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

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**Abstract**

This paper is one of the first to test for a causal relationship between sleep and human capital. It exploits the quasi-experimental nature of Daylight Saving Time (DST), all 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 in fall significantly extends sleep and reduces self-reported tiredness during the day. In turn, self-reported health improves and hospital admissions decrease significantly for about four days. Finally, we categorize and monetarize various economic benefits of getting more sleep for the sleep deprived.

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

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

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# 1. INTRODUCTION

Since the seminal work by Becker (1964), Grossmann (1972) and more recently by Heckman (e.g. Cunha and Heckman, 2007), large strands of the economic literature theoretically model and empirically test 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 studies test for the short and long-run health effects of pollution (Graff Zivin and Neidell, 2013; Currie et al., 2014), education (DiNardo and Pischke, 1997; Bhuller et al., 2016), health behavior (Kenkel, 1991; Schultz, 2002; Cawley, 2015), adverse early childhood events or *in utero* conditions (Almond and Currie, 2011; Conti et el. 2012; Figlio et al., 2014), human capital transition from parents to children or vice versa (Black et al., 2005; Kuziemko, 2014; Black et al. 2016) or human capital formation in general (Cadena and Keys, 2015). 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 that investigates how sleep may affect one central component of human capital: human health. Despite the abundance of studies investigating the formation and effects of human capital, the one single activity that humans spent most of their lifetime doing—sleep—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. However, despite a recent "Economics of Sleep" Freakonomics episode (Dubner 2015), economic sleep research is still scant.

Using data from 12 countries, Biddle and Hamermesh (1990) show that more labor market activities reduce hours of sleep, as do higher wage rates. Carrell et al. (2011) find that a later school start time increases test scores. Brochu et al. (2012) show that hours of sleep follow countercyclical pattern, which may explain why economic booms are positively correlated with mortality (Ruhm, 2000). Piper (2015) finds that eight hours of sleep are associated with the highest reported life satisfaction in surveys—but most people actually sleep less. One of the few causal effect studies identifies positive wage returns to sleep (Gibson and Schrader, 2015). In another causal effects study, Hamermesh et al. (2008) exploit television schedules, time zones, and US time use data to demonstrate the relevance of time zones for the scheduling of market work and sleep. Finally, Giuntella and Mazzona (2015) likewise exploit US time zones and time use data to show in a geographic Regression Discontinuity Design that sleep deprivation can lead to poor health and obesity.

Overall, there is strong evidence that a significant share of people in industrialized countries are permanently sleep deprived (cf. 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, 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). 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). While lab experiments are extremely valuable, the natural experiment in this paper allows us to explore the relevance of sleep for health in the general population and under "natural" circumstances.

This paper exploits the quasi-experimental nature of a simple policy 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. First proposed by Benjamin Franklin to save candlelight in a satire letter to the *Journal de Paris* (Franklin, 1784; Aldridge, 1956), Germany and Austria-Hungary were the first countries to introduce DST during World War I (WWI). The original DST rationale was to save energy.[1] Today, all countries in the European Union, the great 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.

We study the transitions into and out of DST to estimate the health effects of changes in sleep patterns. In spring, when the clocks are set forward by one hour from 2am to 3am, a person who does not adjust one's sleep schedule loses an hour of sleep as a result of the time shift. People who perfectly adjust their sleep schedule would not lose any sleep, but most people will fall in between the two extremes of not adjusting and perfectly adjusting. Likewise, when clocks are set backward by an hour in fall, most people gain sleep by some positive fraction of an hour. Indeed, there is strong and clear evidence from the medical and psychology literature suggesting that transitioning into and out of DST significantly influences sleep duration and efficacy (cf. Lahti et al, 2008; Barnes and Wagner, 2009). We exploit these transitions as exogenous shocks to sleep pattern, and investigate their short-term effect on several health measures ranging from self-reported health to hospital admissions.

To study this task, we use two very large datasets that complement each other in an ideal way: *(a)* The US Behavioral Risk Factor Surveillance System (BRFSS), which elicits self-reported health and allows us to study mild health effects, and *(b)* The German Hospital Census, from which we observe more serious health effects that require inpatient stays. Both datasets carry a very large number of

[1] Energy conservation is still an argument. Starting in 2007, the US has 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 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 (2014) 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.

observations—3.4 million interviews from the U.S. and 160 million hospital admissions from Germany—which is crucial to sufficiently control for important seasonal confounding factors while maintaining enough statistical power to identify health effects at a daily level. Both datasets together provide empirical evidence from the biggest American and the biggest 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.

Focusing on the exogenous extension of sleep duration in fall, we find clear and consistent evidence that health improves in the short-run for about four days. We find surprisingly similar patterns in both the German administrative hospital data and the US BRFSS survey, and our results are robust across a number of different model specifications. The data show that the share of US citizens who believe that they are in excellent health increases from 19 to 20% between days 1 to 4 after the change in fall DST. The estimated health benefits of sleep extensions are even sharper for the at-risk population in Germany: We find consistent and unambiguous decreases in hospital admissions across several disease categories between days 1 to 4 after the fall change. Consistent with these positive health effects and underscoring that sleep is very likely the major driving force of the effects, the likelihood to unintentionally fall asleep during the day decreases for four days after the fall transition according to the BRFSS.

In addition to running extensive permutation tests using all non-DST weeks during the year, we carry out falsification tests using health outcomes that have no theoretical link with sleep, such as having flu shots in the previous year. The findings reiterate the robustness of our findings. We also discuss the potential relevance of recently identified causal effects DST on crimes rates (Doleac and Sanders, 2015) and on traffic fatalities (Smith, 2016) and conclude that these alternative operating channels are very unlikely to be the major driving force of the population health effects that we identify.

The findings for spring transition into DST are less clear but are nevertheless consistent: We find no significant effect on either sleep duration, self-reported health, or hospital admissions. There seems to be a pattern of systematic negative health effects on the Monday following spring DST, but these effects are less strong, persistent, and distinct than the very clear effects in fall. The paper discusses possible explanations for the weaker effects in spring and provide evidence for their relevance. For example, every year there are plenitude of media reports around spring DST transitions alerting vulnerable population subgroups of the increased risks of heart attacks and accidents due to DST "mini-jetlag" which is likely to induce behavioral adjustments for marginal people.[2]

In summary, this study is one of the first to show that more sleep may lead to significant, immediate, health improvements for people on the margin to getting hospitalized. For a broader subgroup of the population—we estimate 2.5 million sleep deprived Americans (out of a total of 320 million)—more sleep leads to significant improvements in subjective well-being and potentially a higher work productivity. In the last part of the paper, we attempt to categorize and monetize the various economic benefits of getting more sleep for the sleep deprived. We assess the average value of feeling rested as a result of enough sleep at $50 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 German Hospital Admissions Census (2000-2008)

These data comprise 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

[2] These reports are mostly based on medical studies which mostly use before-after comparisons to identify health effects and increases in traffic fatalities (Ferguson et al., 1995; Coren 1996a, Hicks et al., 1998; Lahti et al., 2010; Alsousou at el., 2011), (workplace) injuries (Barnes and Wagner, 2009; Lahti et al. 2011), cardiovascular diseases (Foerch et al., 2008; Janszky et al., 2012; Jiddou et al. 2013); disruption of sleep (Kantermann et al. 2007), and even suicide rates (Berk et al. 2008). Other welfare-relevant effects that the economic literature identifies are decreases in stock market returns (Kramer et al., 2000), decreases in SAT scores (Gaski and Sagarin, 2011), changes in outdoor activity (Wolff and Makino, 2012) and increases in cyberloafing (Wagner et al., 2012).

for researchers.

Germany counts about 82 million inhabitants and registers the total of 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.[3]

As seen in Appendix A, besides others, 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 changes in spring and fall. 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 counts 1,429,196 county-day observations over 9 years.[4] 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 A1).[5] However, the rate varies substantially at

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

[4] 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.

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

the daily county level and the standard deviation is 25.73. Note that the county refers to the county of residence of the patient—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—the variable *Cardiovascular admission rate* is calculated. Admissions due to cardiovascular diseases are the single most important subgroup of admissions—9.53 admissions per 100,000 population account for 16% of all admissions (Table A1).

*(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 make use of 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.

**Measuring Daylight Saving Time in Germany**

In Germany, clocks are set back and forth on the same day in all German counties. Table 1 shows that Germans spring forward at the last Sunday in March and fall back at the last Sunday in October. The day of the change in clocks is always the night from Saturday to Sunday from 2am to 3am and vice versa. As mentioned, for the main analysis, we restrict our sample to six weeks around each of the two daylight savings times—one in spring and the other in fall (see Figure 1). This resembles the approach by Doleac and Sanders (2015) who chose a bandwidth of 21 days in their RD design exploiting DST. We show that the findings are robust to using the full sample.

In total, 3,916 county-day observations mark the first day of summertime in our data (on average 435 county-day observations over 9 years). Analogously, 3,916 county-day observations mark the last day of summertime.

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

The Behavioral Risk Factor Surveillance System (BRFSS) is a large, on-going annual telephone survey of US adults aged 18 or above, administered by the Centers for Disease Control and Prevention (CDC) in collaboration with state health departments. The survey began in 1984 with fifteen participating states; by 1996, all 51 states (including the District of Columbia) 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.

Again, our main restricted sample extracts six weeks around both spring and fall DST (Figure 1); it counts 799,171 observations; Table B1 reports descriptive statistics of this subsample. As shown, the dataset includes demographic variables such as age, sex, race, and marital status, as well as the level of education and employment status.

**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 B2 shows the distribution of five answer choices: 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. In our restricted sample, 19.3% responded "excellent" general health, and 18.4% responded with either "fair" or "poor" general health.

Second, we also exploit measures that capture self-reported insufficient rest and three sleep-related outcome variables. In 2009, six states began to include questions about sleep inadequacy in the BRFSS, and this expanded to nine states in 2010.[6] In robustness checks and falsification testes, we uses information on whether respondents received a flu shot in the past calendar year or whether they exercise.

Section 2.3 below discusses in detail issues related to measurement errors in these self-reported measures.

**Measuring Daylight Saving Time in the US**

In the US, as of 2016, DST begins on the second Sunday in March and ends on the first Sunday in November. Time change occurs at 2am, where the clocks are moved forward from 2am to 3am in spring and moved back from 2am to 1am in fall. Table 2 shows the various dates of DST for the years 2001 to 2010. Note that there was a structural change to extend DST in 2007; prior to 2007, DST began in April and ended in October.

DST is observed by most states in the US. As of 2015, 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.

### 2.3. Do the Dependent Variables Measure True Population Health Effects?

Both types of health measures—self-assessed health (SAH) in surveys as well as 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 enough set of different health measures from different countries to validate our findings.

First, a rich health economics literature investigates reporting heterogeneity (or systematic reporting biases) in the standard SAH measure. *(a)* Despite its simplicity, it has been shown that SAH is an excellent predictor of true health (McGee et al., 1999). *(b)* It has been demonstrated that responses to the SAH question are systematically biased with respect to health and gender, whereas there is less evidence

[6] 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.

that this holds 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 threat to our estimates. *(c)* There is no reason to believe that a age or gender reporting bias would be correlated with DST. *(d)* As shown in the Appendix, 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 (see 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 weigh days closer to the interview day much stronger. 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. Still, in case of classical measurement error in self-reports, one expects attenuated estimates.

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 (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's health care infrastructure offers 824 hospital beds, while the US has only

304 (OECD, 2015). Hence, geographic hospital access barriers, such as travel distances, are low in Germany. Moreover, the uninsurance rate in Germany 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).

## 3. EMPIRICAL SPECIFICATION

We take advantage of different model specifications on two very large datasets to test for the health effects of changes in sleep pattern caused by DST transitions. Because the transitions into and out of DST always occur on Sundays around similar time of the year, a regression specification without sufficient seasonal controls may provide spurious estimates by accidentally picking up effects of other time discontinuities that occur naturally. Sundays are different from Mondays, October 31st is different from November 1st, and so forth.

Our main model specification employs daily dummies around DST transitions, along with an extensive set of seasonal control variables. This method is similar to a regression discontinuity approach in that we are comparing the DST transition days with neighboring days that are unaffected by the transition, but the daily-dummy approach has a few unique advantages. First, by employing daily dummies, we clearly observe how the health effects persist or evolve over time. We are also able to control for seasonal effects more flexibly and comprehensively, which is made possible by the very large nature of the two datasets that we use.

As a secondary robustness check, we also adopt a regression discontinuity design similar to Doleac and Sanders (2015) and Smith (2016), who exploit DST in an RD design. Smith (2016), for instance, demean the outcome variable by day-of-week and year before estimating a standard RD specification. Doleac and Sanders (2015) include day-of-week and jurisdiction-by-year fixed effects, along with weather variables, to control for seasonal variations in estimating an RD model. We follow a similar RD approach and find that our results are consistent and robust across different specifications.

### 3.1 Daily-level Effects

Our preferred model specification employs daily dummies around each of the two DST transition days—the start of DST in spring and the end of DST in fall—as shown below:[7]

$$y_{id} = ß_0 + ß_1\, BeginDST_{id} + ß_2\, EndDST_{id}$$

$$+ X_{id}'\gamma + Easter_d + Vacation_d + \phi_m * \delta_t + DOW * \phi_m + t + t^2 + \mu_s + \varepsilon_{id} \qquad (1)$$

Where $y_{id}$ is the health outcome variable of interest using the German Hospital Census (BRFSS), for county (individual) *i* on day *d*. *BeginDST* and *EndDST* are vectors each containing fifteen daily dummies around the spring and fall transitions of DST, respectively.

The second row of equation (1) lists sets of controls that net out systematic seasonal confounders which are very relevant when using high frequency data within the DST context. For example, it is well-known that hospital admissions sharply decrease on Sundays and also on national holidays (Witte et al., 2005). In our data, relative to Sundays, hospital admissions increase strongly by 52 per 100,000 pop. (mean: 59.57) on Mondays.

Because transitions into and out of DST always fall on Sundays, it is crucial to net out day-of-the-week effects. Moreover, because spring DST sometimes fall into school vacations and on Easter (which is a national holiday in Germany), $Easter_d$ and $Vacation_d$ include control variables for these days.[8] An

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

[8] In Germany, official school vacations vary at the level of the 16 states by date and also in 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. Fall vacations lie between the beginning of October and mid-November, and vary likewise by state, both in term of time and length. In the US, we also 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.

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.

Likewise, because of the strong correlation between our outcome variable and day-of-the-week (DOW) effects, we interact DOW effects additionally with calendar month fixed effects ($DOW^*\phi_m$). As seen, the list of control variables also includes month-year fixed effects ($\phi_m^*\delta_t$) as well as 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. Our specification also corrects the sample composition for county-level or individual-level socio-demographics ($X_{id}'\gamma$) as well as persistent differences across states or counties ($\mu_s$). Appendix A and B list the socio-demographics included in $X_{id}$.

Because it is not likely that the county-day 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. We also estimate an analogous model with health effects aggregated at a weekly level.

### 3.2 Regression Discontinuity Design

As a second specification and 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 one if day $d$ falls under daylight saving time (i.e., in the summer months), and $DaysToDST_{id}$ is a running variable that counts days to the DST transition of interest—either spring or fall. Following Doleac and Sanders (2015), we include state-year and day-of-week fixed effects. While this is not our preferred specification, one nice feature of this approach is that it estimates a single instantaneous jump following the DST transition, and it provides us with a complementary robustness check alongside our main results.

### 3.3 Identification

The key idea of our identification strategy is that the running variable represents time, and that 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 we de-trend the outcome variables using DOW-month and month-year fixed effects in addition to a rich set of socio-demographic controls. The richness of our data allows us to still obtain remarkably 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 Easter Sunday or vacation day effects. In effect heterogeneity specifications that intend to test for behavioral adaptions and 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 each of the two DST transition dates 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 as a "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).

**[Insert Figure 1 about here]**

Table B3 compares the mean covariate values for the week of spring or fall DST—our "treatment week"—to the control weeks prior and post the DST week. 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 surprisingly balanced samples. This suggests that the BRFSS does a good job providing balanced samples over the 52 weeks of a year. Figure B1 only shows a slight increase in the sample size of the ten years under consideration. This basically just implies that the behavioral reactions in more recent years get attached slightly larger weights, which is no threat to our identification strategy. Figure B2 shows that the BRFSS has very balanced sample sizes over the 12 calendar months.

Inspecting the observable characteristics of respondents on the DST Sundays in spring and 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 DST on Hospital Admissions

First, we study whether hospital admissions vary significantly in the days following DST. Recall that clocks are set back by one hour from 3am to 2am on the last Sunday of October in Germany (and from 2am to 1am on the first Sunday of November in the US, Tables 1 and 2). We expect the significant share of sleep deprived people in industrialized countries to sleep one hour longer because this time change effectively extends the sleep time by up to one hour. Note that this sleep extension could last for more than one day if people do not push back their bedtime by one hour immediately in the following days; for example, if they still get tired at their usual summer time bedtime.

Figures 2 and 3 plot the coefficient estimates of the daily dummies in equation (1). Figure 2 shows total admissions per 100,000 population (mean 59.8) and Figure 3 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 daily effects in a very precise manner.

**[Insert Figures 2 and 3 about here]**

Figures 2b and 3b clearly show a characteristic four-day pattern of improved health subsequent to leaving DST in fall: We observe the 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 disappearing 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 in Figures A1b to A6b, one obtains exactly the same pattern using the full sample (Figure B1b), heart attacks (Figure B2b), injuries (Figure B3b), respiratory and metabolic admissions (Figure B4b and B5b) as well as neoplastic admissions (Figure B6b) and even suicide attempts (Figure B7b). There is little room for interpretation whether these patterns could be due to voluntary behavioral responses of going to hospitals less following the transition when we see that the same pattern for, for example, heart attacks.[9]

We interpret the similarity of these four-day pattern across different disease groups as strong support for our identification strategy. The implication is that additional sleep leads to short-term health improvements across a broad range of disease groups for people who are *on the margin* to getting

[9] 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.

admitted to a hospital. The medical advice for most people on the margin to getting hospitalized is certainly to lay down and rest, which is essentially what one hour of additional sleep represents.

Note that the medical literature provides support of our notion that sleep matters and crucially affects patients in critical health conditions. For example, it is well known that cancer patients suffer from fatigue and sleep disorders that are "not well defined or well understood at present (Ancoli-Israel et al., 2002; Davidson et al., 2002). Stepanski and Burgess (2007) note that "patients with cancer commonly report disturbed sleep, fatigue […]" and that the overall significance of poor sleep would be unknown. These medical facts and considering that the ICD-10 coding solely refers to the main diagnosis of a patient with an overnight stay—and *not* the first time the disease is diagnosed—the significant decrease in neoplastic admissions by 1 per 100,000 pop. is entirely in line with the medical state-of-the art knowledge about sleep and cancer patients.

