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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), up to 3.4 million BRFSS respondents from the US, and all 160 million hospital admissions from Germany over one decade. We find evidence of mild negative health effects when clocks are set forward one hour in spring. When clocks are set back one hour in fall, effectively extending sleep duration for the sleep deprived by one hour, sleep duration and self-reported health increase and hospital admissions decrease significantly for four days.

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

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 broadly defined as the stock of health, knowledge, ability, or personality. In addition to human capital life cycle models and their empirical applications (Cervelatti and Sunde, 2005; Michelacci and Ruffo, 2015, Low and Pistaferri, 2015), important studies test for the short and long-run health effects of pollution (Almond et al., 2009; Graff Zivin and Neidell, 2013; Currie et al., 2014; Isen et al., 2015), education (DiNardo and Pischke, 1997; Clark and Royer, 2013; Benhassine et al., 2015; Akbulut-Yuksel and Mutlu Yuksel, 2015; Bhuller et al., 2015), fertility (Avitabile et al., 2014), health behavior (Kenkel, 1991; Schultz, 2002; Cawley, 2015), adverse early childhood events or *in utero* conditions (Currie, 2009; Almond and Currie, 2011; Conti et el. 2012; Heckman et al. 2013; Nilsson, 2014; Yi et al. 2015), or human capital formation in general (Black et al., 2005; Oster et al., 2013; Cadena and Keys, 2015). The outcome variables vary from measures of birth outcomes, to specific diseases, health care utilization, labor market and social outcomes. Human capital determinants are likewise plentiful and include health behaviors, adverse environmental shocks, and education.

This paper contributes to the human capital literature by examining the role of sleep. It is one of the first papers in the economic literature that investigates how sleep may affect human capital. 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. Using data from 12 countries, Biddle and Hamermesh (1990) show that more labor market activities reduce hours of sleep, as do higher wage rates. Brochu et al. (2012) find that hours of sleep follow countercyclical pattern, which may explain why economic booms are negatively correlated with mortality (Ruhm, 2000). Piper (2015) finds that eight hours of sleep are correlated 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). Hamermesh (2008) exploits television schedules, time zones, and US time use data to demonstrate the relevance of time zones for the scheduling of market work and sleep. Giuntella and Mazzona (2015) also 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 (Valdez et al. 1996; Duffy et al. 2001, Duffy and De Gennaro, 2001; Moore et. al., 2002; Roenneberg et al., 2007; Knutson et al., 2007). The social and human capital effects of the sleep deprivation phenomenon are poorly understood due to a lack of studies that are able to exploit credible exogenous variation in sleep. While existing correlation studies outside of economics generally find a link between sleep deprivation and bad health, it is unclear to what degree this link represents a causal relationship (Pilcher and Huffcutt, 1996; Pilcher et al., 1997; Pilcher and Ott, 1998; Ferrara and De Gennaro, 2001; Ayas et al., 2003; Taheri et al., 2004; Mullington, et al., 2009; Haack et al., 2013).

The 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)" setting clocks forward one hour in spring and backward 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). Under DST, during the summer, people spend more time with natural daylight in the evening; this aligns with most people's preferences. The original DST rationale, however, 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.

The paper uses two datasets that complement each other in an ideal way to study the human capital effects of mild changes in sleep pattern due to DST: *(a)* The US Behavioral Risk Factor Surveillance System (BRFSS), and *(b)* The German Hospital Census, which provide empirical evidence from the biggest American and the biggest European country. In total, the representative databases cover the behavioral reactions of 400 million individuals in Europe and the US. Each database covers the entire year—not just spring or fall DST clock changes—over the first decade of the new millennium. Both representative

[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.

datasets carry a very large number of observations. This is crucial to control for important seasonal confounding factors while maintaining enough statistical power to identify health effects at the daily level. Because our estimates rely on both up to 3.4 million individual observations from the US (BRFSS), and up to 160 million observations from Germany (German Hospital Census), they are very precise. In addition, the two databases complement each other: While the BRFSS captures the entire population and contains a rich battery of self-reported health, satisfaction, and sleep measures that elicit mild(er) human capital effects of changes in sleep pattern, the Hospital Census captures severe health effects that require inpatient stays.

During the fall, as a result of DST, clocks are set forward by one hour from 2am to 3am. This time change effectively extends the sleep duration for sleep-deprived people by one hour. The sleep deprived "gain" one hour of sleep in the middle of the night. Evaluating the human capital effects of this exogenous extension of sleep duration, we find clear and consistent evidence that health improves in the short-run for four days when people on the margin get one more hour of sleep. We find surprisingly similar pattern in both the US BRFSS survey and the German administrative hospital data. 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. We also provide direct evidence that the share of people who report having unintentionally fallen asleep during the day decreases significantly between days 1 to 4 after the time change. Further, we find consistent and sharp decreases in hospital admissions across several disease categories between days 1 to 4 after the fall change.

