ***Mean*** | ***Std.Dev*** | ***Min.*** | ***Max.*** | ***Obs.*** |
|---|---|---|---|---|---|
| **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:** 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. Consequently, 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.

## Linking Hospital Data with Official Weather, Pollution, and Socioeconomic Data

We merge the *Hospital Admission Census* with official daily weather and pollution data to exploit additional exogenous variation in ambient conditions that prevail during the time of DST change.

***Weather Data.*** The weather data is 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 in this study.

The pollution data are provided by the German Federal Environmental Office (*Umweltbundesamt (UBA)*). The data contains daily pollution measures from up to 1,314 ambient monitors and covers the years 2000 to 2008. We make use of four pollutants: CO, $NO_2$, $SO_2$, and $PM_{10}$.

We extrapolate the point measures of the ambient weather and pollution stations 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 information, we link yearly county-level data with the hospital data. As shown in Appendix A, 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 DST on Total Hospital Admissions, 2000-2008, by Weather and Pollution Conditions**

| | **All cause hospital admission rate** | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| ***Variable*** | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| | **Temp.** | **Rainfall** | **Sunshine** | **Cloudiness** | ***CO*** | ***NO2*** | ***SO2*** | ***PM10*** |
| End DST * [column header] | -0.2378 | 0.0991 | -0.2095 | 0.4458 | 8.2131** | 0.2071*** | 0.3059 | -0.0337 |
| | (0.2329) | (0.1396) | (0.3431) | (0.5083) | (3.5118) | (0.0552) | (0.2824) | (0.0894) |
| End DST | -3.1330* | -5.1829*** | -4.4812*** | -7.6059** | -8.8630*** | -10.75*** | -6.0685*** | -4.1789* |
| (3am → 2am in fall) | (1.8671) | (1.2059) | (1.1962) | (3.3876) | (2.2912) | (2.236) | (1.3316) | (2.3038) |
| **Controls** | | | | | | | | |
| Easter, Halloween, Vacation FE | X | X | X | X | X | X | X | X |
| Day of Week * Month FE | X | X | X | X | X | X | X | X |
| Month * Year FE | X | X | X | X | X | X | X | X |
| Linear & quadratic trend | X | X | X | X | X | X | X | X |
| Socioecon. covariates | X | X | X | X | X | X | X | X |
| Weather and pollution controls | X | X | X | X | X | X | X | X |
| $R^2$ | 0.8372 | 0.8372 | 0.8373 | 0.8373 | 0.8373 | 0.8375 | 0.8372 | 0.8373 |
| Observations | 336,604 | 336,604 | 336,604 | 336,604 | 336,604 | 336,604 | 336,604 | 336,604 |

**Notes:** Standard errors in parentheses are two-way clustered at the date and county level. *** Significant at 1% level, ** 5%, * 10%. *Begin/End 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 and pollution 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:
## Do the Dependent Variables Measure True Population Health Effects?

Both types of health measures—self-assessed health (SAH) and hospital admissions—are routinely used by health economists as their main health outcome measures. This does not mean that they are flawless, but we believe that our findings are based on a broad set of different health measures from different countries to validate our findings.

First, a rich health economics literature has investigated reporting heterogeneity (or systematic reporting biases) in the standard SAH measure. In summary: *(a)* Despite its simplicity, SAH is an excellent predictor of true health (McGee et al., 1999). *(b)* Responses to the SAH question are systematically biased with respect to health and gender, whereas this does not appear to be true for other socio-demographics (cf. Ziebarth, 2010). Older people tend to judge their health more mildly relative to younger ones on this absolute scale. Respondents seem to refer to an age-gender dependent reference group when answering the question. Because we control for socio-demographics, age-gender dependent reporting heterogeneity should not be a major threat to our estimates. *(c)* There is no reason to believe that an age or gender reporting bias would be correlated with DST. *(d)* As shown in Table A3, the respondents' socio-demographics are very balanced in the weeks before and after the DST change.

Second, some BRFSS outcome measures refer to "in the last 30 days", which may also introduce measurement error and a non-straightforward interpretation. 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 who were interviewed on the day of DST change would report X+1 instead of X days in excellent health, 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 the control group status (Figure 1), our estimates would be downward biased because 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 (available upon request), we assign respondents interviewed in weeks t+2 and t+3 to the treatment group and the results hold up.

Third, 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 severe population health shocks. Germany has 82 million residents living in an area which has roughly the size of the US state Montana. Thus, the average German population density is seven times higher than the US population density and 231 vs. 32 people per $km^2$ (U.S. Census Bureau, 2012; German Federal Statistical Office, 2012). The hospital bed density is also much higher. Per 100,000 population, Germany has 824 hospital beds, while the US has 304 beds (OECD, 2015). 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).