However, despite the very characteristic four-day drop in admissions for several diagnoses, not all diagnoses seem to react in the same way, which is also reassuring. We do not observe much evidence for sleep-related short-term admission changes when it comes to *(i)* drug overdosing, and *(ii)* infectious diseases in Figures A8 and A9. All curves are very flat around the zero line in the week following the change in clocks. Drug overdosing should be of function of very time-persistent individual issues such as addiction and not be triggered by one hour more or less sleep. Overall, we interpret the fact that drug overdosing and infectious diseases do not seem to be affected by either spring or fall time changes to be in line with our identification strategy.

As seen in Figures 2a and 3a (as well as A1a to A6a) there is not much evidence for severe negative health shocks after the spring DST change. The curves in Figures 2a and 3a fluctuate slightly, which is not surprising given the powerful data, but overall they are relatively flat around the zero line. Figures A1a to A9a show the graphs for the *(i)* full sample, *(ii)* heart attacks, *(iii)* respiratory, *(iv)* metabolic, *(v)* neoplastic and *(vi)* infectious admissions. One may be tempted to interpret the observed significant increases for injuries (Figure A3a), metabolic admissions (Figure A5a), neoplastic admissions (Figure

A6a) and suicide attempts (Figure A7a) on Monday and Tuesday after the time change as evidence for negative health effects, but such an interpretation would be speculative. It should also be noted that correcting the confidence intervals for multiple testing (Benjamini and Hochberg 1995; Gelman et al. 2012), would eliminate any evidence for the alleged spikes in heart attacks or suicide on the Monday following spring DST as observed in Figures B3a and B7a (Coren 1996a, Berk et al. 2008; Lahti et al. 2010, 2011). We discuss possible explanations for the seemingly asymmetric effects below.

### 4.2 Evidence on Sleep as the Driving Mechanism

By exploiting the survey questions on sleep duration and deprivation in the BRFSS, next, we provide more than theoretical evidence that sleep is the main driving force behind our health estimates. Table 3 presents the results when we aggregate the effects at the weekly level to increase the statistical power. This is necessary because the sleep outcomes have not been surveyed every year in the BRFSS which results in smaller sample sizes with less statistical power (see also Section 2.1). Recall that our specification is still very rich, given that we use survey data here, and includes hundreds of fixed effects.

Column (1) of Table 3 suggests that, on average, people sleep an additional 0.1 hours in the entire week of fall DST (which would equal an average of 0.7 more hours in *one* night). However, there is no evidence that people sleep fewer hours as a result of spring change. Note that this average estimate of 0.1 may appear small but is, in fact, relatively large when one considers that it is the *average* population sleep effect. The reason is that we expect only sleep deprived people to effectively sleep one hour more for one day. Knutson et al. (2010) report that about ten percent of the US population would regularly sleep less than six hours per night. Scaling the 0.1 hours estimate with this number, we would find that the sleep deprived gained 1 hour of sleep—as a weekly estimate. Again, it is very data demanding to obtain precise sleep estimates on a daily level using survey data. In Section 2.3 we discussed potential issues related to measurement errors which could attenuate the estimates additionally.

Column (2) of Table 3 provides consistent further evidence on sleep as the main underlying mechanism of the identified health effects: The share of Americans who unintentionally fall asleep during the day decreases by a significant 4.4ppt (12.6%) in the week of leaving DST in fall, but the effect is insignificant in spring. The analogous graphical representation with the plotted daily effects is in Figure 4. It is remarkable that we observe the exact same characteristic four day pattern after the fall DST transition as above—despite the fact that the measures are self-reported and from a different continent. Figure 4a also provides some evidence that spring DST may increase the share of tired people on the Monday and Tuesday following spring DST by a significant 0.5 and 1ppt.

In line with the small sleep effects, there are a few possible explanations for the asymmetrical spring and fall DST effects that we observe. One obvious possibility is that people make asymmetric behavioral adjustments with respect to their sleep time between entering and leaving DST. In spring, for example, people may be more likely to go to bed an hour earlier to ensure that they are not tired the following day, but such behavioral adjustment may occur less in fall when people gain sleep. Media exposure may be the driving force behind this asymmetric sleep adjustments. When springing forward one hour in spring, experts in the media regularly warn about the dramatic health dangers that DST could trigger. One likely consequence of this media exposure is that people on the margin are effective in adjusting their bedtime and act more carefully on the days following the spring change. Finally, it is possible that DST changes may have different effects on sleep for different types of people (Lahti et al, 2008). For example, marginal individuals may be more affected by the fall than the spring transition. If morning types are more affected by spring DST and are also more likely to be hospitalized (e.g. because they tend to be older and more fragile), then estimating different Local Average Treatment Effects (LATEs) could explain the asymmetries. Finally, it is also thinkable that there exists a biological explanation of non-linear health effects when sleep deprived humans gain vs. lose one hour of sleep.

**[Insert Table 4 and Figure 4 about here]**

Fortunately, the observed asymmetry provides us with a direct test on the role of ambient light: Following the DST transition in spring, where ambient light shifts by construction and we effectively find no influence on sleep, we do not observe much evidence of negative health effects. We take this as evidence that changes in ambient light would have minimal direct effects on population health, which is intuitively plausible. In addition, we carry out a robustness check with region-calendar-day fixed effects to absorb daily changes in ambient light and find robust effects for the US (results available upon request).

In sum, given theoretical insights, the BRFSS evidence on the role of sleep and tiredness, and the non-significant impact of ambient light shifts in spring, we are confident to interpret the fall estimates as primarily driven by changes in sleep.[10]

### 4.3 Evidence on Self-Reported Health

Figures 5 and 6 again plot the estimated coefficients $\beta_1$ and $\beta_2$ of equation (1). Figure 5 shows the share of respondents who self-report that they are in excellent health, and Figure 6 shows the share of respondents reporting fair or poor health. All point estimates are plotted along with 90% confidence intervals. Despite the self-reported nature of the dependent variables and an entire different population, all results resembles the hospitalization pattern above surprisingly well.

**[Insert Figures 5 and 6 about here]**

Let us first focus on the health effects of the fall change. Figures 5b and 6b plot the fall effect coefficients $\beta_2$ of equation (1). Figure 6b yields no evidence that the share of respondents in bad health changes by more than +/- 2ppt. However, Figure 5b provides evidence that the share of Americans in "excellent health" increases by a statistically significant 1ppt on Monday after fall DST. This effect persists until Thursday before it dissipates, and has again exact the same pattern as the hospitalization and tiredness effect above. The size of the probability-weighted coefficient appears to be relatively small but would translate into about 2.5 million marginal Americans who would report excellent instead of very good health for four days.

[10] We are unable to tease out whether it is sleep *quality* or *quantity* that improves health. We believe it is both, but our data on sleep are not rich enough for a more rigorous analysis.

The distinct health improvement pattern for four days also holds up in robustness checks. For example, Figure B3b shows the same health improvement pattern when the underlying model makes use of the full sample—all 52 weeks of the year instead of the 12 weeks—and does not weight the regressions.[11] The improvement in self-reported health in Figure B3b also lasts for about four days before it disappears again. All pattern also remain robust when we explore movements from SAH category three (good health) to category two (very good health) or movements between categories one (poor health) and two (fair health). Detailed results are available upon request.

The health effects of entering DST in spring are illustrated in Figures 5a and 6a. As seen, in line with the findings above, the spring effects are not as distinct as the fall effects.

### 4.4 Regression Discontinuity Estimates

The regression discontinuity approach of equation (2) yields results that are in line with the findings from the daily-dummy approach of equation (1). Table 4 reports the estimated health effects of entering and leaving DST, for four binary self-reported health outcomes: excellent health; very good or excellent health; fair or poor health; and poor health. Again, one observes no health effects following the start of DST in spring. The estimates are all close to zero, with relatively tight standard errors.

The transition out of DST in fall, however, generate significant health benefits. This specification finds that people in the lower category of self-reported health benefit more. The time shift reduces the share of people reporting either "fair or poor health" by about 2ppt, implying that 13% of people in this category are pushed towards the "good" category as a result of the fall DST transition. Similarly, the share of people reporting poor health falls by about 1.3ppt. The coefficient estimate is statistically significant at the 1% level. Relative to the mean, it corresponds to a large 25% reduction of people in the

[11] 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).

poor health category. Again, one percentage point change in these models equals about 2.5 million marginal Americans.

**[Insert Table 3 about here]**

The RD estimates are graphically represented in Figures B6a and B6b in the Appendix. The plots are based on the predicted outcome variables of the corresponding regression outcomes, which are then collapsed by days to the fall time change. The fitted lines are polynomials of order 3. As with Doleac and Sanders (2015) and Smith (2016), we still observe some leftover day-of-week patterns despite having controlled for the day-of-week effects in the regression model.[12] We believe these seasonalities are better controlled for in our main model specification, but nevertheless, this approach reinforces the notion that sleep extensions can significantly improve health.

### 4.5 Effects by Weather and Pollution Conditions and the Role of Ambient Light Changes

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

We hope to learn more about the underlying behavioral mechanisms through the stratification via ambient conditions. The underlying hypothesis here is that weather conditions determine how and where individuals spend their time (Gebhart and Noland, 2014). Furthermore, being active outside may provide more opportunities for dangerous activities that are more likely to trigger health shocks when humans are sleep deprived. Because pollution has also been shown to have a direct effect on hospital admissions (Schlenker and Walker, 2016), we expect pollution to operate in interaction with changes in sleep pattern.

[12] Doleac and Sanders (2015), for example, plot figures without weekends to better illustrate the patterns without being affected by the leftover day-of-week effects, but we include all data points.

The first four columns of Table A2 stratify the DST effects by *(i)* temperature, *(ii)* rainfall, *(iii)* sunshine, and *(iv)* cloudiness. The “better” the weather conditions for outdoor activities—higher temperatures, more sunshine, less rainfall, and less cloudiness—the more admission rates increase in the week following spring DST. However, interestingly, ambient conditions do not matter at all in fall. This finding is in line with the asymmetric health findings, and we interpret it as suggestive evidence that the spring DST effects are accompanied or “confounded” by behavioral adjustments while this is not the case for the clean and distinct fall effects. Obviously, ambient conditions make a difference when clocks are set forward in spring but they do not matter when humans’ night sleep is extended in fall. An obvious explanation could be that, when the opportunity costs are high and outdoor conditions better, people are less likely to adjust their bedtime and go to bed earlier.

In addition to the stratification by ambient outdoor conditions, as mentioned, we also 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.

Next, we exploit the BRFSS to test whether DST transitions may affect the level of physical exercise. More exercising as a results of more ambient light could potentially be a confounding mechanism that may affect self-reported health or hospitalizations. As shown in Figure B7, we do not find evidence that “sport kills” and that exercising is a main underlying mechanism here. Also note that hospital admissions as a result of more time spent outdoors would most likely increase the number of admissions due to accidents, which we do not observe in the data.

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, which also speaks against changes in sunlight triggering the health effects.

Columns (5) to (8) of Table A2 show that admissions increase whenever pollution conditions worsen, independent of spring and fall time changes. The fact that the pollution effects are not asymmetric suggests, in line with the literature, that pollution is always bad for humans on the margin. As the interaction terms show, in the week of spring or fall DST, admissions always increase with higher air pollution. In line with above, the plain *End DST* coefficient is consistently negative and highly significant indicating human capital improvements when the sleep deprived get more sleep. However, when pollution is high in the week after fall DST, part of this general decrease in admissions is offset as indicated by the interaction term.

### 4.6 Placebo Estimates

We provide two approaches to placebo regressions. The first exploits an outcome measure that cannot—by construction—be affected by DST in the current year: having had a flu shot in the past year. The second approach carries out a permutation test and estimates weekly placebo DST effects for the rest of the year without time changes.

Figures B5 plots the results for having gotten a flu shot in the past year. Both Figures B5a and B5b show no evidence of a systematic reaction in either spring or fall DST. All curves are very flat around the zero line in the week following the change in clocks.

Our second approach for estimating placebo effects is a permutation test: We start in January of each year and select a six-week window 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 spring DST week. Next, we move the six week window one week further into February and repeat the approach. We permutate until week six of our selected sample hits the true DST week and continue with six week windows post spring DST until end of June.[13] As such, we obtain a total of 22 spring placebo weekly

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

DST estimates. We repeat the exercise in a similar fashion for fall DST until the end of the year and obtain 23 pseudo-fall DST estimates.

The distributions of the weekly coefficient estimates are plotted in Figures A10a and A10b along with the true spring and fall estimates. In line with all results, Figure A10a shows that the small and non-significant spring DST estimate is not an outlier and falls within the standard range of weekly estimates when running the model in equation (2). Furthermore, Figure A10b shows that the substantial decrease in hospital admissions following fall DST does not fall within the statistical placebo estimate distribution during the second half of the calendar year.

### 4.7 More Evidence on Alternative Mechanisms

Our approach provides clean, reduced-form causal effects when interpreting the findings as intent-to-treat effects of entering and leaving DST on population health. In other words, we can directly interpret the identified effects as “The effect of DST transitions on health.”

Above we provide several “mechanism checks” to show that time shifts caused by fall DST transitions induce changes in sleep patterns, and that these are the main driving force behind the effects on health. Furthermore, we provide evidence that ambient conditions matter in spring and may confound the spring estimates, but that there is no evidence that this is the case for our main, clean and distinct, fall effects. Recall that, without behavioral adjustments, setting the clocks back by one hour from 3am to 2am in fall simply means that sleep-deprived people will sleep one hour longer. Changes in sleep pattern due to DST transitions have been convincingly demonstrated by sleep researchers (Kantermann et al. 2007; Berk et al. 2008; Barnes and Wagner 2009).

Doleac and Sanders (2015) exploit DST to study the impact of ambient light on crime rates. They show that robberies *decrease* in the days following the *spring* DST change. Entirely in line with the findings in this paper, Doleac and Sanders (2015) find a significant impact of ambient light following the spring transition but not much evidence for behavioral changes in fall. Moreover, unlike the sleep mechanism, a potential robbery effect in fall would imply adverse health effects suggesting our estimates would be lower bounds.

Smith (2016) also 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 channel for the mechanism.

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 substantially. Moreover, both studies focus on the spring transition whereas this study focuses on the fall transition. 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 case numbers certainly do 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.

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

There is anecdotal evidence of humans believing 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 how daily activities are carried out, e.g. work productivity, or how they are perceived and enjoyed. Even when we are personally aware of the fact that less sleep would make 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 economist argue (Mullainathan, 2014). As yet another explanation for the phenomenon of sleep deprivation: It could simply be that an increasing work load and private 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 5 provides an attempt to categorize and monetize these benefits. Obviously, these calculations are based on several assumptions but should nevertheless provide a basic framework for such an exercise. Scaling the coefficient estimate in column (3) of Table 3 back to the daily level would yield an average 0.7 hours one-time increase in the sleep duration. Knutson et al. (2010) report that the share of Americans who regularly sleep less than six hours per night is about 10 percent. It seems reasonable to assume that these 10% sleep-deprived Americans are primarily those who gained more sleep in the fall DST transition.

The first column of Table 5 monetizes the benefits of the (subjective) improvement in self-reported health for 2.5 million marginal Americans in the four days following the fall time change (Figure 5b, B6b). One Quality Adjusted Life Year (QALY) is typically valued with about $100K, or ~$270 per day (Kniesner et al. 2010). Assuming that the shift in subjective health equals an increase in quality-adjusted well-being 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 $550M or about $1.7M per one million population. Whereas these back-of-the envelope calculations naturally rest on a plenitude of assumptions, they are yet useful to assess the monetary dimensions of the different potential benefits of more sleep.

Colum (2) assesses the value of potential increases in work productivity when sleep-deprived workers gain more sleep and feel rested. According to Gibson and Schrader (2015), the short-term wage returns for an additional hour of sleep would equal 1.5% of the wage. For the average American daily wage of $230, this would translate into $14 over four days. Assuming that this productivity gain only applies to the 10% sleep deprived full-time employed Americans, it would sum to $160M or $0.5M per one million population. It is easy to test the sensitivity of these estimates with respect to their underlying assumptions. For example, if the work productivity gain would not just apply to 10% but 20% of US full-time employees, the total monetized benefits would obviously double to $1M per one million population.

**[Insert Table 5 about here]**

The next three columns of Table 5 monetize the value of avoided hospital admissions. Figure 2b suggest 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 (Figure 5b). 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 assessed 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 thus decomposed into €2000 for medical costs and about €500 for each lost labor and a loss in quality of life during hospital stays.

Finally, we put bounds on the number of avoided deaths per one million population. Smith (2016) quantifies the number of avoided traffic fatalities with 30 for the entire US (0.09 per 1M pop.). On an average day, 30 people per one million population die in Germany (Karlsson et al. 2015). With a lower bound decrease of 2% and an upper bound decrease of 10% for four days, we would obtain sums for saved statistical lives of between $12 and 60$ million per one million population (evaluated at $5M per life saved).

This little exercise illustrates that the individual-level benefits of additional sleep could be dramatic for the marginal individual who is 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 when monetized with the standard economics tools. The reason is simply that a large number of people are affected by such well-being effects. The monetized health effects of the fall DST change alone—as identified in this paper—add up to $2 per person and the entire population. Eighty percent of this amount goes back to an assumed 20% increase in the quality of life for sleep deprived people who feel as being in better health for four days after gaining more sleep.

## 5. DISCUSSION AND CONCLUSION

This paper exploits the quasi-experimental nature of Daylight Saving Time (DST) to assess whether more sleep may affect human capital, in this case human health, in the short-run. It is one of very few causal studies on this topic and exploits one decade of both a very large survey dataset for the US and the universe of all hospital admissions in 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 DST happens only twice a year, and always on Sunday nights around similar times of the year, it is crucial to run rich econometric specifications that consider weekday effects in addition to general and specific seasonal adjusters. Our empirical models yield surprisingly consistent results for the US and Germany, from the mild self-reported health outcomes to more severe disease categories that result in hospitalizations.