The findings for spring DST are somewhat less clear but are nevertheless consistent; they are likely confounded by the plenitude of media reports every year around spring DST alerting vulnerable population subgroups of the increase in heart attacks and accidents due to DST "mini-jetlag."[2] We thus hypothesize that many people on the margin adjust their sleep behavior accordingly around spring DST. We do not find empirical evidence of a general increase in hospital admissions or a general decrease in

[2] These reports are mostly based on medical studies which use simple before-after comparisons to presumably identify causal adverse health effects and increases in traffic fatalities (Ferguson et al., 1995; Coren 1996a, Hicks et al., 1998; Sood and Ghosh, 2007; Lahti et al., 2010; Huang and Levinson, 2010; Alsousou at el., 2011), (workplace) injuries (Coren, 1996b; Barnes and Wagner, 2009; Lahti et al. 2011), cardiovascular diseases (Janszky and Ljung, 2008; Foerch et al., 2008; Janszky et al., 2012; Jiddou et al. 2013); disruption of sleep (Lahti et al., 2006; Kantermann et al. 2007), and even suicide rates (Berk et al. 2008). Smith (2015) provides credible causal evidence that fatal accidents increase following spring DST. Other welfare-relevant effects that the literature links to DST are decreases in stock market returns (Kramer et al., 2000; Pinegar, 2002; Kramer et al., 2002, Brahmana et al., 2012), decreases in SAT scores (Gaski and Sagarin, 2011) and increases in cyberloafing (Wagner et al., 2012).

self-reported health subsequent spring DST. However, we do find significant increases in self-reported poor health for certain population subgroups, such as males. We also find a significant short-term increase in the share of people reporting that they fell asleep during the day. Injuries and the prevalence of some other diseases significantly increase on the Tuesday following spring DST change. However, the effects are less strong and less distinct compared to the very clear and clean fall DST effects.

In summary, this study is one of the first to show that even mild changes to sleep patterns can affect human capital in significant ways. We demonstrate that more sleep may lead to significant, immediate, health improvements for people on the margin to getting hospitalized.

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

2. DATASETS

2.1 The US Behavioral Risk Factor Surveillance System (BRFSS)

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, and by 1996, all 51 states (including the District of Columbia) participated in the survey. It covers an extensive set of self-reported health measures and is, by design, representative of state populations. There are more than 3.4 million observations over the period 2001-2010. Our regressions routinely use sampling weights provided by the survey.

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. The reason for restricting the sample this way is to reduce seasonal effects that are difficult to control for entirely. However, we show that the findings are robust to this using the full sample. There are 799,171 observations in the restricted sample; Table A1 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

For our analysis, we focus on people's responses to the standard self-assessed health (SAH) question: "Would you say that in general your health is ___?" Table A2 shows the distribution of 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. The latter variable equals one if the individual responded with either "fair" or "poor" to the general health question, and zero otherwise. In our restricted sample, 19.3% responded "excellent" general health, and 18.4% responded with either "fair" or "poor" general health.

In extended model specifications, we exploit six additional measures that capture self-reported *(i)* poor physical health, *(ii)* poor mental health, and *(iii)* insufficient rest. In addition, refined models use three sleep-related questions as outcome variables which have been surveyed in up to nine US states since 2009.

Measuring Daylight Saving Time in the US

In the US, as of 2015, daylight saving time 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 1 shows the various dates of daylight saving time 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.

Daylight saving time 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.

2.2 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* (*Statistische Ämter des Bundes und* *der Länder*) provides restricted data access for researchers.

Germany counts about 82 million inhabitants and registers the total of about 17 million hospital admission per year.[3] To obtain the working dataset, we aggregate the individual-level data on the daily county level and then normalize admissions per 100,000 population.

As seen in Appendix B, 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 mentioned and discussed in more detail in the next section, for the main models, we restrict the sample to a total of six weeks around the time change in spring and fall. However, in robustness checks we make use of the entire 52 weeks of the year and show that the findings are robust to this sample restriction. 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]

Construction of Main Dependent Variables

Using the information on the primary diagnosis, we generate the following dependent variables: *(a)* The *All cause admission rate* by aggregating over the total numbers of admissions on a given day in a given county and normalizing per 100,000 population. On a given day we observe 59.77 hospital admissions per 100,000 population (see Appendix Table B1).[5] However, the rate varies substantially at the daily county level and the standard deviation is 25.73. Note that the county refers to the county of residence of the patient—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

[3] This 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.

admissions (Table B1).

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

(d) Finally, we 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 2 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.

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.

3. EMPIRICAL SPECIFICATION

3.1 First Empirical Model: Extracting 15 Daily Effects ("Daily Approach")

The first empirical specification estimates the following model by OLS:[6]

$$y_{ct} = ß_0 + ß_1 BeginDST_{ct} + ß_2 EndDST_{ct} + X_{ct}'\,\gamma + Easter_{ct} + Vacation_{ct}$$

$$+ \phi_m \delta_y + DOW_t \phi_m + t + t^2 + \mu_c + \varepsilon_{ct} \tag{1}$$

Where y_{ct} stands for one BRFSS (German Hospital Census) health outcome variable for individual (county) *c* on day *t*. $BeginDST_{ct}$ is a vector of 15 daily binary variables *-7,…., -1, 0, 1,….,7* indicating the seven days leading up to spring DST, the day of DST, and seven days following it. The variable $EndDST_{ct}$ is similarly constructed with daily dummies for the two weeks around fall DST.

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

For the German Census data, X_{ct} includes a series of county-specific control variables that may vary at the daily level, such as the share of admitted people in each age group, their gender, and the share of hospitals that are private. In addition, X_{ct} incorporates a series of county control variables that vary at the annual level, such as the unemployment rate or GDP per capita (see Appendix B). For the BRFSS, X_{ct} measures a set of socio-demographic variables including age, sex, race, marital status, level of education, and employment status (see Appendix A).

Next, $Easter_{ct}$ represents a vector of four Easter dummies indicating *Holy Thursday*, *Good Friday*, *Easter Sunday* and *Easter Monday*. For example, in 2005 and 2001, spring DST fell on Easter Sunday in Germany, and in 2008, the Sunday before spring DST was Easter Sunday. In Germany, Good Friday and Easter Monday are national holidays and admissions sharply drop on such days, which is why it is important to consider these religious holidays.