Overall, the findings provide strong support for the notion that humans' most time-consuming activity, sleep, does affect their health status. The clearest evidence of the positive health effects of sleep stem from the time change in fall. 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 heavily sleep deprived (Moore et. al., 2002; Roenneberg et al., 2007; 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 daylight savings time in fall. About 2.5 million more Americans consider themselves in better 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 also demonstrates a characteristic four-day drop in admissions in the days following fall DST 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, 2015), increases in obesity (Giuntella and Mazzonna, 2015), or 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 and companion research in economics. Under some assumptions, we assess the societal benefits of the marginal sleep deprived person with $50 per day due to a higher quality of life when feeling rested. Moreover, we monetize a four day long avoided hospital admission 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, one could imagine sleep affecting mood, cognitive skills and health cumulatively over time, 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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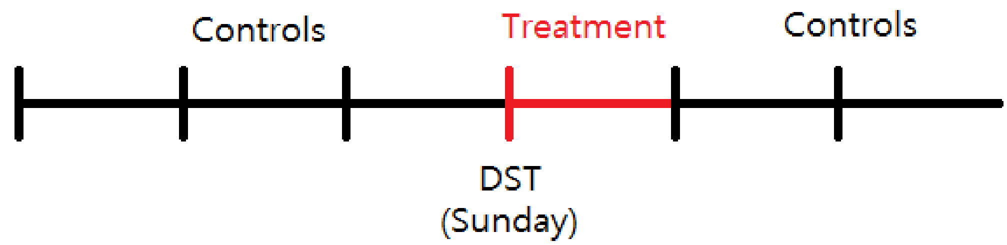
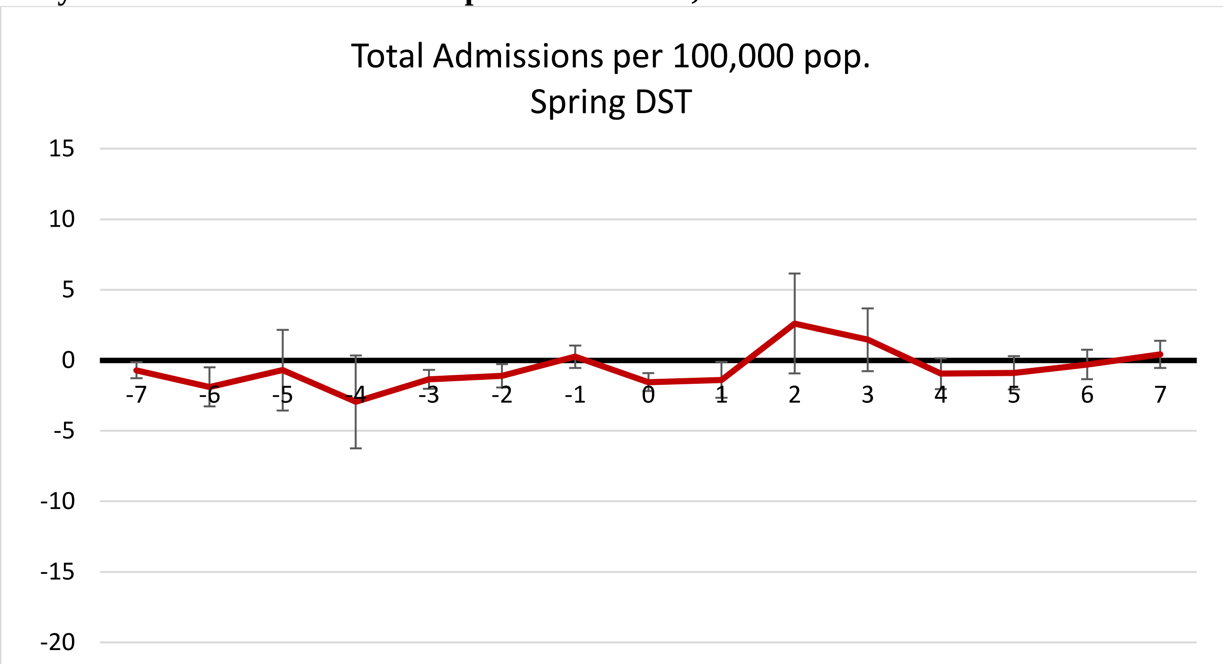
## Figures and Tables

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



***<u>Figure 2a and b, Hospital Census:</u>***

**Daily Effects of DST on Total Hospital Admissions, 2000-2008**



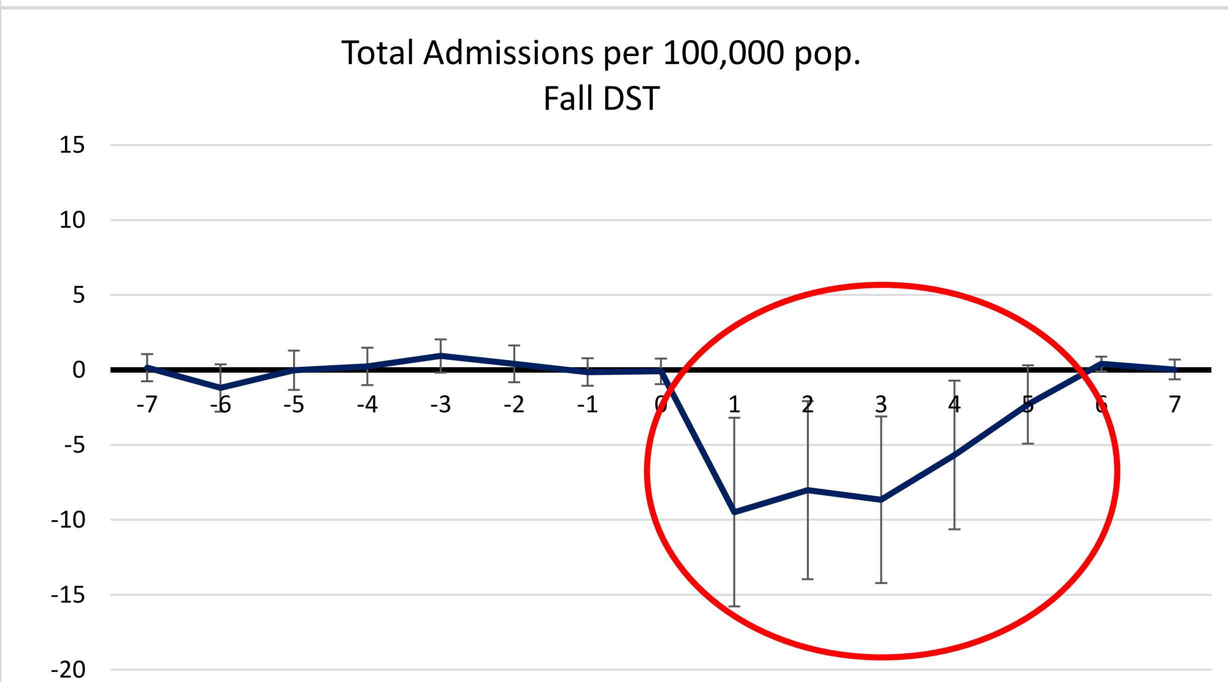
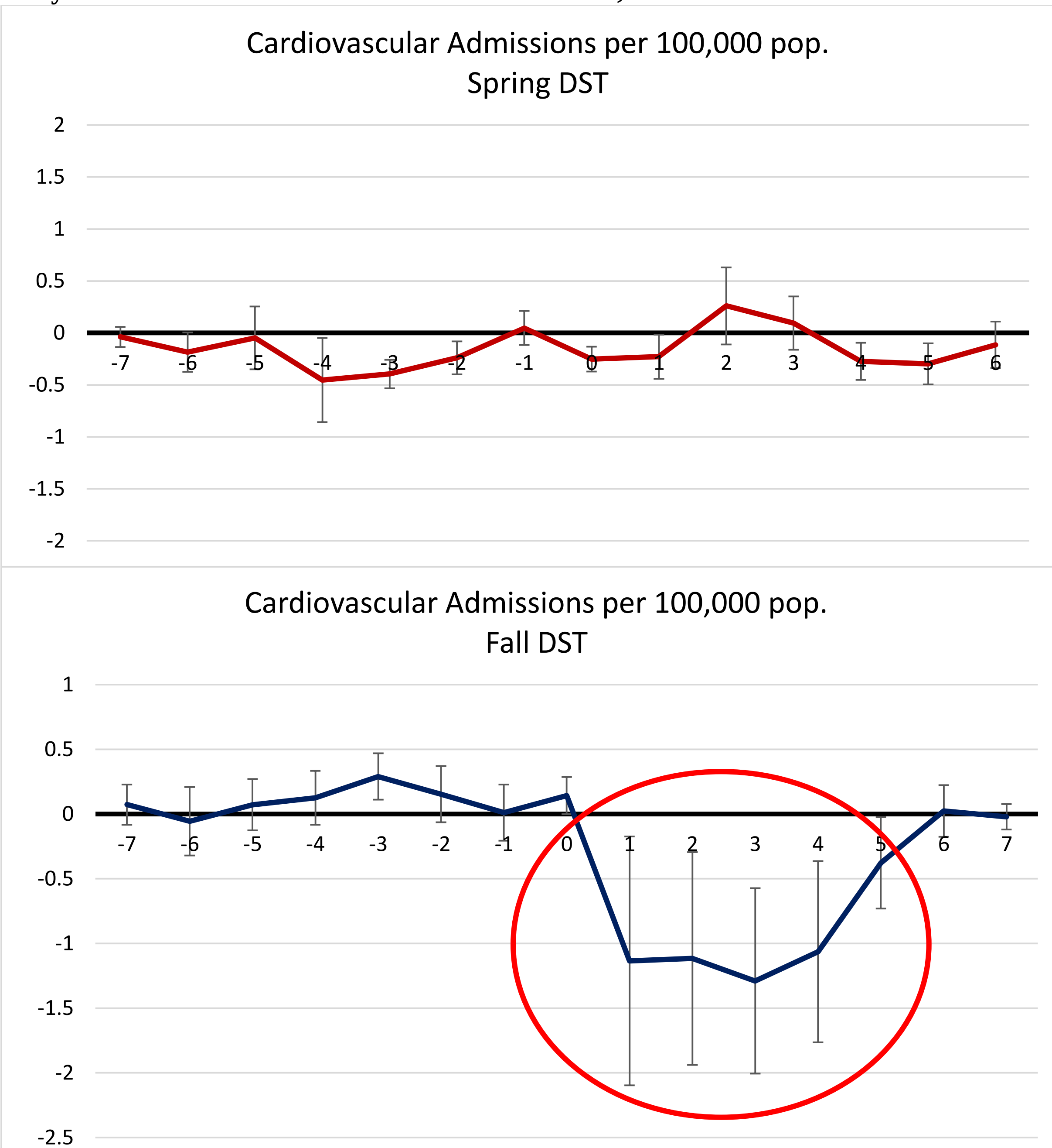
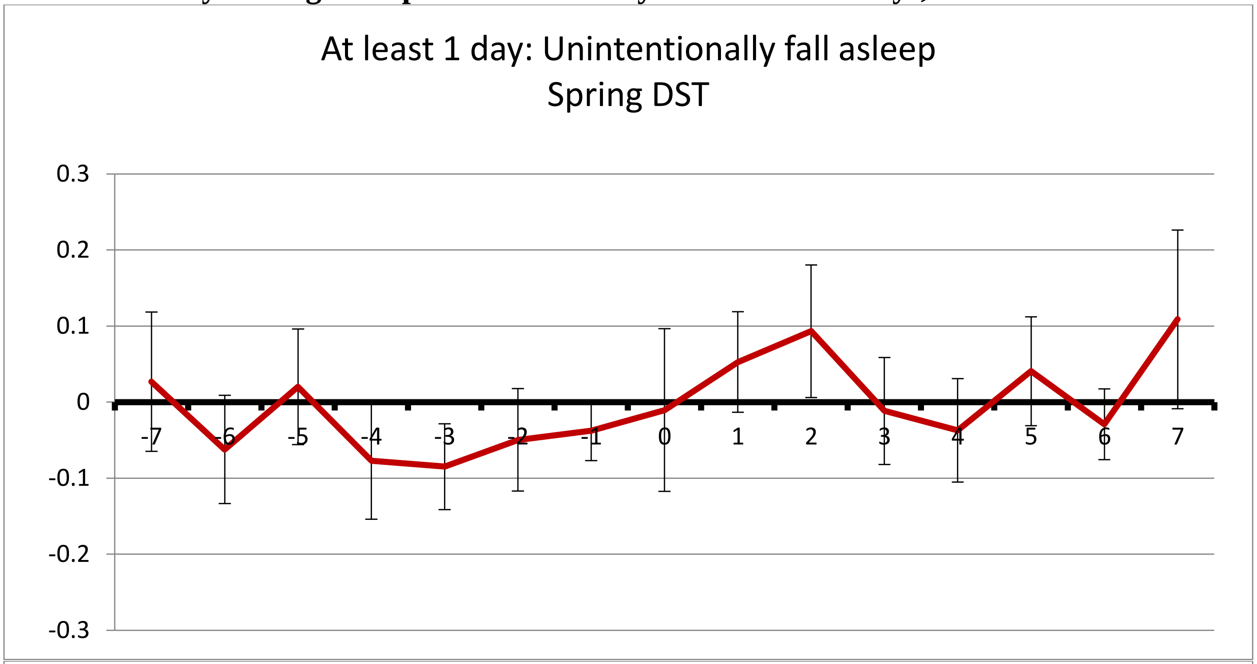



***Figure 3a and b, Hospital Census:***
**Daily Effects of DST on Cardiovascular Admissions, 2000-2008**



***<u>Figure 4a and b, BRFSS:</u>***
**Unintentionally Falling Asleep on at least 1 Day in the Past 30 Days, 2001 2010**



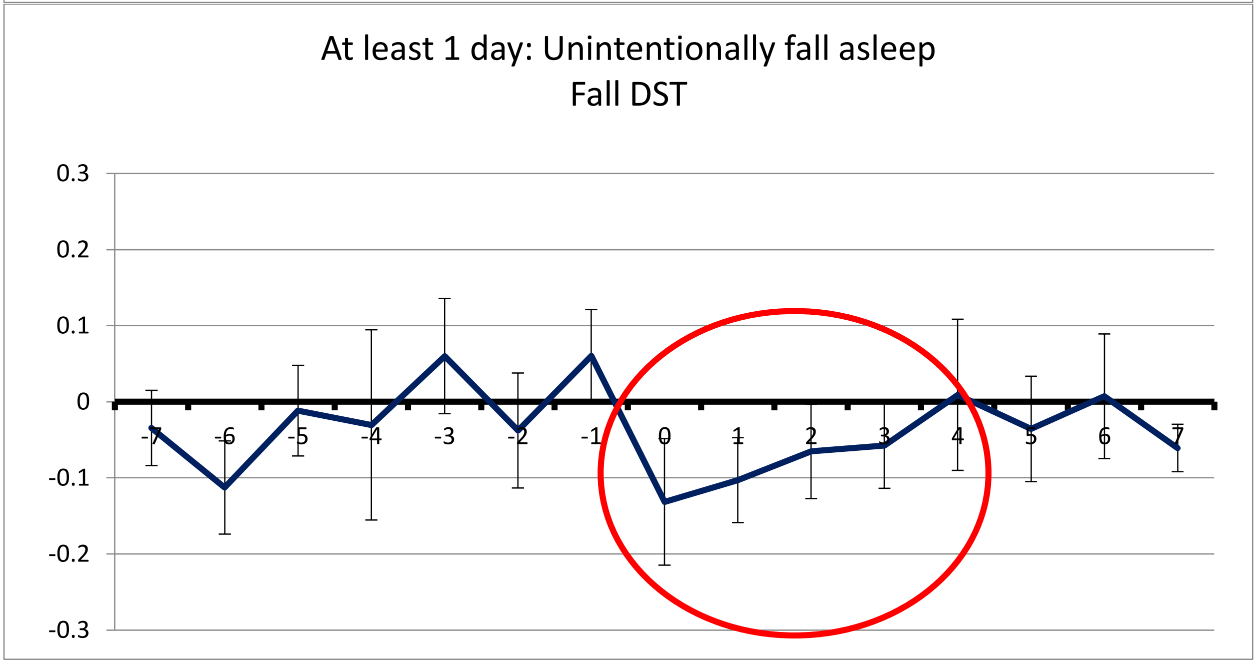
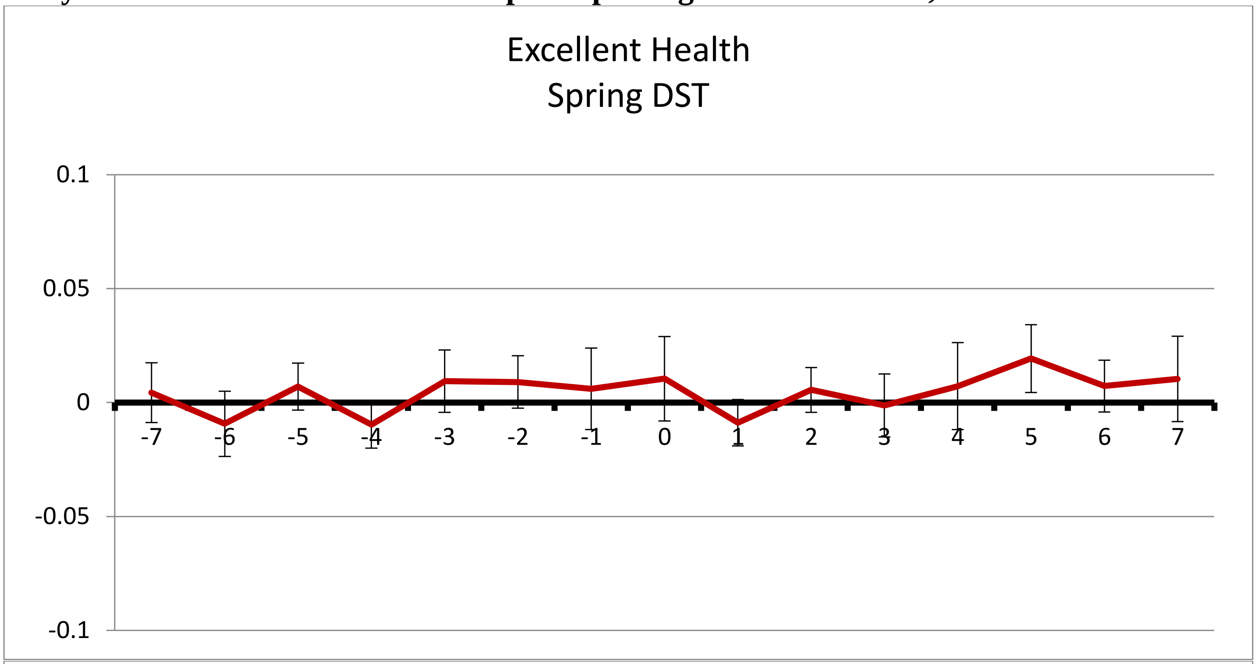



***Figure 5a and b, BRFSS:***
**Daily Effects of DST on Share of People Reporting Excellent Health, 2001-2010**



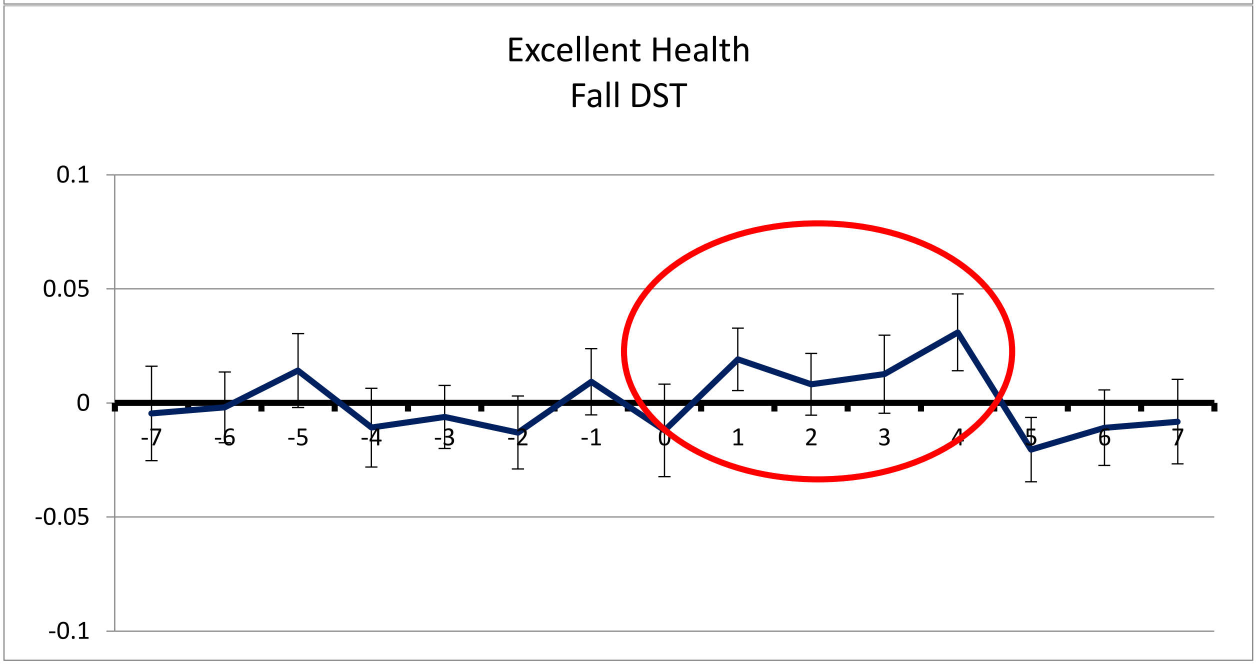
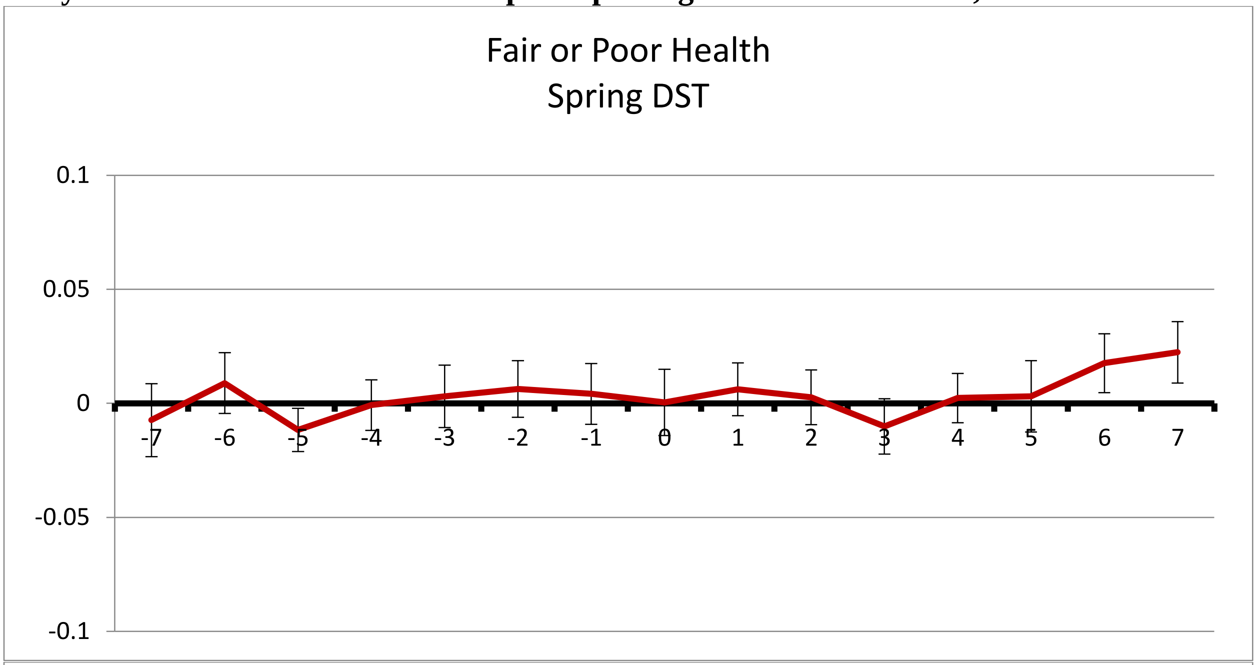



***Figure 6a and b, BRFSS:***

**Daily Effects of DST on Share of People Reporting Fair or Poor Health, 2001-2010**



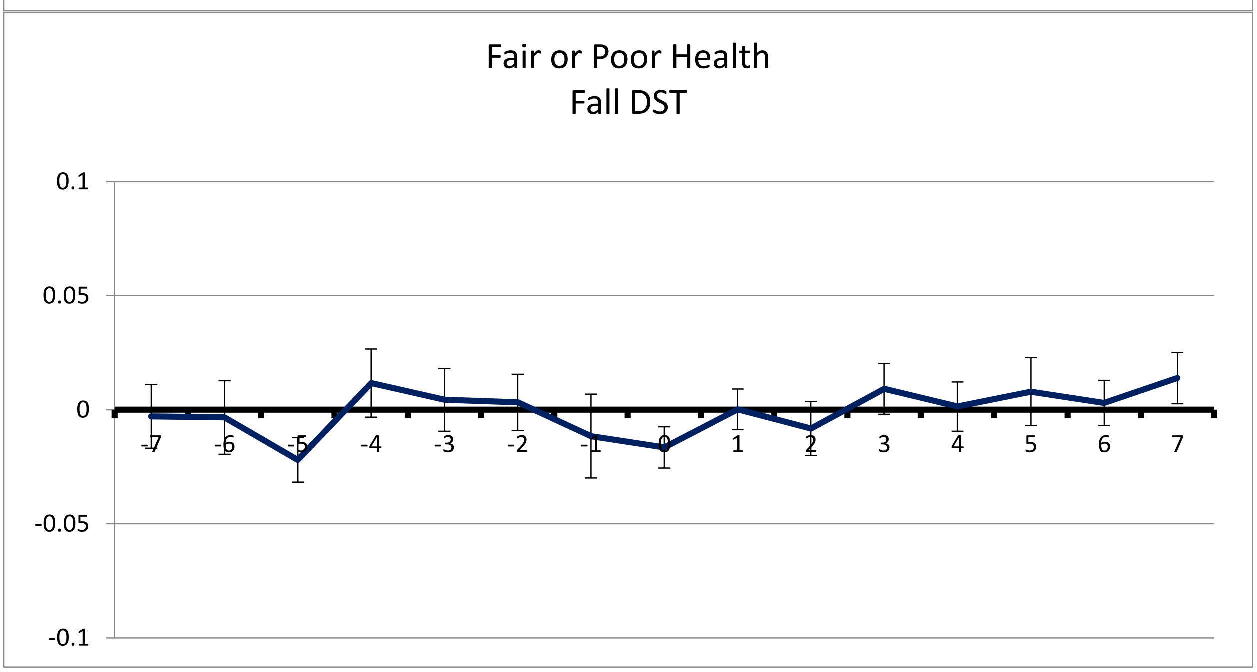



***Table 1: DST Germany***

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

| Year | DST spring | DST fall |
|---|---|---|
| 2000 | 3/26/2000 | 10/29/2000 |
| 2001 | 3/25/2001 | 10/28/2001 |
| 2002 | 3/31/2002 | 10/27/2002 |
| 2003 | 3/30/2003 | 10/26/2003 |
| 2004 | 3/28/2004 | 10/31/2004 |
| 2005 | 3/27/2005 | 10/30/2005 |
| 2006 | 3/26/2006 | 10/29/2006 |
| 2007 | 3/25/2007 | 10/28/2007 |
| 2008 | 3/30/2008 | 10/26/2008 |

***Table 2: DST US***

**Begin and End of Daylight Saving Time (DST) in the US (2001-2010)**

| Year | DST spring | DST fall |
|---|---|---|
| 2001 | 4/1/2001 | 10/28/2001 |
| 2002 | 4/7/2002 | 10/27/2002 |
| 2003 | 4/6/2003 | 10/26/2003 |
| 2004 | 4/4/2004 | 10/31/2004 |
| 2005 | 4/3/2005 | 10/30/2005 |
| 2006 | 4/2/2006 | 10/29/2006 |
| 2007 | 3/11/2007 | 11/4/2007 |
| 2008 | 3/9/2008 | 11/2/2008 |
| 2009 | 3/8/2009 | 11/1/2009 |
| 2010 | 3/14/2010 | 11/7/2010 |

***Table 3, BRFSS:***

**Weekly Effects of DST on Sleep, 2001-2010**

| | (1)<br>Hours of sleep in a 24-hour period | (2)<br>At least 1 in past 30 days: unintentionally falling asleep during day | (3)<br>At least 1 in past 30 days: Nodded off/ fell asleep while driving |
|---|---|---|---|
| Week of Begin DST | 0.00661 | 0.03074 | -0.00873 |
| (2am → 3am in spring) | (0.06822) | (0.02100) | (0.00708) |
| Week of End DST | 0.09900* | -0.04427** | -0.00842 |
| (2am → 1am in fall) | (0.05394) | (0.01895) | (0.00605) |
| **Controls** | | | |
| State FE | X | X | X |
| Easter and Halloween | X | X | X |
| Day of Week * Month FE | X | X | X |
| Month * Year FE | X | X | X |
| Linear & quad. time trend | X | X | X |
| Socioeconomic covariates | X | X | X |
| *Mean of dep. Var.* | *7.07* | *0.35* | *0.03* |
| $R^2$ | 0.0529 | 0.0655 | 0.0284 |
| Observations | 19,772 | 19,772 | 19,772 |

**Notes:** Standard errors in parentheses are clustered at the date level. *** Significant at 1% level, ** 5%, * 10%. Regressions are probability-weighted. *Week of Begin/End DST* are indicator variables equal to 1 if the interview is on the DST Sunday 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 (2) and (3) use binary measures, and column (1) has values between 0 and 24. The summary statistics of the dependent variables are in Table A1. Each column is one model as in equation (2).

***Table 4, BRFSS:***

**Effects of DST Transitions on Self Assessed Health (SAH) using Regression Discontinuity Approach, 2001-2010**

| | (1) Excellent health | (2) VG / Excellent | (3) Fair / Poor | (4) Poor health | (5) Excellent health | (6) VG / Excellent | (7) Fair / Poor | (8) Poor health |
|---|---|---|---|---|---|---|---|---|
| Entering DST | 0.0044 | 0.0040 | 0.0017 | -0.0021 | | | | |
| (Spring) | (0.0050) | (0.0063) | (0.0050) | (0.0025) | | | | |
| Leaving DST | | | | | 0.0010 | 0.0054 | -0.0232*** | -0.0125*** |
| (Fall) | | | | | (0.0062) | (0.0073) | (0.0054) | (0.0026) |
| *Mean of dep. Var.* | *0.19* | *0.51* | *0.18* | *0.05* | *0.19* | *0.52* | *0.18* | *0.05* |
| Observations | 388,982 | 388,982 | 388,982 | 388,982 | 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).

***Table 5: Economic Benefits of Sleep***

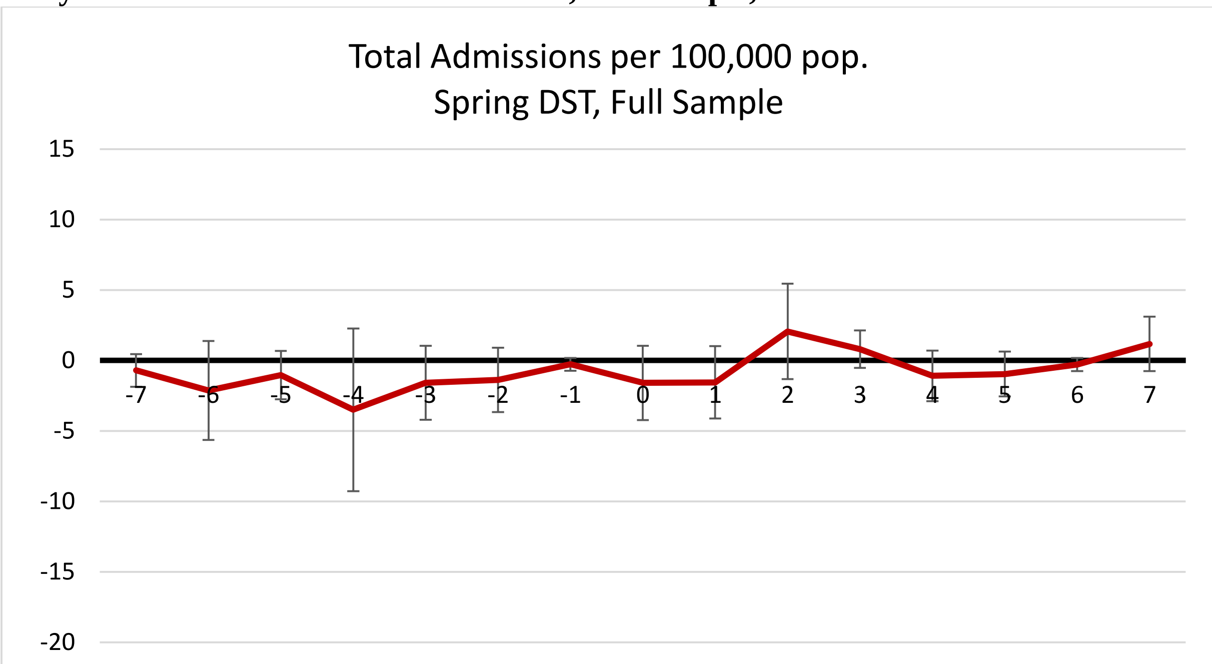
**Monetizing Health Benefits of Additional Sleep**

| | ***Mild Health Effects*** | ***Productivity Effects*** | ***Severe Health Effects*** | | | ***Mortality Effects*** |
|---|---|---|---|---|---|---|
| | BRFSS (Fig 2b, 4b; Tab. 3) | Gibson and Schrader (2015) | 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 1.5% 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*** | ***=$12*** | ***=€2000*** | ***=€450*** | ***=€550*** | ***=€5M*** (VSL) |
| | *2.5M/320M | *10% sleep deprived of 11.5M US full-time employees: 161M/320M | *100 | *(100/3) | *100 | *2.4 lifes (2%)/ *12 lifes (10%) |
| Sum per 1M pop. | **=$1.7M** | **=$500K** | **=€200K** | **=€15K** | **= €55K** | **=€12M-€60M** Germany: 30 deaths per 1M pop per day (Karlsson et al. 2015). |

**FOR ONLINE PUBLICATION**

## Appendix A: German Hospital Census

***Figure A1a and b, Hospital Census Full Sample:***
**Daily Effects of DST on Total Admissions, Full Sample, 2000-2008**



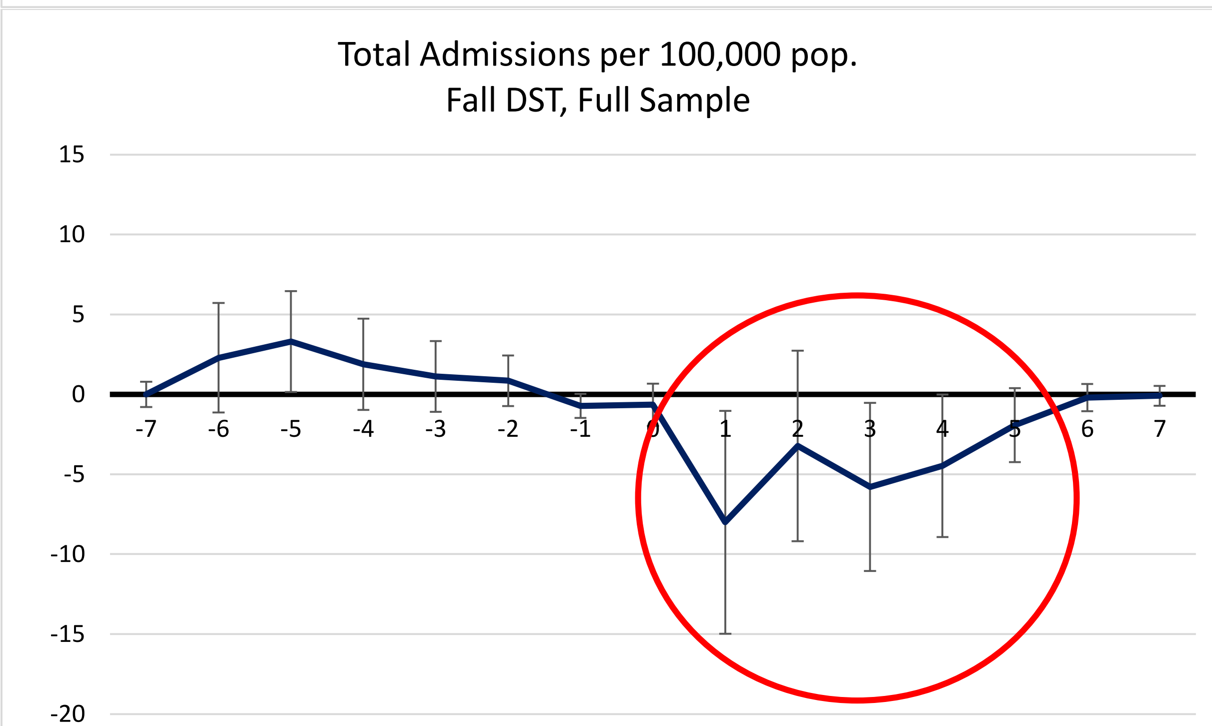
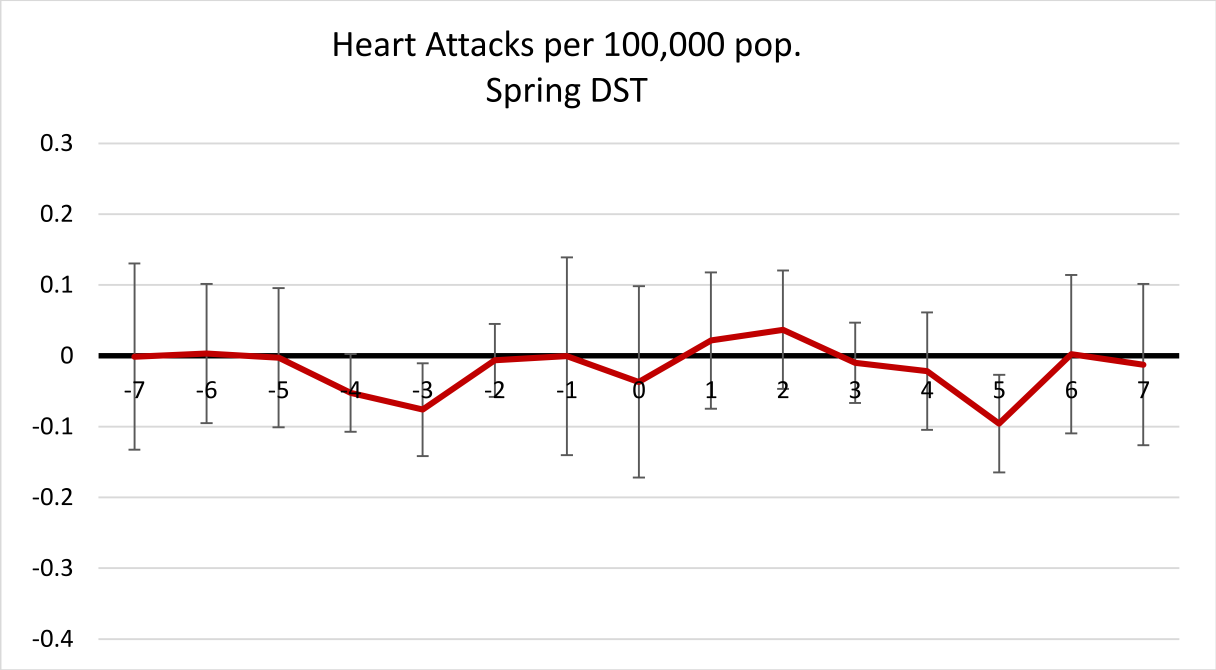