$Vacation_{ct}$ indicates those days around spring and fall DST that are official school vacation days in Germany or the US. 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 also vary in length from one up to three weeks, depending on the state. German 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.[7]

DOW_t carries six binary variables netting out persistent day-of-week effects in hospital admissions or subjective health assessments. For example, relative to Sundays, hospital admissions increase strongly by 52 per 100,000 pop. (mean: 59.57) on Mondays. Over the rest of the week, the relative increase decreases almost linearly to 37 admissions on Wednesdays and 12 admissions (per 100,000 population) on Fridays. Self-reported health also varies significantly by day-of-week; for example, 20.8% of survey respondents report “excellent health” on a Sunday, significantly higher than the 19.0% on Mondays. Because it is imaginable that day-of-week health effects differ by season, we include a set of $DOW_t\phi_m$ interaction terms between the six day-of-week dummies and the 11 monthly dummies ϕ_m. We also include month-year fixed effects $\phi_m\delta_y$, plus a linear and quadratic time trend $t + t^2$.

[7] Halloween is only a very recent phenomenon in Germany and has no tradition. However, the German findings are robust to including Halloween fixed effects.

Lastly, when we use the Hospital Census we consider county fixed effects μ_c, and when we use the BRFSS we consider state fixed effects. Because it is not likely that the county-day hospital admission rates are either independent over time or across space, we also correct the standard errors ε_{ct} by applying two-way clustering across counties and over time (Cameron et al., 2011). When using the independently drawn and representative observations of the BRFSS, we cluster standard errors at the date level. All BRFSS regressions are probability weighted.

3.2 Second Empirical Model: Assessing the Aggregated Week Effects ("Weekly Approach")

The second empirical specification estimates an almost identical model:

$$y_{ct} = ß_0 + ß_1 WeekBeginDST_{ct} + ß_2 WeekEndDST_{ct} + X_{ct}'\gamma + Easter_{ct} + Vacation_{ct}$$

$$+ \phi_m \delta_y + DOW_t \phi_m + t + t^2 + \mu_c + \varepsilon_{ct} \qquad (2)$$

However, instead of netting out systematic differences on a daily basis in the week before and after the DST change, we now employ $WeekBeginDST_{ct}$ and $WeekEndDST_{ct}$ as the main regressors of interest. $WeekBeginDST_{ct}$ and $WeekEndDST_{ct}$ are indicators for any of the seven days following the clock change, starting with the DST Sunday and ending on the next Saturday. This is basically a model where we aggregate effects at the weekly level and estimate the net health effects of DST over the entire following week.

3.3 Identification

The two empirical models above make use of a very saturated specification that is only estimable thanks to our rich databases while still yielding remarkably precise estimates at the daily level. This 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—clocks are typically changed on Sundays while is it a stylized fact that hospital admissions spike on Mondays (cf. Witte et al., 2005), or *(iv)* general seasonal effects as well as specific seasonal effects such as Easter Sunday or vacation day effects. In the refined effect specifications that intend to test for behavioral adaptions, we *(v)* stratify the results by individual-level socio-demographics as well as ambient climatic conditions such as temperatures, hours of sunshine, and pollution.

The key idea is, however, that the running variable represents time and the treatment is represented by the specific DST dates. Time is arguably exogenous to individuals because humans cannot influence time. This can be thought of as a variant of an RD approach with time as the running variable. However, the specification is even richer because we do not just rely on the days before and after the treatment (DST), and compare the health of individuals before and after DST changes. In our main model of choice, in addition to the week before and after the DST change, we rely on four additional weeks as control groups: two more weeks before the two DST weeks of interest and two more weeks after the two DST weeks of interest. This means that we extract 6 weeks around spring DST as well as 6 weeks around fall DST to estimate our main models. Using the BRFSS, we do this for 10 years and exploit 10*6*6=360 weeks or 2,520 days. The sample is corrected for the battery of socio-demographic and economic covariates discussed, in addition to county or state fixed effects, month-year as well as DOW-month fixed effects. Moreover, in robustness checks, we show that the results are robust to including all 52 weeks of the year, not just the 2*6 weeks around DST. Figure 1 illustrates the sample selection for the main model.

[Insert Figure 1 about here]

Table A3 in Appendix A 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. Figures A1 and A2 likewise reinforce this point. Figure A2 shows that the BRFSS is very balanced in terms of sample sizes over the 12 calendar months of a year, and Figure A1 only shows a slight overall increase in the sample size of the ten years under consideration. The latter observation basically just means that the behavioral reactions in more recent years get attached slightly larger weights, which is no threat to our identification strategy. Recall that the length of the summer time was extended in 2007 (Table 1), which is when the BRFSS reached its current steady state sample size.

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).

Do the Dependent Variables Measure True Population Health Effects?

One may wonder whether our health outcome variables measure true population health effects. In general, we would like to emphasize that both types of health measures—self-reported health in surveys as well as hospital admissions—are routinely used by health economists as their main health 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, with respect to self-reported BRFSS health measures: a rich health economics literature investigates reporting heterogeneity (or systematic reporting biases) in the standard self-assessed health (SAH) measure. One can summarize this literature as: *(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 general SAH question are systematically biased with respect to health and gender, whereas there is less evidence that this holds for other socio-demographics (Ziebarth, 2010). For example, this means that older people tend to judge their health more mildly relative to younger ones on this absolute scale. People seem to refer to an age-gender dependent reference group when answering the question. Cross-country reporting biases due to language are another issue discussed in the literature (Jürges, 2007). *(c)* Because we control for a rich set of socio-demographics and only rely on within country variations, age-gender dependent reporting heterogeneity should not be a threat to our estimates. *(d)* There is no reason to believe that the age or gender dependent bias is correlated with DST. *(e)* As shown in Table A3, the respondents' socio-demographics are very balanced in the weeks before and after the DST change.