***Figure A2a and b, Hospital Census:***
**Daily Effects of DST on Heart Attack, 2000-2008**



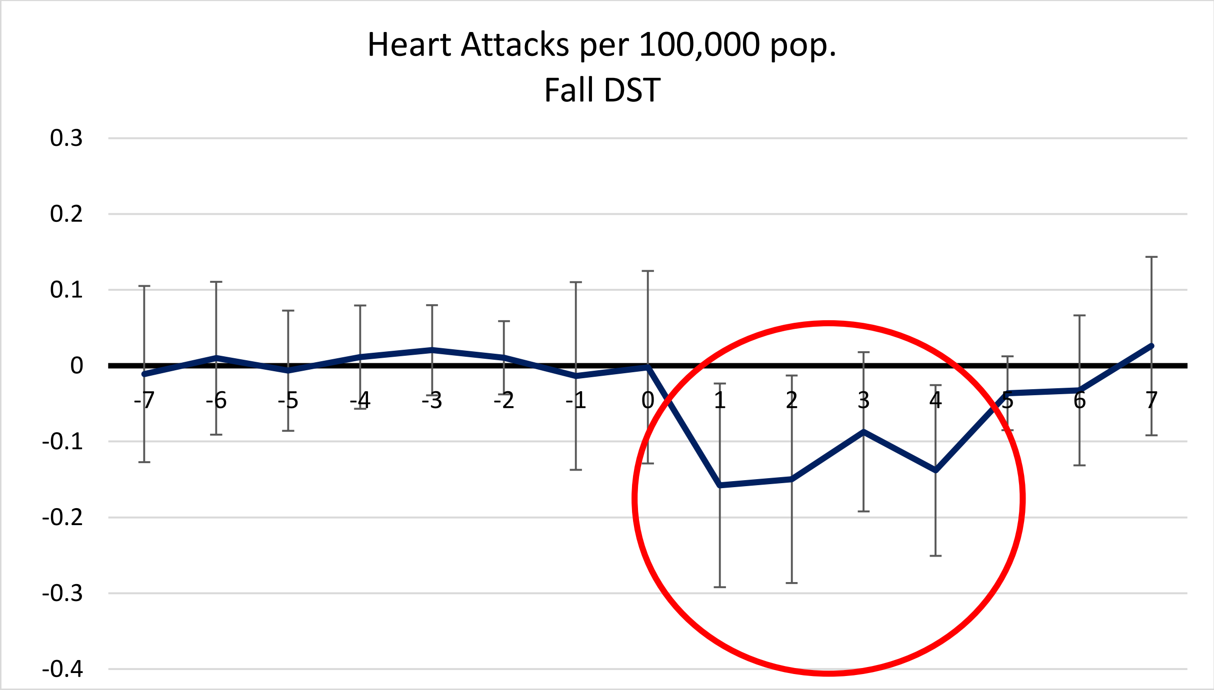



***Figure A3a and b, Hospital Census:***
**Daily Effects of DST on Injuries, 2000-2008**

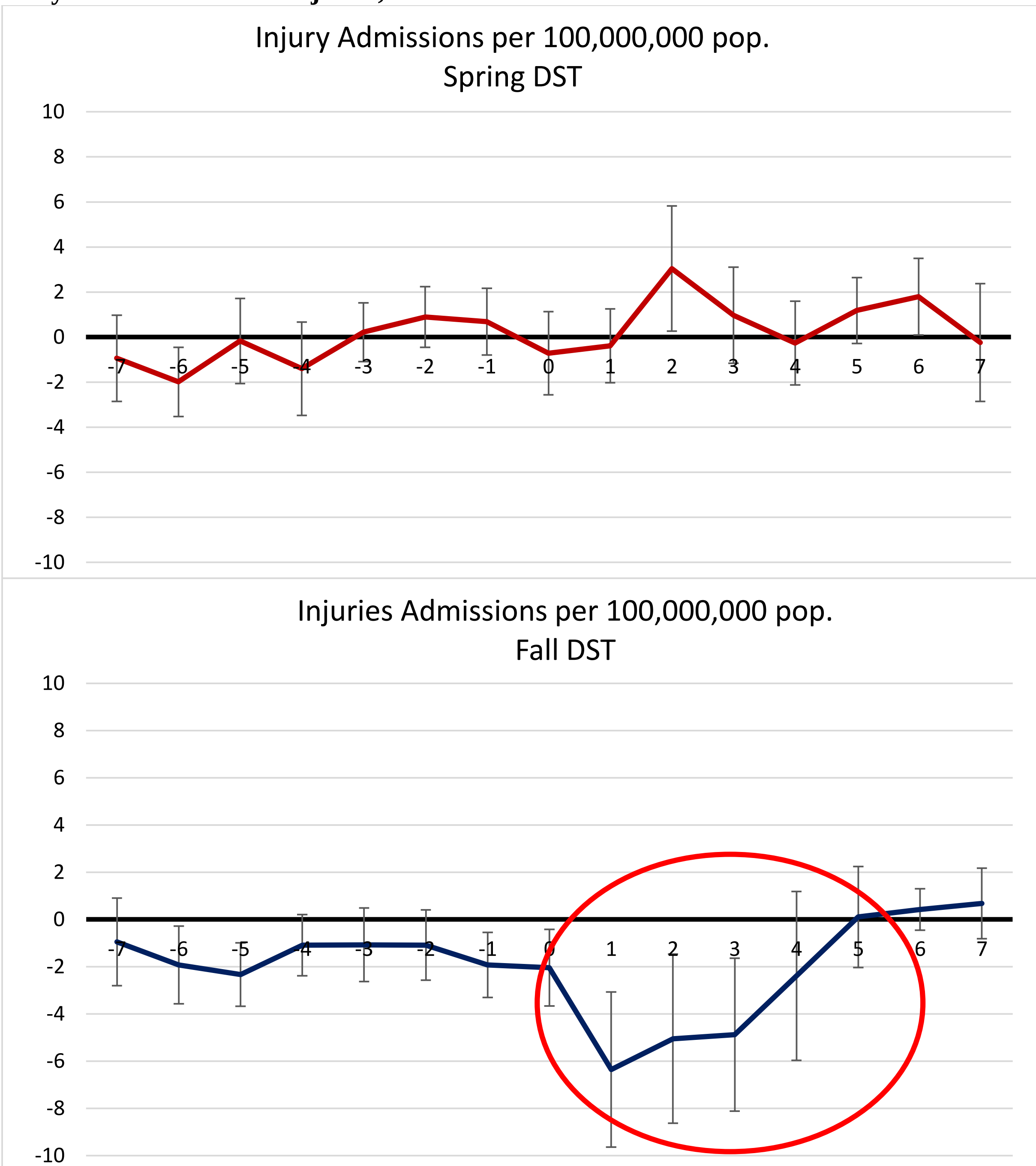
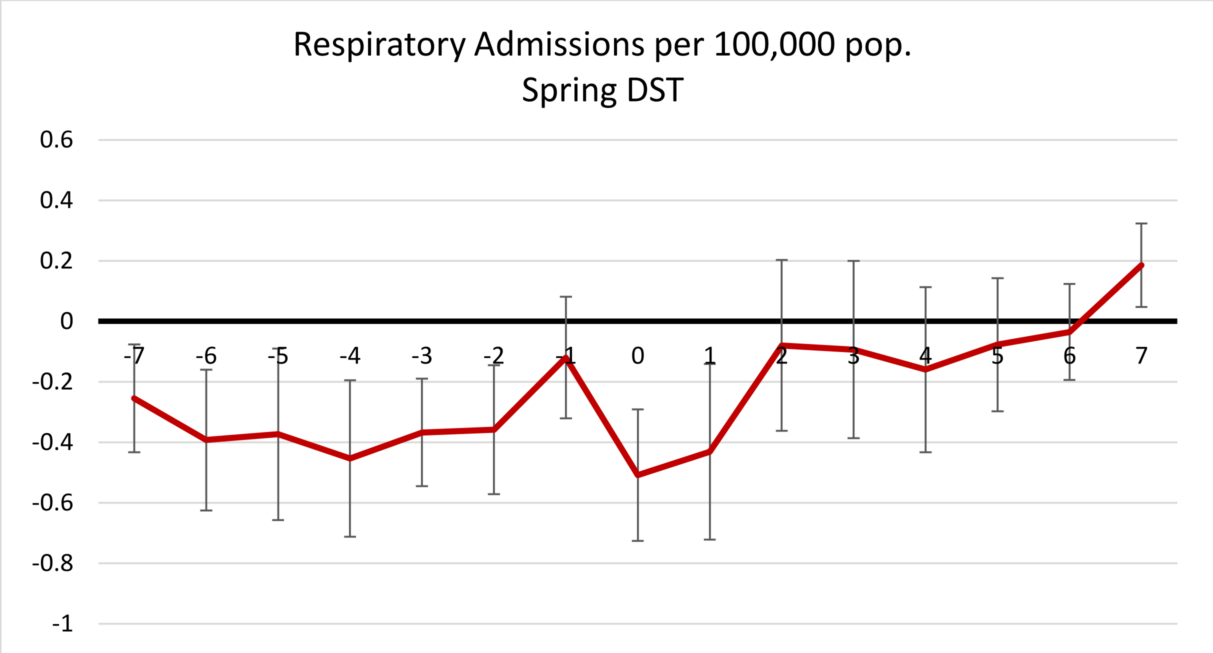


***Figure A4a and b, Hospital Census:***
**Daily Effects of DST on Respiratory Admissions, 2000-2008**



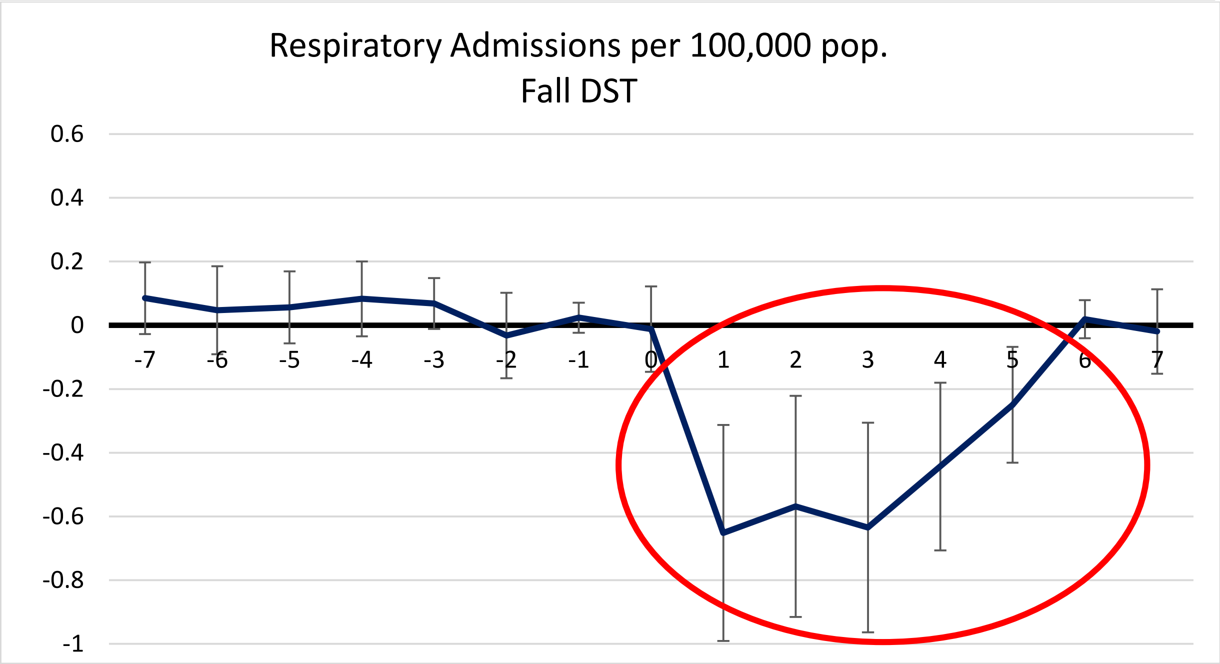




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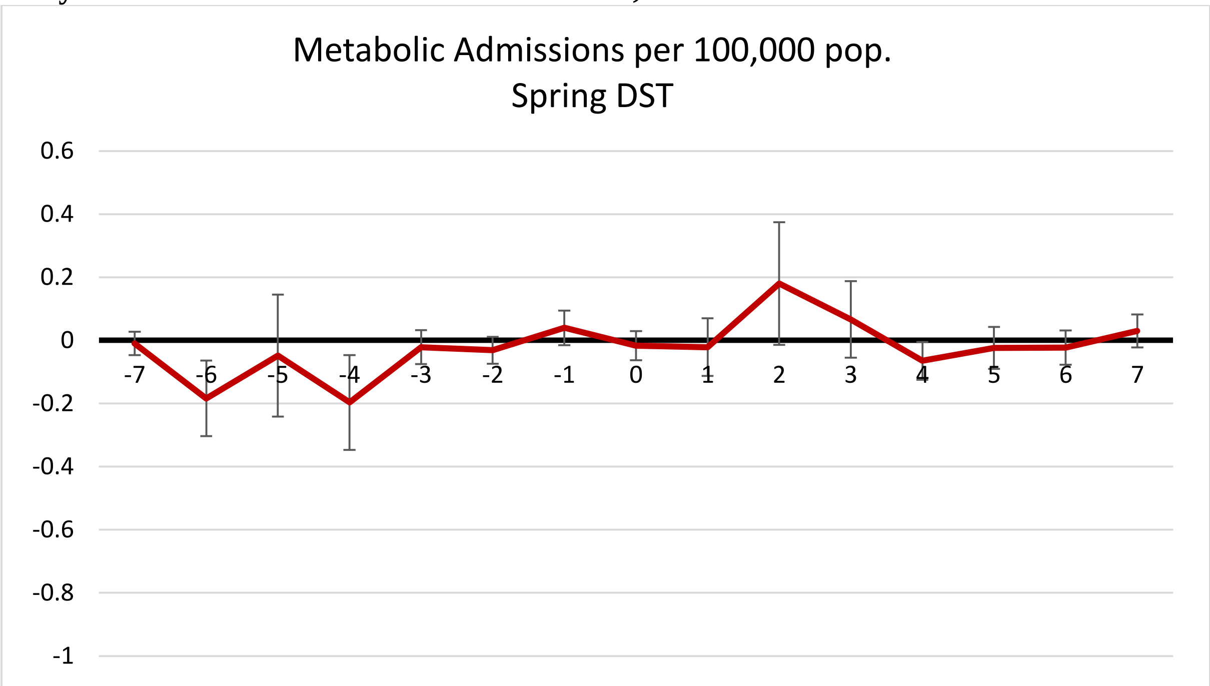
***Figure A5a and b, Hospital Census:***
**Daily Effects of DST on Metabolic Admissions, 2000-2008**



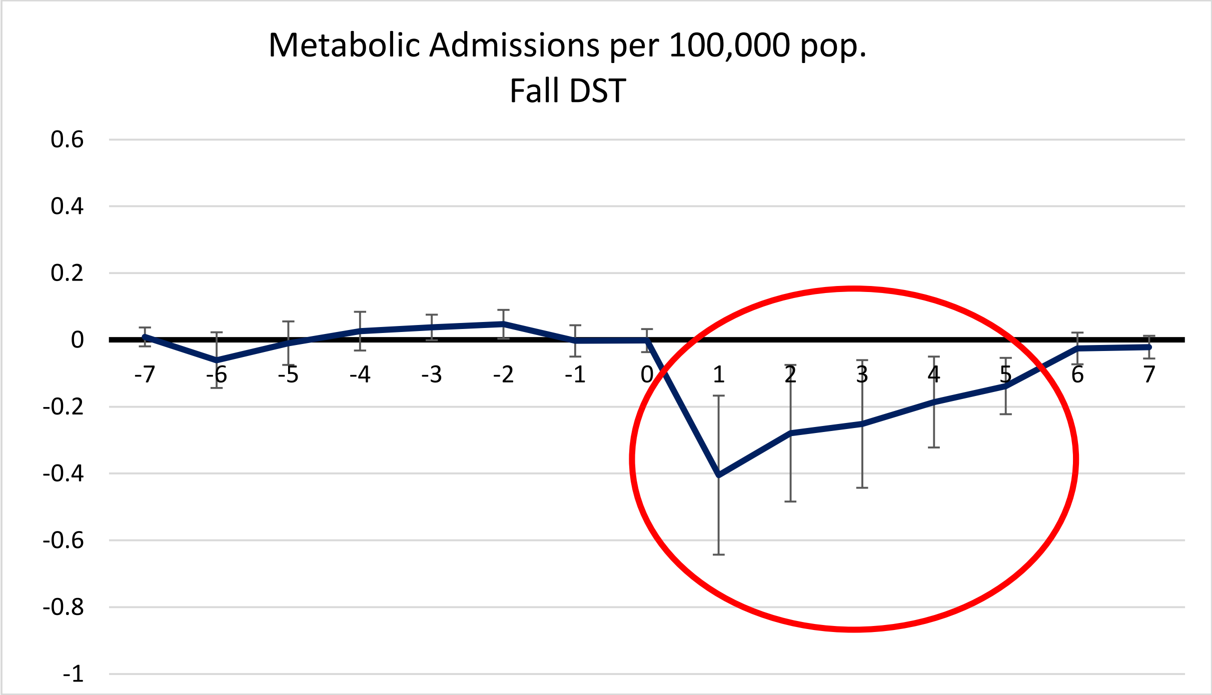



***Figure A6a and b, Hospital Census:***

**Daily Effects of DST on Neoplastic Admissions, 2000-2008**

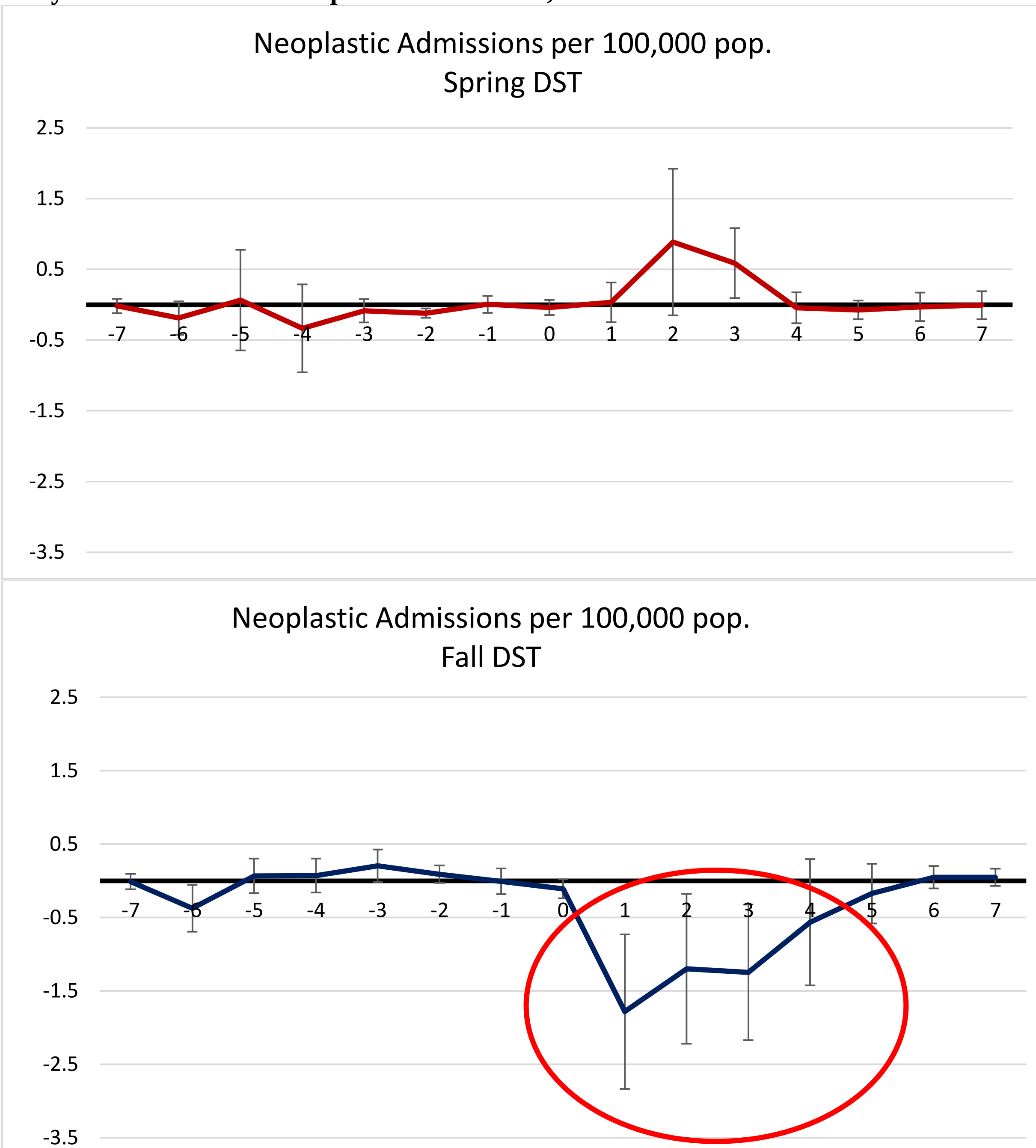
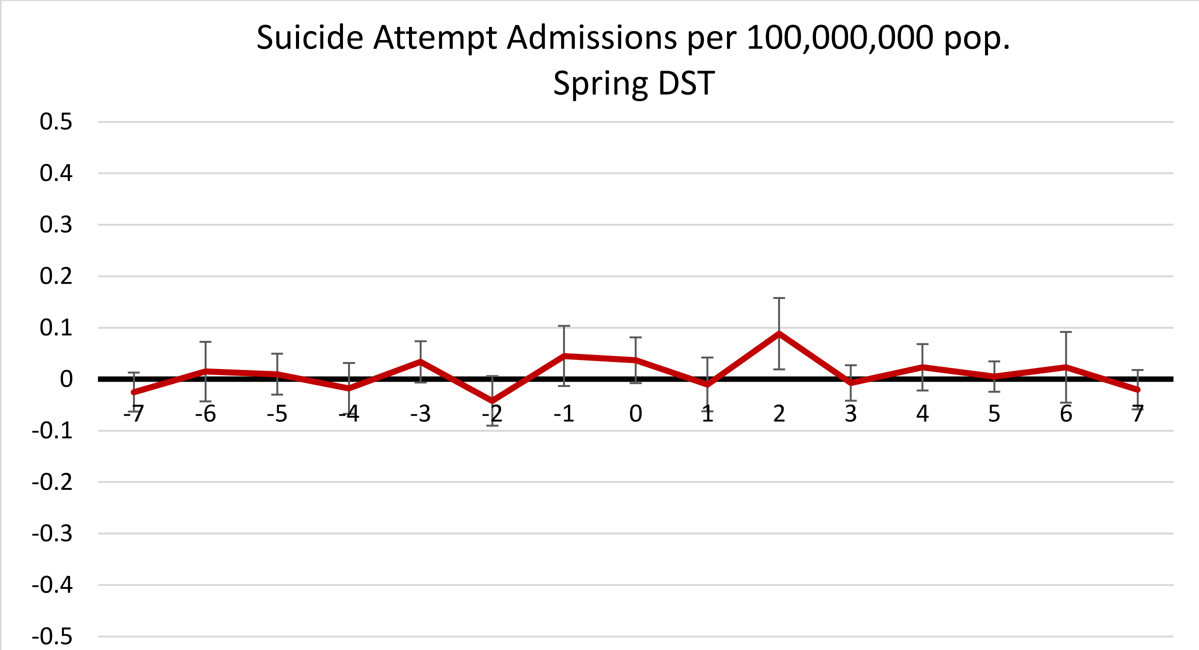


***Figure A7a and b, Hospital Census:***
**Daily Effects of DST on Suicide Attempts, 2000-2008**



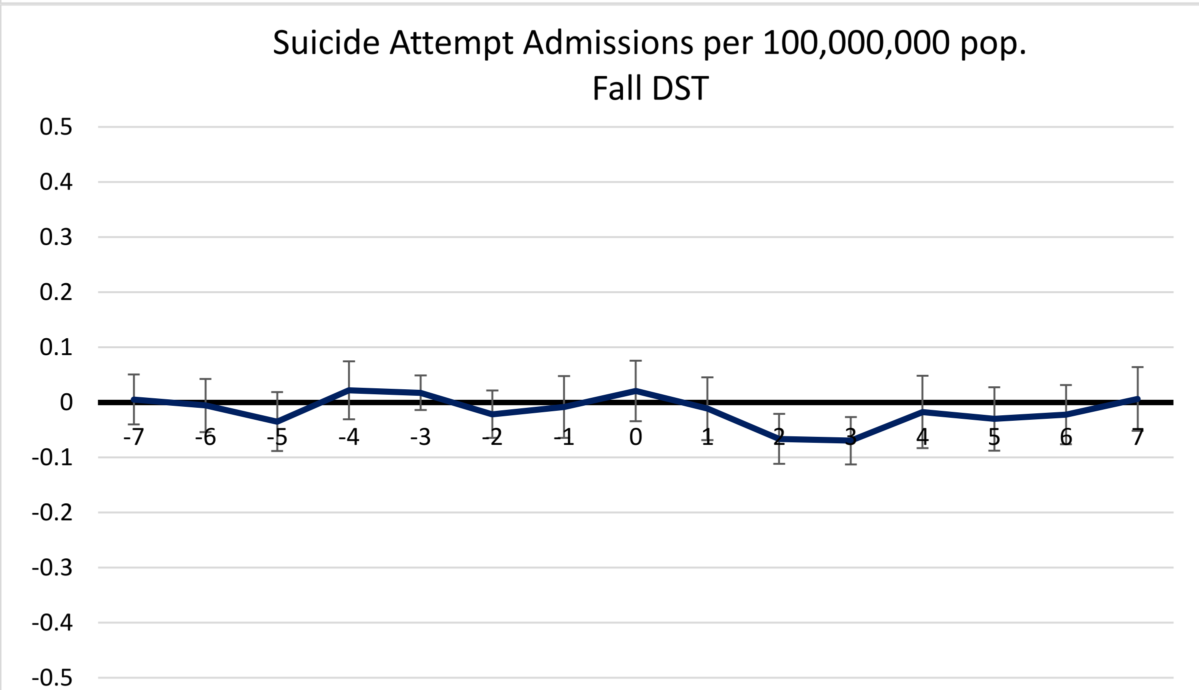
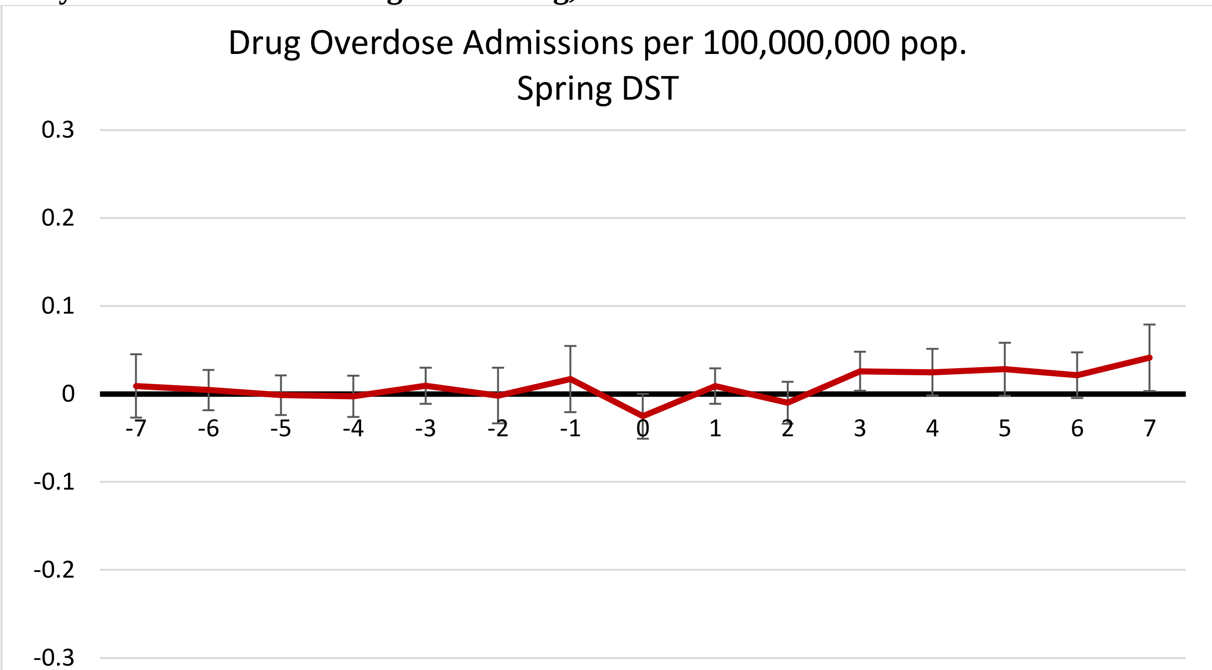



***Figure A8a and b, Hospital Census:***
**Daily Effects of DST on Drug Overdosing, 2000-2008**



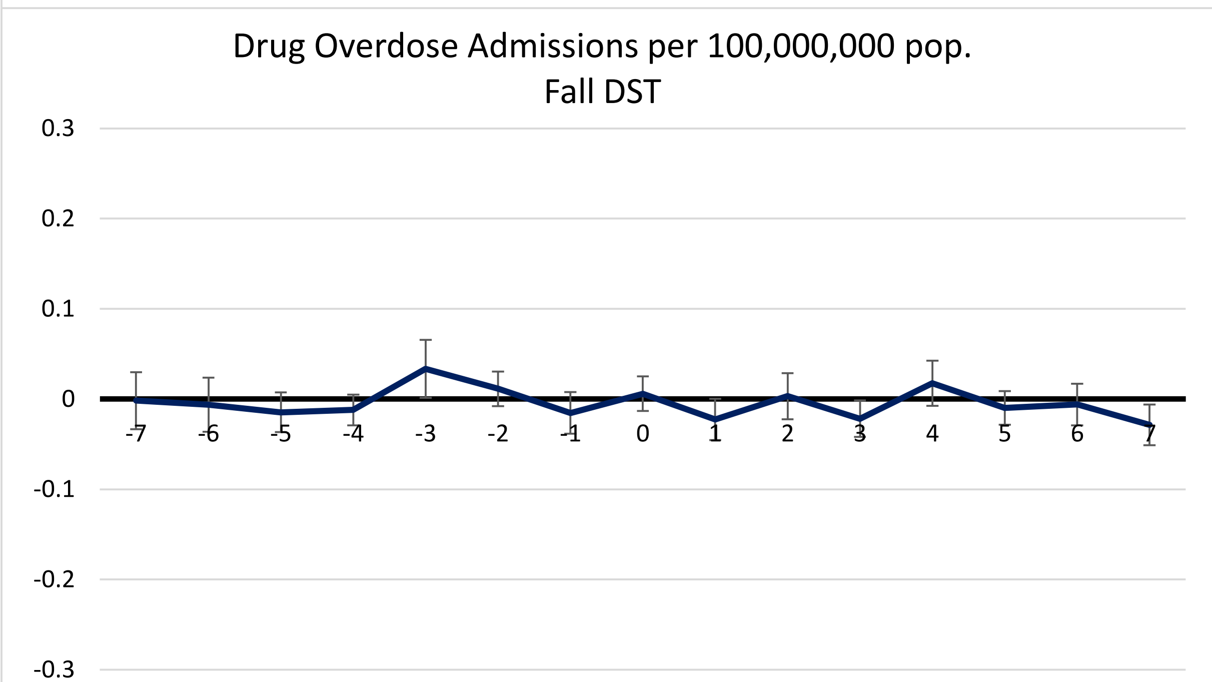



***Figure A9a and b, Hospital Census:***
**Daily Effects of DST on Infectious Admissions, 2000-2008**

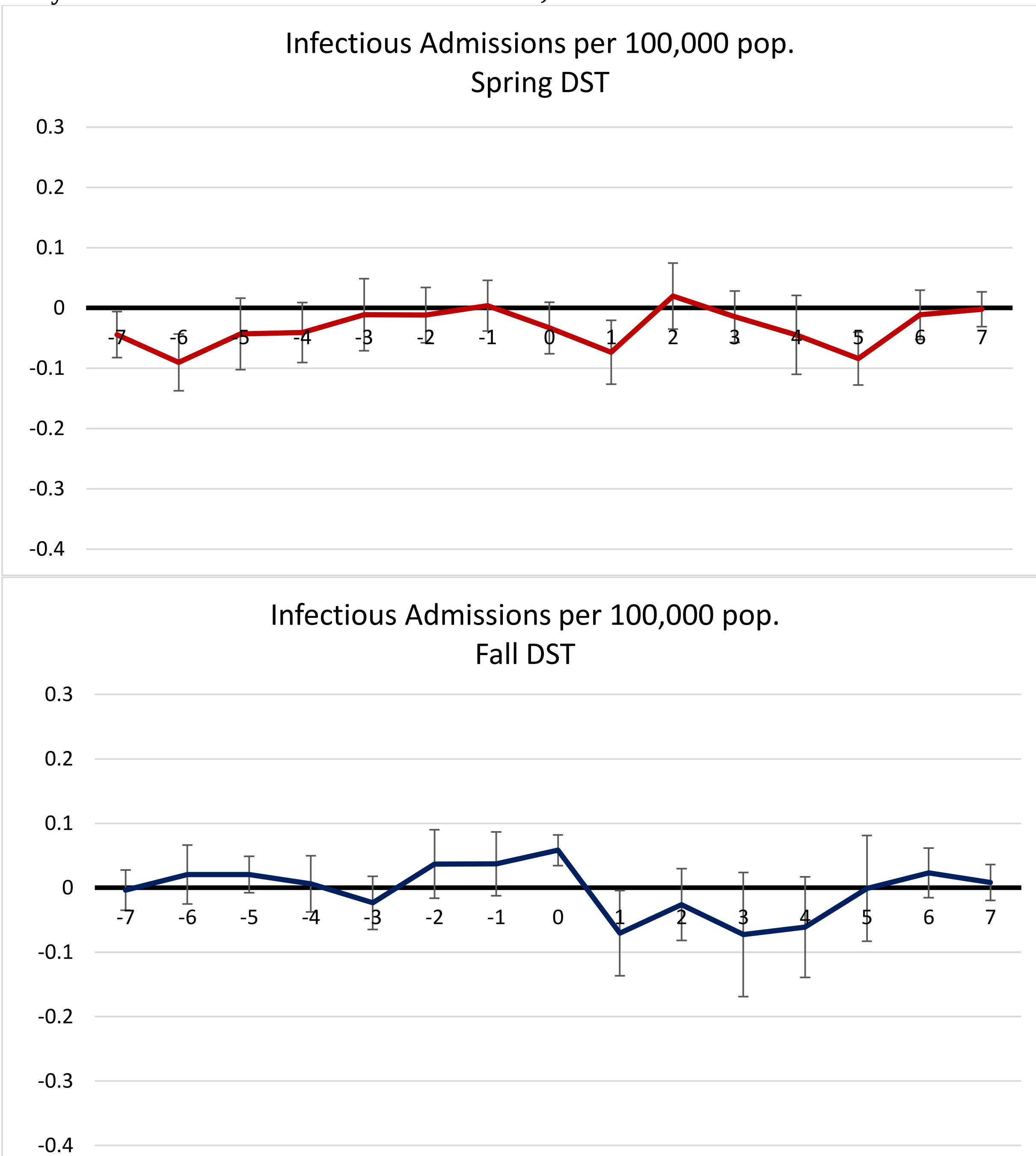


***Figure A10a and b, Hospital Census***

**Permutation Test of Summer (a) and Winter (b) Placebo Effects as Compared to DST Week, 2000-2008**

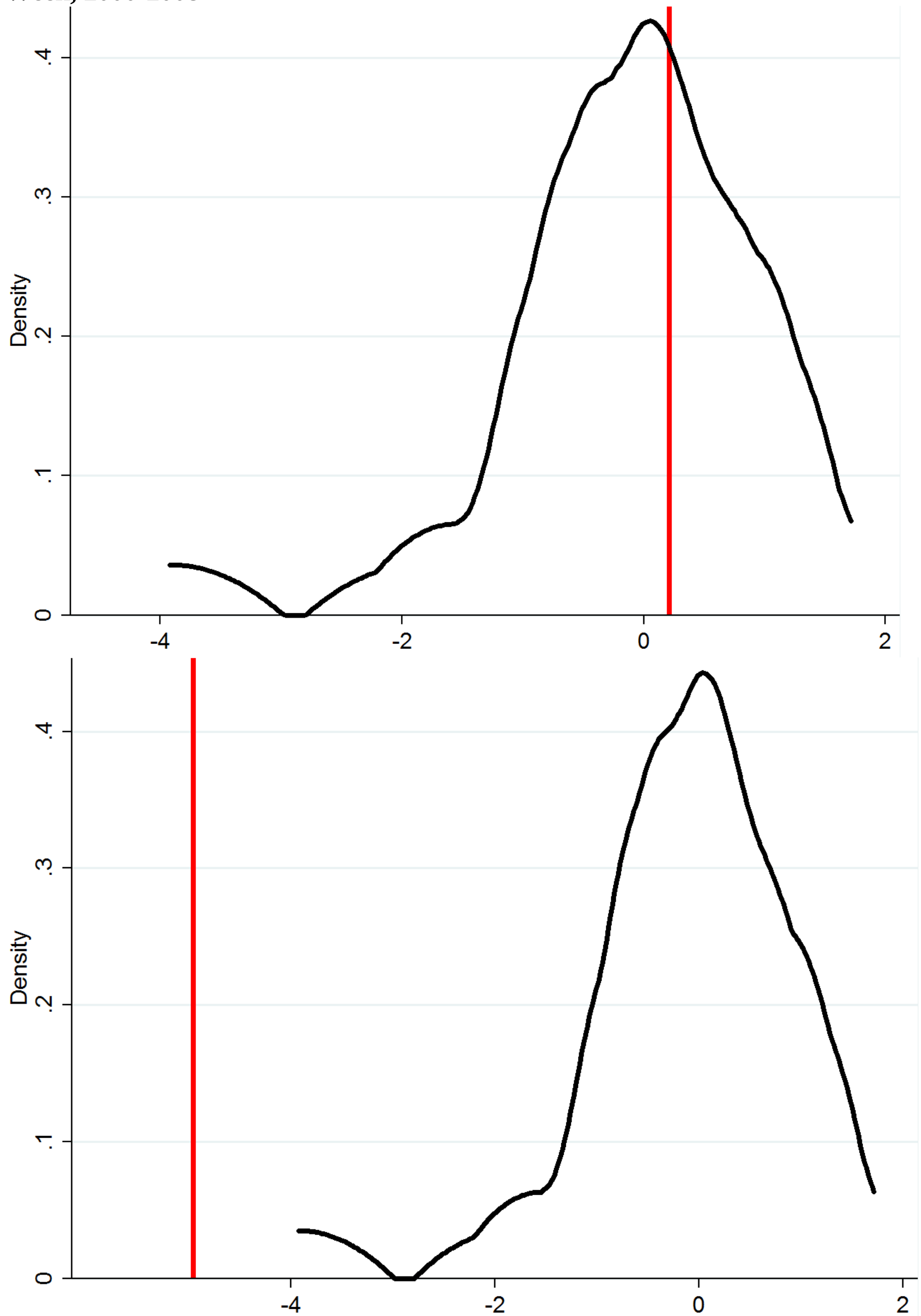


***Table A1:*** **German Hospital Census Descriptive Statistics**

| | ***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 |
| 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 |
| Infectious admission rate per 100,000 | 1.4069 | 1.1953 | 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 $PM_{10}$.