Second, with respect to the German Hospital Admission Census: German geography, combined with the institutional setting of the German health care system, makes it very reasonable to assume that variations in hospitalizations represent variations in 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 as high as 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, 2014). 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). Thus, insurance or cost-sharing barriers to hospital access are also very low in Germany. Overall it is very reasonable to assume that the universe of hospital admissions indicates severe health shocks very comprehensively in Germany.

4. RESULTS

4.1 Evidence from the BRFSS and the US: 2001-2010

Figures 2 and 3 plot the 15 daily dummy variables around spring and fall DST from the regression model in equation (1). Figure 1 focuses on the share of respondents who self-report that they are in excellent health, and Figure 2 shows the share of respondents reporting fair or poor health. All point estimates are plotted along with 90% confidence intervals.

[Insert Figures 2 and 3 about here]

Let us first focus on the human capital effects of the fall DST, as measured by self-reported health. Recall that clocks are set back by one hour from 2am to 1am on the first Sunday of November in the US (and from 3am to 2am on the last Sunday of October in Germany, Tables 1 and 2). This time change effectively extends the sleep time by one hour for the sleep deprived who would not automatically wake up after X hours of sleep. The exogenous time change extends sleep time by up to one hour and one would not expect behavioral adaptations as a reaction to this fall time change. Also note that the night sleep extension could last for more than one day under the assumption that people do not push back their bedtime by one hour immediately in the following days (because they get still tired at their usual summer time bedtime).

Figures 2b and 3b plot the 15 daily coefficients of equation (1) around fall DST for the years 2001 to 2010. The dependent variables measure the share of Americans in self-reported excellent and fair/poor health. Figure 3b yields no evidence that the share of respondents in bad health decreases by more than +/- 2ppt. However, Figure 2b provides evidence that after "gaining" one hour of night sleep, the share of Americans who report "excellent health" increases by a statistically significant 1ppt on Monday. This effect persists until Thursday before it dissipates. The size of the probability-weighted coefficient is relatively small, but would translate into about 2.5 million marginal Americans who would report excellent instead of very good health for four days.

The finding of a shift of the right tail of the subjective health distribution is reinforced by Figures A3b and 4b in Appendix A. Figure A3b shows the same health improvement pattern as above when the underlying model makes use of the full sample—all 52 weeks of the year instead of the 2*6 weeks in the main specification—and does not weight the regressions. As seen, the increase in health in Figure A3b 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). We find that the share of respondents in very good or excellent health increases by up to 1ppt from 51 to 52% but there is no evidence that the share of respondents in poor health changes post fall DST (detailed results available upon request).

Figure 4b identifies a surprisingly similar four day decrease in the share of Americans who report that they unintentionally fell asleep during the day. Hence Figure 4b strongly reinforces that a nighttime sleep extension is the driving mechanism of the subjective health improvement. Column (1) of Table 3 underlines this mechanism by showing that (self-reported) hours of sleep increase significantly in the week following fall DST (more discussion below).

[Insert Figure 4 about here]

The spring DST effects as illustrated in Figures 2a and 3a are not as distinct as the fall effects. If the effects were symmetric, one would expect to see decreases in self-reported health because the night time sleep during would be shortened by one hour. Figure A5a does yield evidence that the share of Americans who unintentionally fall asleep during the day significantly increases by up to 1ppt on the Monday and Tuesday after the spring DST change. However, there is no systematic decrease in self-assessed health in Figures 2a, 3a, or 4a. Note that the standard errors are very tight, and that we employ a very rich fixed effects specification. For example, relative to the baseline share of Americans in excellent health (19%, Table A2), with 95% statistical certainty, this share does not decrease by more than 1ppt after the clocks are set forth by one hour in spring. Likewise, we can exclude with great statistical certainty that the share of Americans in fair or poor self-assessed health increases by more than 1.6ppt from a baseline of 18% (Figure 3a and Table A1). As we discuss in more detail below, our main explanations for the less distinct and smaller health effects after spring DST refer to the biological rhythm and behavioral bed time adjustments of marginal people. As a result of the broad media exposure and alerts for vulnerable people around spring DST, we hypothesize that many of those sensitive humans pay extra attention and go to bed earlier.

All effects just discussed persist in the upcoming specifications, robustness checks, and using the universe of hospital admissions from Germany. One finds *(i)* distinct and characteristic health improvements for four days after people "gain" one hour of sleep time in fall, and *(ii)* less distinct and very small health decreases on the Monday and Tuesday after the clocks are set back by one hour in spring.

Alternative Outcome Measures: Physical and Mental Health, Rest, and Sleep

Table A4 in the Appendix makes use of alternative outcome measures, the model in equation (2), and explores whether the self-reported number of days with *(a)* poor physical health, *(b)* poor mental health, or *(c)* insufficient rest change after the clock changes in spring and fall.

Note that the survey questions for the outcome measures refer to the last 30 days, which may introduce measurement error and a non-straightforward interpretation. First of all, 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 poor health, those interviewed on Monday X+/-2 instead of X days, and so on. Because our standard approach assigns respondents in weeks t+2 and t+3 the control group status (Figure 1), our estimates would be biased downwards because their retrospective 30-day responses would be affected by DST as well. Therefore, in Table A4, we assign respondents interviewed in weeks t+2 and t+3 to the treatment group. In practice, we expect recall biases and that respondents weigh days closer to the interview day much stronger. We also expect attenuation biases due to the measurement errors.

The six *Week of Begin DST* and the six *End of DST* coefficients are all small in size and not statistically significant. For example, the coefficient estimate for *# days in past 30 days w/ poor mental health* in column (3) has a size of 0.7% of the mean, and we can exclude with 95% statistical certainty that spring DST increases the number of days in bad mental health by more than 0.15.

However, for *# days in past 30 days w/ insufficient rest* we find evidence for significant effects, both in spring and fall. The coefficient estimates are relatively large in spring, 2.5% of the mean, and also marginally statistically significant when we do not weight the regressions.

[Insert Table 3 about here]

The latter findings is further reinforced in Table 3 where we use three BRFSS sleep measures as outcomes and estimate equation (2). In 2009, six states began to include questions about sleep inadequacy in the BRFSS, and this expanded to nine states in 2010.[8] Column (1) uses *#Hours of Sleep*. While the *Week of Begin DST* estimate is small and insignificant, the *End of DST* estimate has a size of 1.4% of the mean and is statistically significant at the 10% level. In line with our explanation and discussion above, this shows that a small group of probably sleep deprived Americans gets more sleep when one hour is "gained" during the night.

Figure 4 and column (2) of Table 3 underscore these findings. The binary outcome *Unintentionally falling asleep* is one if this had happened at least once in the past 30 days. While the spring DST estimate is positive and carries no significance, the fall DST estimate is large in size (13%) and statistically significant at the 5% level, suggesting that the share if Americans who unintentionally fall asleep during the day decreases by more than 4ppt in the week after fall DST change.

Figure 4 documents this effect graphically using our daily approach and the plotted coefficients of equation (1). Despite some fluctuations due to the smaller sample size in this model, the four day effect following fall DST that we already observed in Figures 2b and A3b is easy to depict.

4.2 Evidence from Administrative Hospitalization Data and Germany: 2000-2008

Next, we use the same empirical approaches as above to study whether hospital admissions vary significantly in the days following DST and mild changes in sleeping pattern. As discussed in Section 3.3, it is very likely that the variation in hospital admissions reflects true variation in underlying health shocks. The reasons are that *(i)* Germany has a very high population and hospital density. The distances to the next hospital are typically short and geographic access barriers low. *(ii)* Insurance access barriers are also low due to universal coverage, free choice of hospitals and small cost-sharing amounts.

Figures 5 and 6 summarize the findings from the daily approach and plot the 15 coefficient estimates of equation (1). Figure 5 plots the impact on total admissions per 100,000 population (mean 59.8) and Figure 6 the impact on cardiovascular admissions per 100,000 population (mean 9.5). Despite

[8] 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.

conservative two-way clustering on the date and county level, the universe of all hospital admissions allow us to assess daily effects in a remarkably precise manner.

[Insert Figures 5 and 6 about here]

Recall that the effects are identified by more than 3,916 fall DST county-day observations between 2000 and 2008 and different calendar dates, which makes it extremely unlikely that systematic supply-side shocks produce these pattern. Also recall that these daily estimates are not nonparametric plots but stem from the rich fixed effects specification in equation (1). The daily effects plotted are net of vacation day and month-day-of-week fixed effects as well as Easter effects. Moreover, the coefficients compare the relative admission rates for the 15 days plotted to the admission rates in the two weeks before and after.

Figures 5b and 6b show an unambiguous clear pattern that resembles the BRFSS survey data pattern for fall DST (see Figures 2b, 4b, A3b): We observe a strong and distinct decreas*e* 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 then decreases smoothly over the next three days before they disappear on day five. The decrease for cardiovascular admissions equals about 1 admission per 100,000 population for four days, or about 40 admissions per 1 million residents over the entire week. In our set of robustness checks, in Figures B1b to B6b, 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 when it comes, for example, to heart attacks.[9]

We interpret the similarity of these four-day pattern across different disease groups and cross-national datasets as strong support for our identification strategy. The implication is that additional sleep leads to human capital improvements across a broad range of disease groups for people who are on the margin

[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.

to getting 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 is "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. The authors state that the evaluation and treatment of sleep disturbance in patients undergoing treatment for cancer was important.

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. (-15%) for four days underscores that we identify true fall DST effects here. And that fall DST effects represent the health effects of more sleep.

In contrast, and in line with the BRFSS, there is not much evidence for significant and severe negative health shocks after the spring DST change. The curves in Figures 5a and 6a fluctuate slightly which is not surprising given the powerful data, but overall, they are remarkably flat around the zero line. One may interpret the slight upticks in total admissions and heart admissions on Monday and Tuesday after the time change as evidence for negative human capital effect of fewer hours of sleep, but such an interpretation would be speculative.

Figures B1a to B9a in the Appendix show the graphs for the *(i)* full sample, *(ii)* heart attacks, *(iii)* respiratory, *(iv)* metabolic*, (v)* neoplastic and *(vi)* infectious admissions. Although, overall, the lines are flat around the zero line, the empirical patterns solidify the observation that there might be significant increases in admissions on Monday and Tuesday after the clocks are turned back in spring—which would shorten sleep by one hour in the absence of behavioral adjustments. One observes such significant increases for injuries (Figure B3a), metabolic admissions (Figure B5a), neoplastic admissions (Figure B6a) and suicide attempts (Figure B7a).

Aggregation at the Weekly Level

Next, we aggregate the effects at the weekly level and estimate equation (2). Each column of Table 4 represents one regression model with the outcome variable displayed in the column header. In line with the findings from the US and the BRFSS, Table 4 does not provide much evidence for increases in hospitalization rates at the weekly level after spring DST. The coefficient estimates carry unsystematic positive or negative signs throughout and have a magnitude of about 1-2% of the mean. Increases of 3-4% can be excluded with 95% statistical certainty (see also Figures 5a-6a, B1a-B8a).