The point measures of the ambient weather and pollution stations are extrapolated 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 contain gender and sex information, official yearly county-level data are linked to these datasets. As shown in Appendix A, the empirical analysis relies on 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, Hospital Census:***
**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** | **Cloud** | ***CO*** | ***NO2*** | ***SO2*** | ***PM10*** |
| Begin DST * [column header] | 0.2569** | -0.1132** | 0.1819** | -0.2856* | 0.7647 | 0.0161 | 0.4457*** | 0.0803** |
| | (0.1182) | (0.0564) | (0.0878) | (0.1588) | (1.5761) | (0.02266) | (0.1606) | (0.0329) |
| 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) |
| Week of Begin DST | -1.7185** | 0.3247 | -0.8004 | 1.5348 | -0.2364 | 0.3581 | -1.6512*** | -2.1358*** |
| (2am → 3am in spring) | (0.6768) | (0.4739) | (0.5233) | (0.9807) | (0.6605) | (0.6581) | (0.5737) | (0.7684) |
| Week of 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 B: BRFSS

***Figure B1:*** **BRFSS Observations by Years**

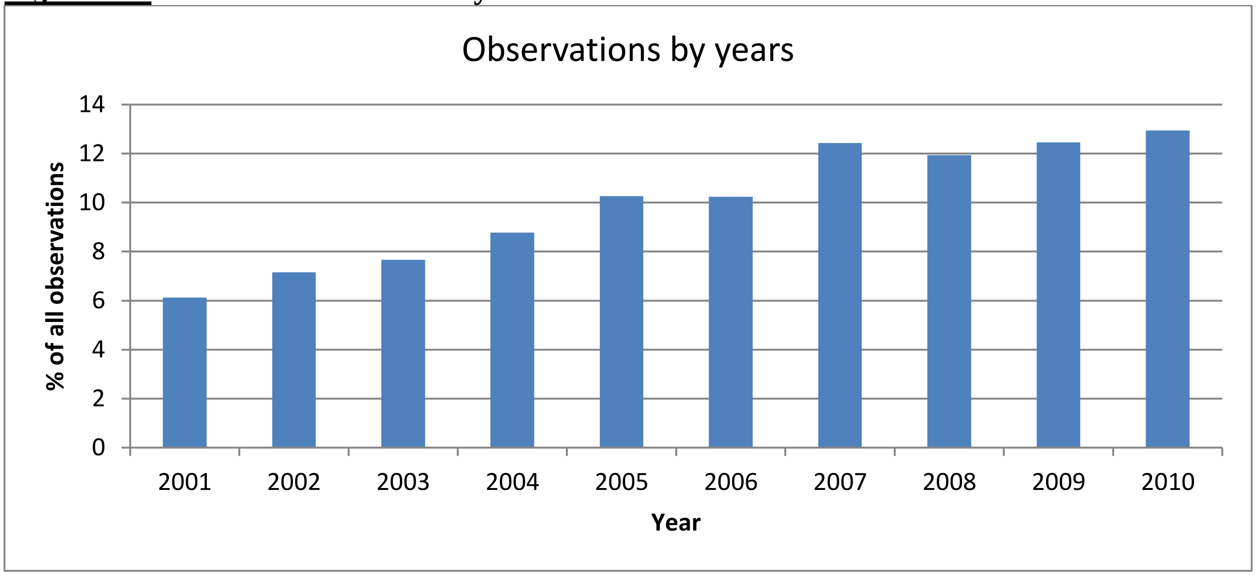



***Figure B2:*** **BRFSS Observations by Month-of-Year**

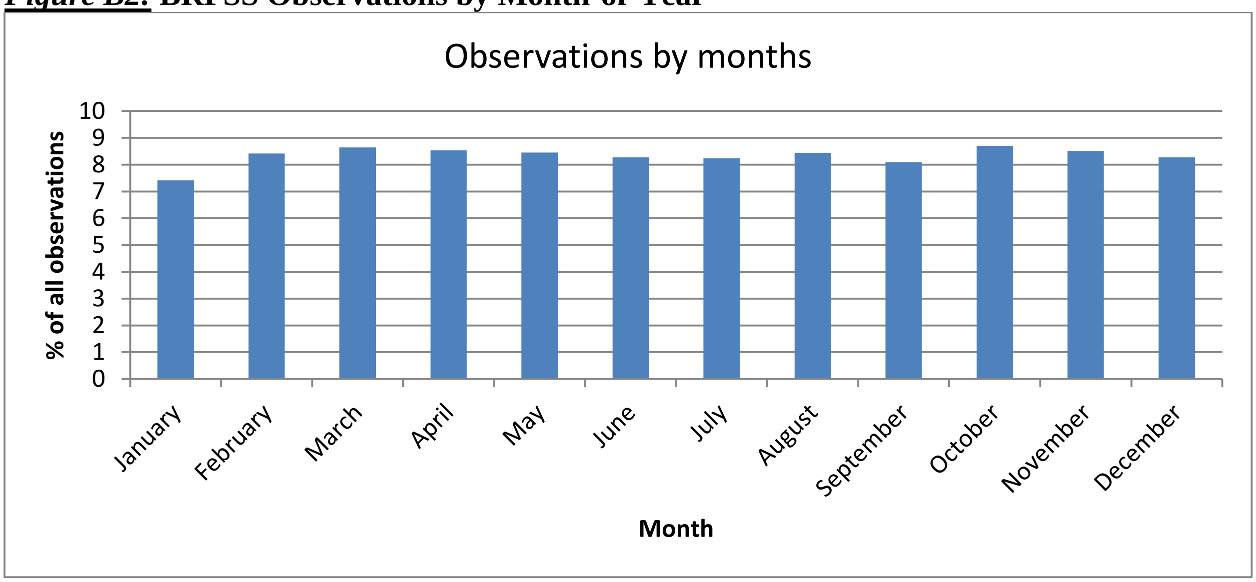
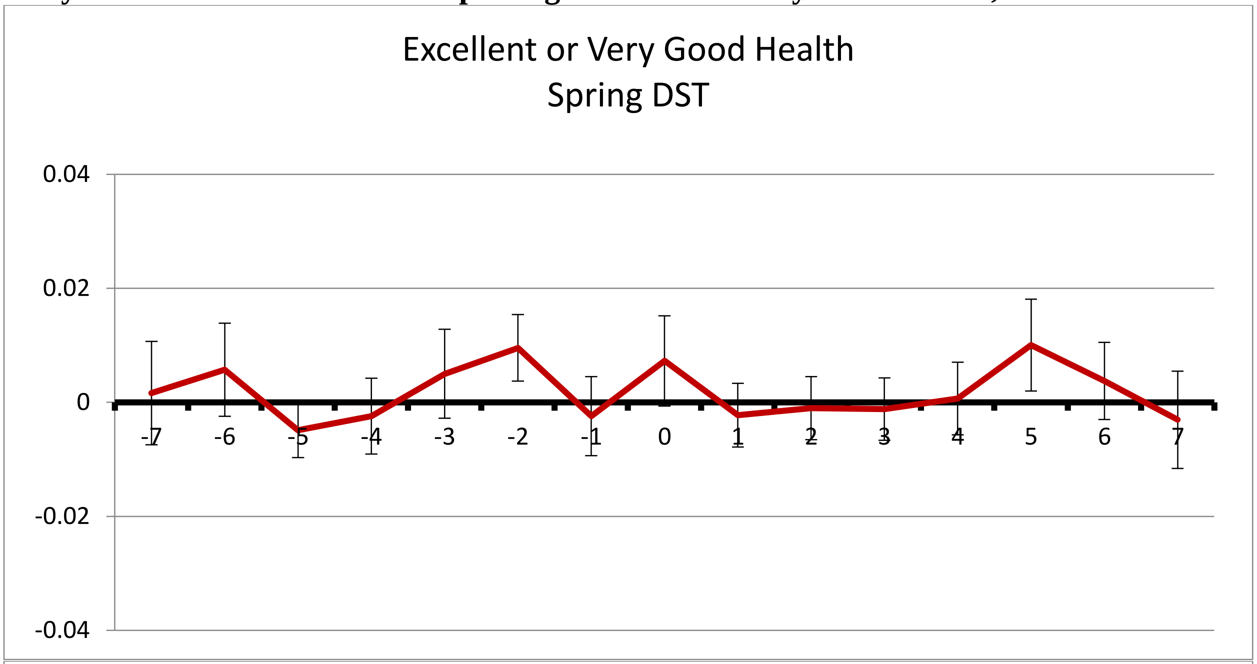


***Figure B3a and b, BRFSS Unweighted Full Sample:***
**Daily Effects of DST on Share Reporting Excellent or Very Good Health, 2001-2010**



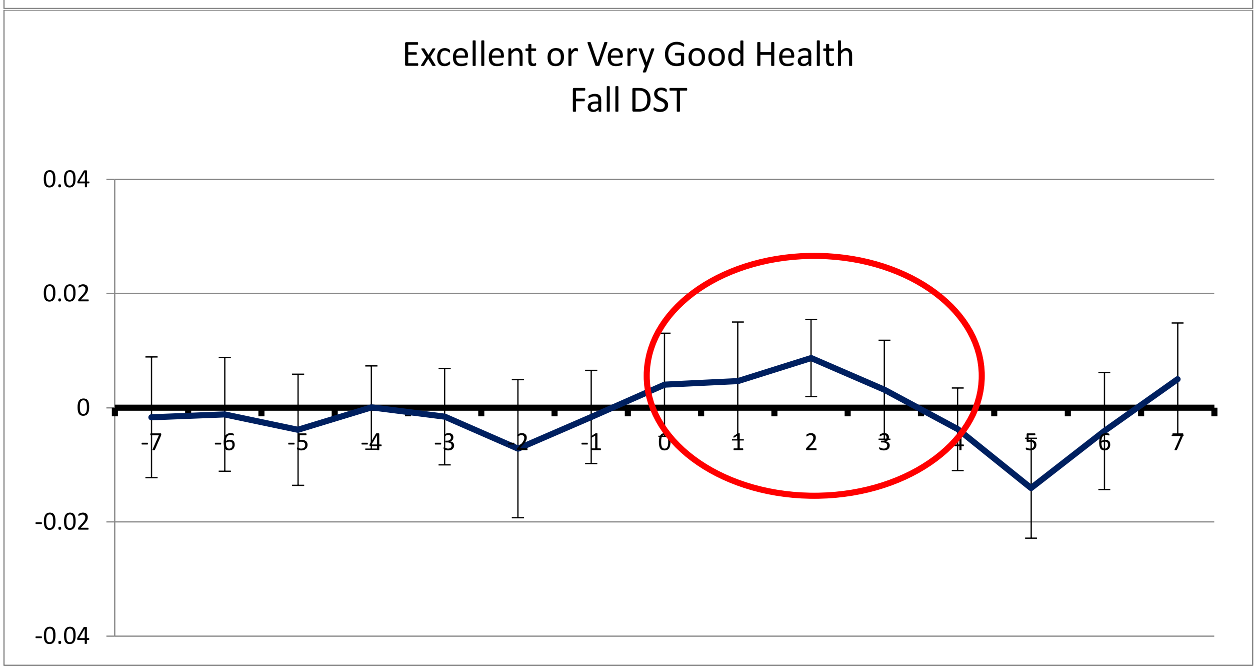



***Figure B4a and b, BRFSS Unweighted Full Sample:***
**Daily Effects of DST on Share of People Reporting Poor Health, 2001-2010**

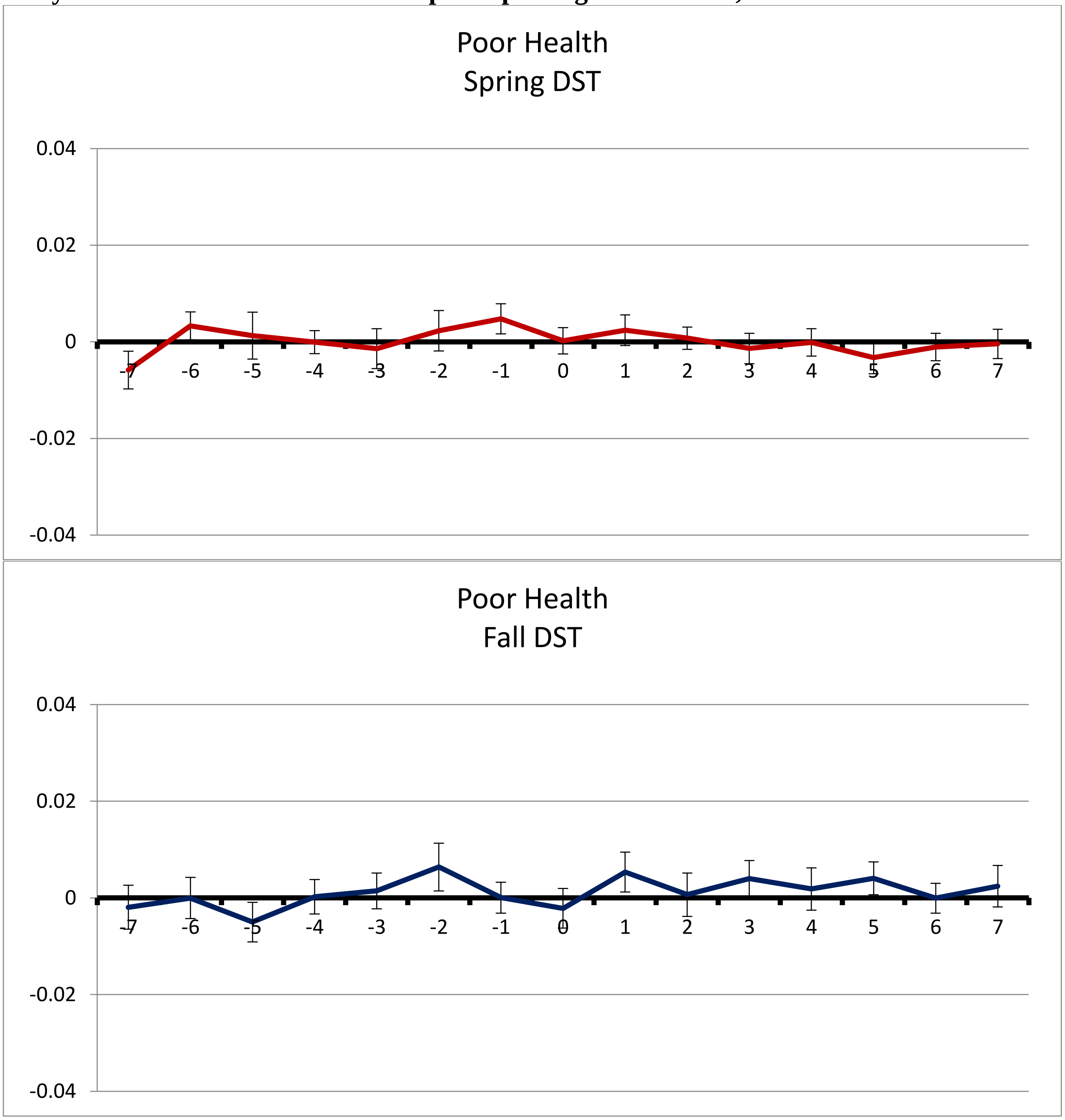
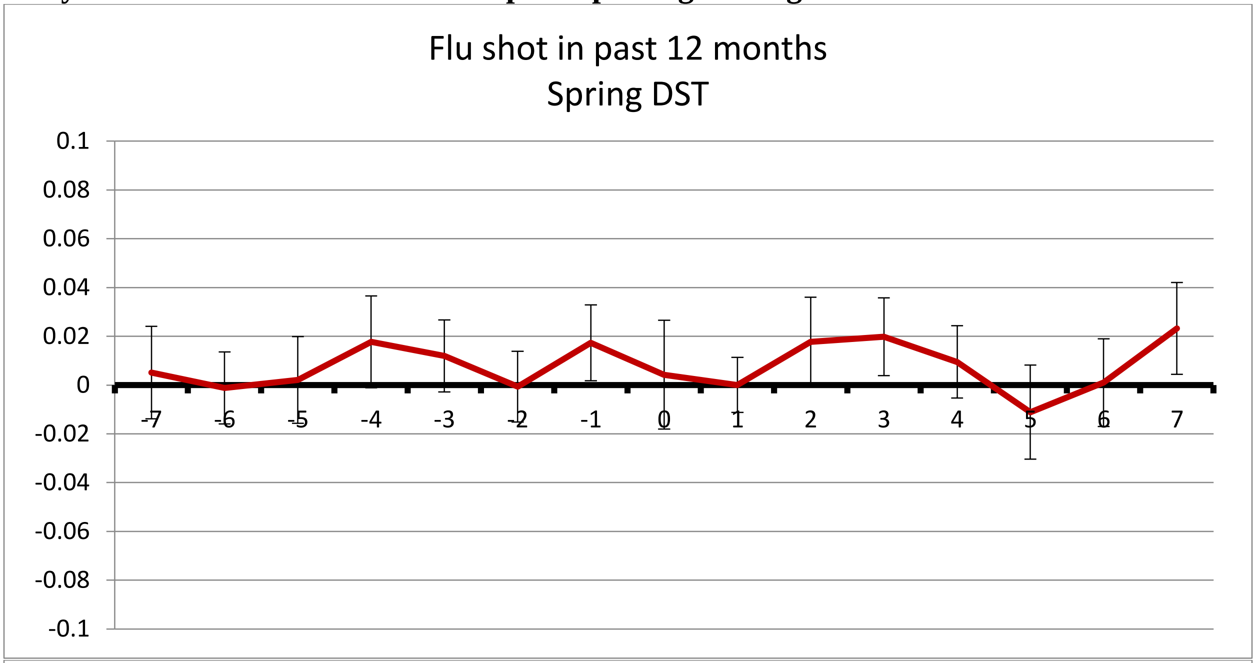