Two exceptions are the neoplastic admission rate which carries a coefficient size of 3.3% of the mean, is positive, and significant at the 5% level. Similar is the case for suicide attempts with a marginally significant, positive, and relatively large coefficient. Inspecting the according Figures B6a and B7a, one finds that the weekly increase is driven by the significant one or two day increases in admissions at the beginning of the week as discussed above. However, one has to interpret these effects very carefully and keep in mind how powerful our data are.

[Insert Table 4 about here]

Second, the aggregated weekly effects following fall DST are in line with the daily approach: for all disease groups except drug overdosing, one finds significant decreases in admissions rates when sleep duration increases by up to one hour in fall. Note that the estimates here represent average daily decreases in the week following fall DST—from Sunday of fall DST to the following Saturday. The results are entirely consistent with Figures 5b and 6b as well as B1b to B9b. For example, Figure 6b shows significant decreases of heart admissions by about 1.2 per 100,000 between Monday and Thursday—about 5 fewer admissions per 100,000 pop. over the entire week. One obtains exactly the same figure when multiplying the coefficient estimate of -0.72*** in column (2) of Table 4 by 7 (days).

4.3 Mechanisms

Table 3 provides direct evidence on sleep adjustments in spring and fall using the sleep-related BRFSS survey questions. Column 1 shows that, on average, people sleep an additional 0.1 hours in the entire week of fall DST (i.e, in total 0.7 hours more). However, there is *no* evidence that people sleep fewer hours as a result of spring DST. This finding strongly suggests that people simply adjust their bedtime following spring DST but not necessarily following fall DST. In addition, column (2) shows that share of Americans who unintentionally fall asleep during the day decreases by a significant 4.4ppt (12.6%) in the week of fall DST but remains insignificant in spring DST.

The analogous graphical representation with the plotted daily effects is in Figure 4. Again we observe the characteristic drop in fall that lasts for four days. Figure 4a also provides some evidence that spring DST may actually increase the share of tired people on the Monday and Tuesday following spring DST by a significant 0.5 and 1ppt—probably because behavioral adjustments in spring are only partial. This finding is in line with the mild health effects observed in Figures 5a, 6a, B3a, B5a-B7a, and the relatively large, albeit imprecisely estimated, "sufficient rest" coefficients in columns (5) and (6) of Table A4.

The biological sleep rhythm of humans provides an explanation for these asymmetric sleep effects after clock changes. Without behavioral adjustments, after spring DST, people would sleep one hour less in the night from Sunday to Monday. However, to the extent that such behavior leads to tiredness on the Monday, most people would probably simply go to bed one hour earlier Monday night, e.g., at 10pm instead of 11pm. In fall, by contrast, sleep deprived people (who do not automatically wake up after X hours) could sleep one hour more in the night from Sunday to Monday. To the extent that the sleep deprived do not fully adjust their bedtime to wintertime but keep on going to bed at summertime 11pm, the sleep extension would carry over to the next days.

A related explanation for asymmetric behavioral sleep adjustments is media exposure. Every year at spring and fall DST, the media broadly covers the topic. In particular when springing forward one hour, experts regularly warn about the dramatic health dangers that DST could trigger. One likely consequence of this broad media exposure is that people on the margin are effective in adjusting their bedtime and/or act more carefully on the days following spring DST. Figure 7 shows the result of a google search request using the keywords "daylight savings time" and "heart attack". As seen, searches for DST spike at exactly the times of spring and fall DST. Interestingly, "heart attack" searches also seem to spike around spring DST but trend smoothly around fall DST, which yields support of hypothesis just discussed.

[Insert Figure 7 about here]

Another explanation for why people react differently to spring and fall time changes could be loss aversion.[10] Without prior adjustments, people lose one hour of sleep in spring and gain an hour of sleep

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[10] Examples of recent research include loss aversion in the context of labor supply of taxi drivers (Camerer et al., 1997), housing prices (Genesove and Mayer, 2001), putting behavior of professional golf players (Pope and

in fall. A loss-averse individual would therefore react more strongly to the spring DST than to the fall DST, for instance by going to bed an hour earlier to ensure to get the same hours of sleep.

Investigating Mechanisms using Socio-Demographics

Next, we make use of the BRFSS socio-demographics to investigate effect heterogeneity and provide further evidence for underlying mechanisms. We hypothesize that the effects differ depending on how time-constraint people are. If people have the possibility to adjust their bed and their wake-up times flexibly, one would expect the potential health effects to be smaller, particularly in spring. Those who work without flexible working schedules are presumably the most time-constrained. This is the societal subgroup among which we would expect to find most sleep deprived people.

[Insert Table 5 about here]

Technically, we investigate effect heterogeneity by interacting the *Week of Begin/End DST* variables of the weekly approach in equation (2) with the socio-demographics of interest. The results are in Table 5. The observed pattern confirms the hypothesis above, namely that subgroups which are likely to be time-constraint, sleep deprived, and inflexible in their daily schedules are the most affected. *(i)* The share of males in bad health significantly increases by 1.4ppt following spring DST (column (5)). It also decreases more strongly when their sleep is extended. *(ii)* The increase in the share of people in excellent health following fall DST is driven by people under 50 (column (2)).

Investigating Mechanisms using Daily Variation in Weather and Pollution

Lastly, we investigate effect heterogeneity by weather and pollution conditions using the German Hospital Census. As explained in Appendix B, we collect 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

Schweitzer, 2010), policies to reduce plastic bag usage (Homonoff, 2013), and income tax sheltering (Rees-Jones, 2014).

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 (Ziebarth et al., 2013), we expect pollution to operate in interaction with changes in sleep pattern.

The first four columns of Table B2 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, they seem to have no impact on admissions following fall DST. This finding reinforces the notion that the spring DST effects are accompanied or "confounded" by behavioral adjustments. Obviously, spending time outdoors does not matter when sleep deprived individuals get more sleep in fall. However it does play a role in spring. This could either be the case because people spend more time outdoors in a more dangerous environment which leads to higher admission rates. Or it could be that individuals do not adjust their bedtime and go to bed earlier because the opportunity costs are higher when the outdoor conditions are better.

Finally, columns (5) to (8) of Table B2 show that admissions increase whenever pollution conditions worsen, independent of spring and fall DST. 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, and also that these people do not or cannot adapt to outdoor air pollution. 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.

4.4 Placebo Estimates

Ex ante, it is very difficult to identify severe health issues that require overnight stays in a hospital but are—by construction—not affected by more rest and sleep. We provide two approaches to placebo regressions. The first plots daily effects for health issues that are least likely to be affected by sleep, and the second approach estimates weekly placebo DST effects for the rest of week without time changes.

First, Figures B8 and B9 plot the results of the daily approach for the diagnoses *(i) drug overdosing*, and *(ii) infectious diseases*. While both diagnoses could, in principle, be affected by sleep we consider it less likely in these cases, because *(i)* should be driven by very time-persistent individual issues such as addiction and not be triggered by one hour more or less sleep. Infectious diseases are transmitted airborne or through personal contact. Both ways of infection should, in principle, not be affected by DST. On the other hand, whether an admission is triggered may depend on the baseline health status of the infected person, which in turn, may be affected by DST.

The graphical evidence in Figures B8 to B9 shows very little 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. If anything, one might observe the characteristic four-day decrease in admissions for infectious diseases. Overall, we interpret the fact that suicide attempts and drug overdosing do not react to either spring or fall DST as in line with our identification strategy and the identified four-day effects of the other disease groups.

Our second approach for estimating placebo effects is as follows: 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 weekly model as in equation (2) 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 reiterate 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.[11] In total, as such, we obtain 22 spring placebo weekly 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 weekly coefficient estimates are plotted in Figures B10a and B10b along with the true spring and fall estimates (rightmost coefficients #23 and #24).

Figure B10a shows the following. First, our empirical approach is sophisticated enough to eliminate most seasonal confounders that may affect admissions. Except for one estimate in the first half of the year, in Figure B10a, all weekly coefficient estimates are close to the zero line, fluctuate very little, and lie between the boundaries of -2ppt and +2ppt (relative to a mean of 59.8). Second, only one estimate—pseudo spring DST estimate #15—is an outlier and a large '-4', representing a decrease in admissions of almost -7%. Pseudo spring DST estimate #15 represents calendar week 18 with the last days of April and first days of May. In Germany, May 1st is Labor Day, a national holiday. Admissions typically decrease strongly on national holidays, e.g., by 54 per 100,000 population (or 90%) on Easter Monday and 24 on Good Friday. Because we do not specifically control for May 1st in the specification, the weekly decrease

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

by -4 could be entirely explained by a May 1st decrease of 28 admissions. Third, the latter point illustrates the limitations of our pseudo DST approach using administrative data: to eliminate all admission shocks, we would need to net out every possible seasonal confounder such as national holidays, all vacations days, periods of regional festivities such as carnival, and also consider specific singular shocks that may have affected admissions. While we are confident to having comprehensively considered all confounders during the true six spring and fall DST weeks, this is not the case for all remaining weeks of the year. When interpreting the placebo estimates, this fact has to be considered when interpreting the estimates.

In Figure B10b, all pseudo DST effects are very small in size, fluctuate only slightly around the zero line, and are very precisely estimated. The only clear outliers are the true fall DST estimate, and pseudo fall DST estimate #15. Pseudo fall DST estimate #15 represents week 41 of the calendar year, which is typically the second week of October. We do not have a perfect explanation for this outlier. It could simply be noise and related to weather effects—admissions decrease on sunny days and mid-October typically provides the last nice days of the year—or due to vacation effects. Even though we control for school vacations, Germans without kids typically go on fall vacation for one to two weeks in October. October 3rd is a national holiday in Germany (German Reunification Day) and often leads to a "long" weekend. Many Germans take advantage of this long weekend and extend it for their fall vacation.

5. DISCUSSION AND CONCLUSION

This paper exploits the quasi-experimental nature of Daylight Saving Time (DST) to assess how changes in sleep affect human capital. It is one of very few causal studies on this topic and exploits one decade of both survey data for the US and administrative data on hospital admissions for Germany. To be able to properly investigate the sleep-health relationship via DST variation, one requires powerful representative data over many years. Because DST happens only twice a year, and always on Sunday nights around Eastern and fall vacation times, 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.

Overall, the findings yield strong support for the notion that humans' most time-consuming activity, sleep, does affect their human capital. The clearest evidence of the positive health effects of sleep stem from time change in fall. The fall DST change effectively means for many people—particularly sleep deprived and time-constraint people—that they can sleep one hour more. Broad and convincing evidence documents that millions of people in Western societies are heavily sleep deprived (Valdez et al. 1996; Duffy et al. 2001, Duffy and De Gennaro, 2001; Moore et. al., 2002; Roenneberg et al., 2007; Knutson et al., 2007). Our findings show consistent and robust evidence that health significantly improves for about four days after people gain one hour of sleep in fall. About 2.5 million more Americans consider themselves in excellent health in self-reports, they sleep significantly more, and one observes a decrease in their probability to fall asleep during the day. Moreover, administrative hospital admission data clearly demonstrate a characteristic four day drop in admissions in the days following fall DST. For example, cardiovascular admissions decrease by a significant 1 admission per 100,000 population over 4 days. One finds very similar patterns and decreases for patients with other diseases (which are not necessarily diagnosed on these days) but—as expected—no changes in drug overdoses or infections. These findings suggest that additional sleep can lead to significant human capital improvement and prevention effects for people on the margin to a severe health shock. It is one of the first clear quasi-experimental evidence of the health benefits of sufficient sleep.