***Figure B5a and b, BRFSS Placebo Test:***
**Daily Effects of DST on Share of People Reporting Having Had Flu Shot in Past Year**



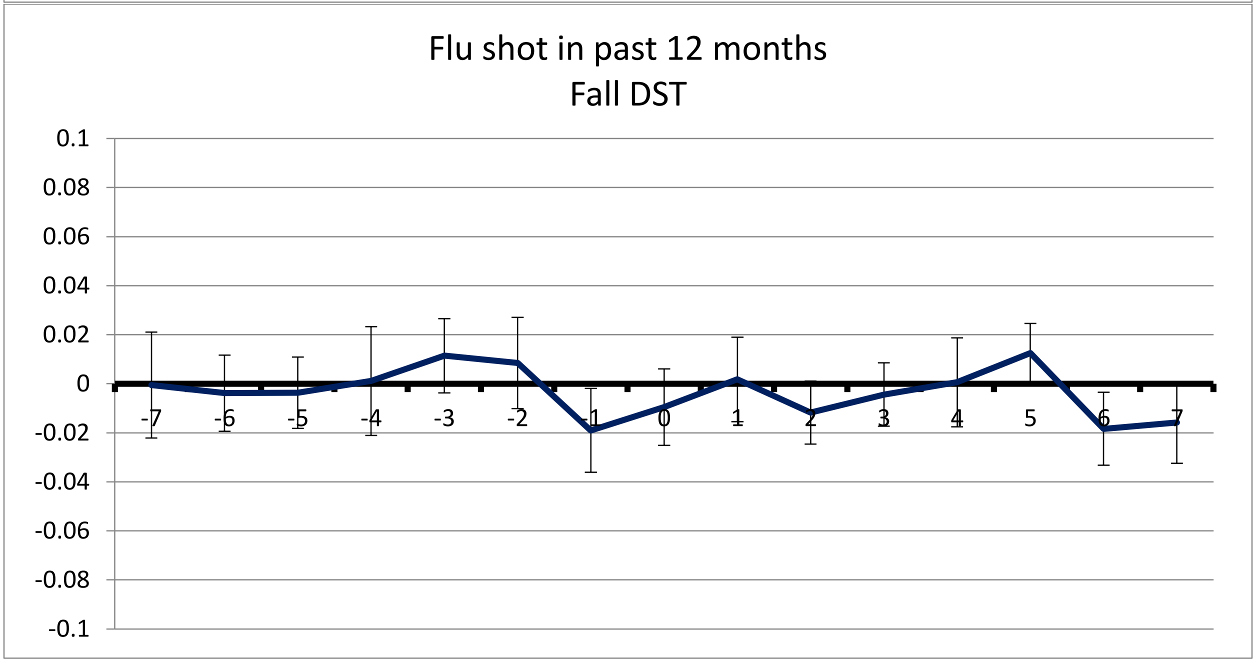
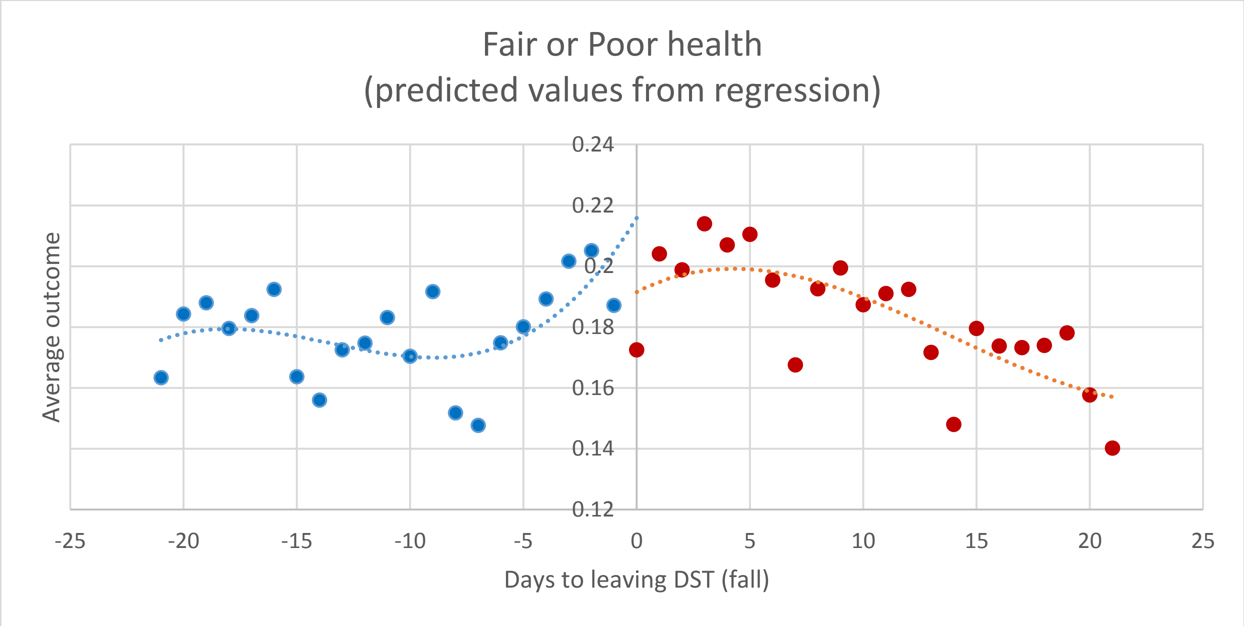



***Figure B6a and b, BRFSS Regression Discontinuity:***

**Daily Effects of Fall DST on People Reporting Fair/Poor and Poor Health**



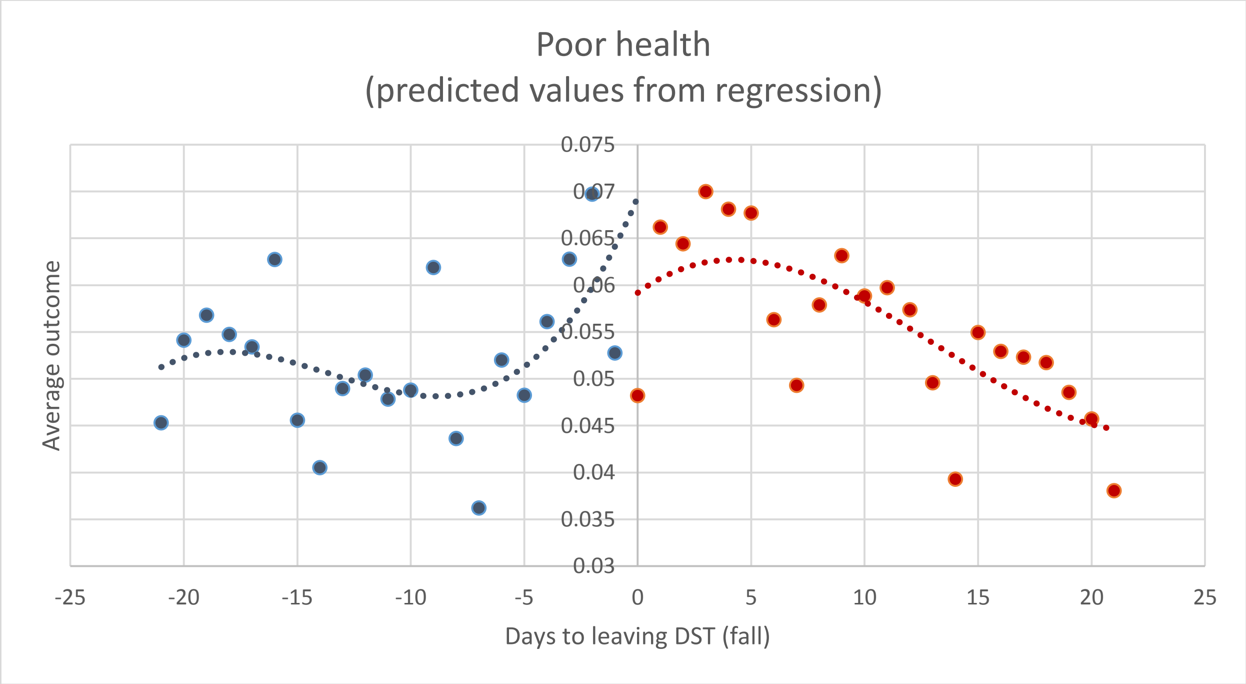
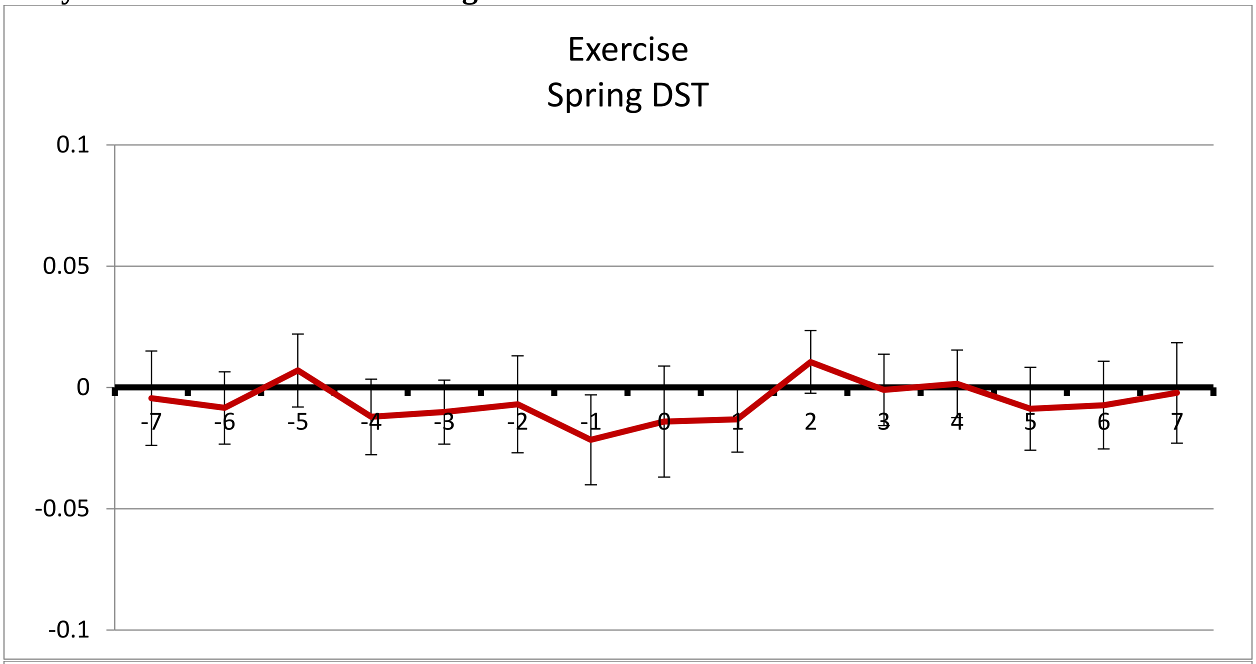



***Figure B7a and b, BRFSS:***
**Daily Effects of DST on Exercising**



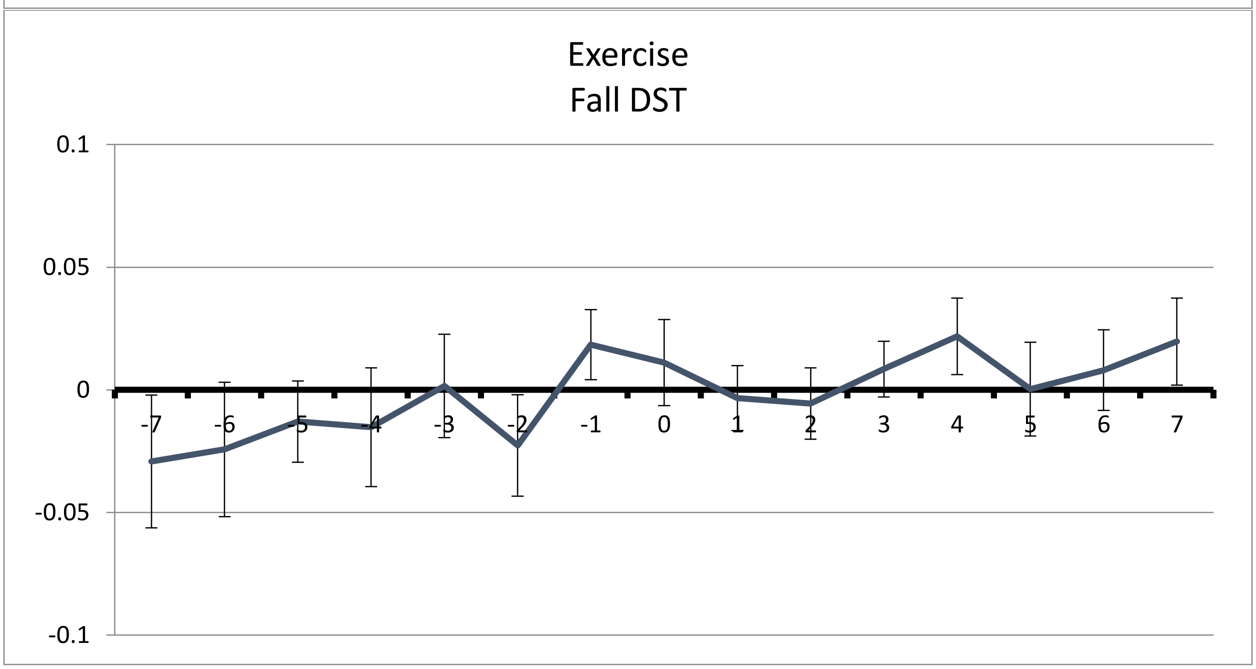



***Table B1:*** **BRFSS Descriptive Statistics**

| | *Mean* | *Std. Dev.* | *Min.* | *Max.* | *Obs.* |
|---|---|---|---|---|---|
| **Dependent Variables** | | | | | |
| General health | 2.532 | 1.106 | 1 | 5 | 799,171 |
| Excellent health | 0.193 | 0.395 | 0 | 1 | 799,171 |
| Fair or Poor health | 0.184 | 0.387 | 0 | 1 | 799,171 |
| Poor physical health | | | | | |
| # days in past 30 days | 4.119 | 8.553 | 0 | 30 | 743,686 |
| At least 1 day in past 30 days | 0.371 | 0.483 | 0 | 1 | 743,686 |
| Poor mental health | | | | | |
| # days in past 30 days | 3.396 | 7.685 | 0 | 30 | 743,686 |
| At least 1 day in past 30 days | 0.320 | 0.466 | 0 | 1 | 743,686 |
| Insufficient rest | | | | | |
| # days in past 30 days | 7.812 | 10.047 | 0 | 30 | 335,930 |
| At least 1 day in past 30 days | 0.638 | 0.481 | 0 | 1 | 335,930 |
| Hours of sleep in past 24 hours | 7.066 | 1.393 | 1 | 24 | 19,772 |
| Unintentionally fall asleep | | | | | |
| At least 1 day in past 30 days | 0.349 | 0.477 | 0 | 1 | 19,772 |
| Nodded off while driving | | | | | |
| At least 1 day in past 30 days | 0.028 | 0.164 | 0 | 1 | 19,772 |
| **Demographic Characteristics** | | | | | |
| Age | 52.049 | 17.444 | 7 | 99 | 799,171 |
| Female | 0.613 | 0.487 | 0 | 1 | 799,171 |
| White | 0.828 | 0.377 | 0 | 1 | 799,171 |
| African American | 0.087 | 0.282 | 0 | 1 | 799,171 |
| Married | 0.554 | 0.497 | 0 | 1 | 799,171 |
| Never married | 0.132 | 0.338 | 0 | 1 | 799,171 |
| Number of Children in Household | 0.633 | 1.082 | 0 | 24 | 799,171 |
| **Educational Characteristics** | | | | | |
| Lower Than Secondary Degree | 0.037 | 0.189 | 0 | 1 | 799,171 |
| Secondary Degree | 0.369 | 0.482 | 0 | 1 | 799,171 |
| Tertiary Degree | 0.592 | 0.492 | 0 | 1 | 799,171 |
| **Labor Market Characteristics** | | | | | |
| Employed for wages | 0.469 | 0.499 | 0 | 1 | 799,171 |
| Self-employed | 0.089 | 0.285 | 0 | 1 | 799,171 |
| Unemployed | 0.045 | 0.207 | 0 | 1 | 799,171 |
| Retired | 0.235 | 0.424 | 0 | 1 | 799,171 |

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

***Table B2:*** **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 B3:*** **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 | 2.570 | 2.521 | 0.031 |
| Excellent health | 0.186 | 0.195 | -0.016 |
| Fair or Poor health | 0.196 | 0.180 | 0.029 |
| Age | 53.500 | 51.649 | 0.075 |
| Female | 0.626 | 0.609 | 0.025 |
| White | 0.838 | 0.825 | 0.025 |
| African American | 0.082 | 0.088 | -0.015 |
| Married | 0.549 | 0.555 | -0.008 |
| Never married | 0.124 | 0.134 | -0.023 |
| Number of Children in Household | 0.594 | 0.643 | -0.032 |
| | | | |
| **Educational Characteristics** | | | |
| Lower Than Secondary Degree | 0.037 | 0.037 | 0.001 |
| Secondary Degree | 0.377 | 0.366 | 0.015 |
| Tertiary Degree | 0.583 | 0.594 | -0.015 |
| | | | |
| **Labor Market Characteristics** | | | |
| Employed for wages | 0.435 | 0.479 | -0.062 |
| Self-employed | 0.085 | 0.090 | -0.013 |
| Unemployed | 0.046 | 0.045 | 0.003 |
| Retired | 0.265 | 0.227 | 0.063 |
| ***N*** | ***172,638*** | ***626,533*** | - |

**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. $\sigma$ 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).