The main objective of this paper is to provide evidence for the existence of a causal relationship between sleep and human capital. We do not intend to draw conclusions about the overall welfare effects of Daylight Saving Time. We also would like to point to a caveat: our 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 human mood 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**

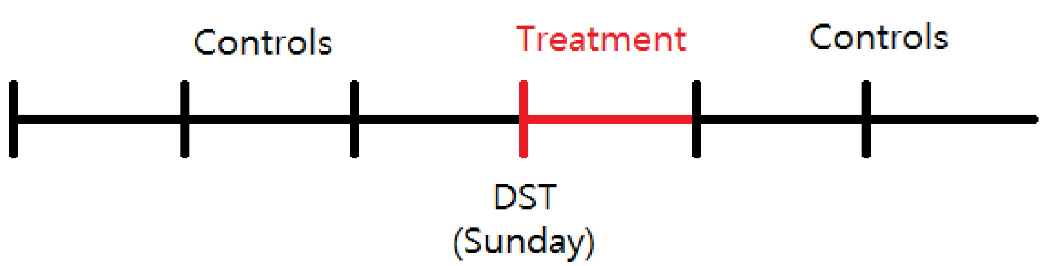


Figure 2a and b: BRFSS Daily Approach:

Daily Effects of DST on Share of People Reporting Excellent Health, 2001-2010

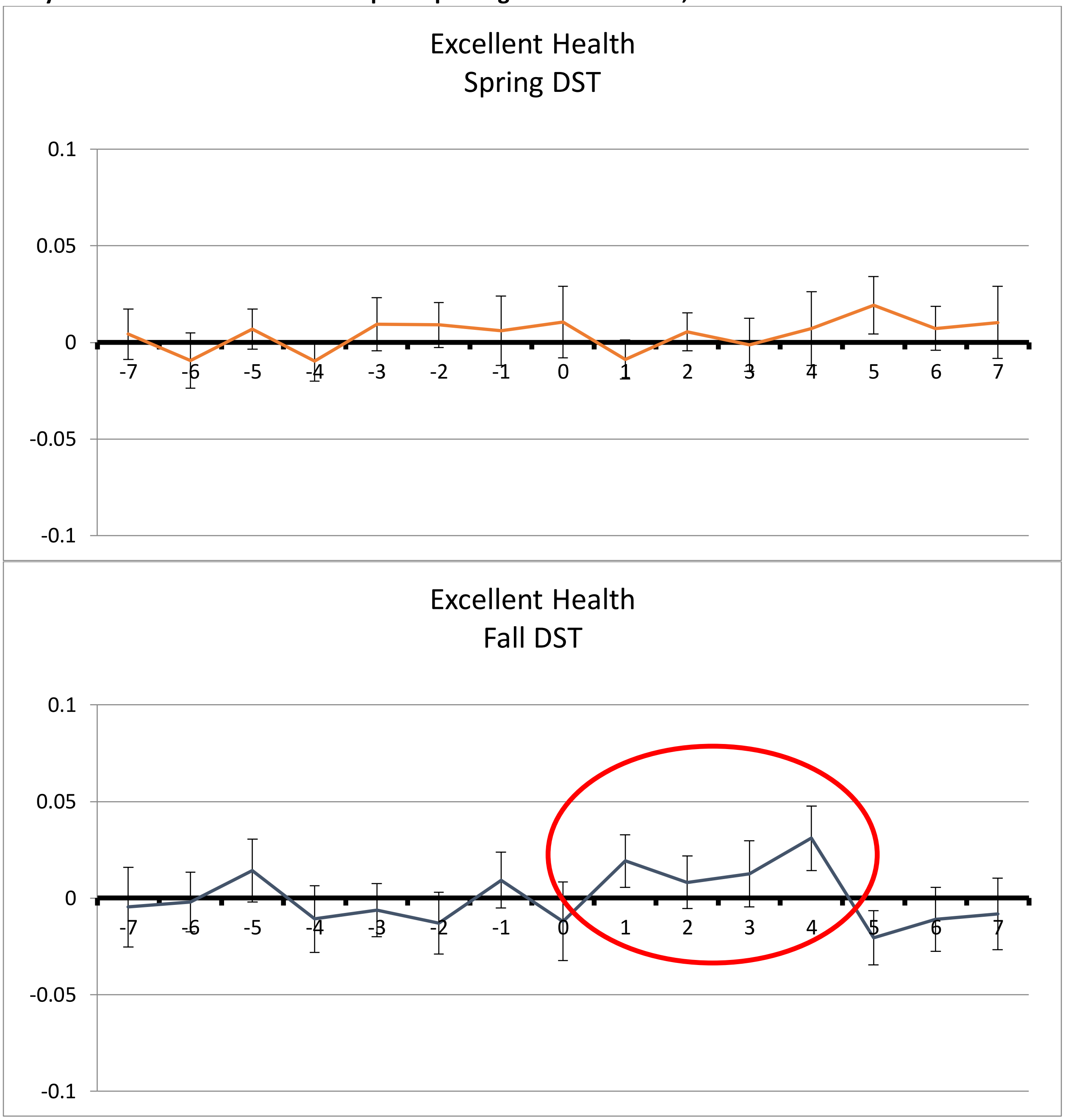
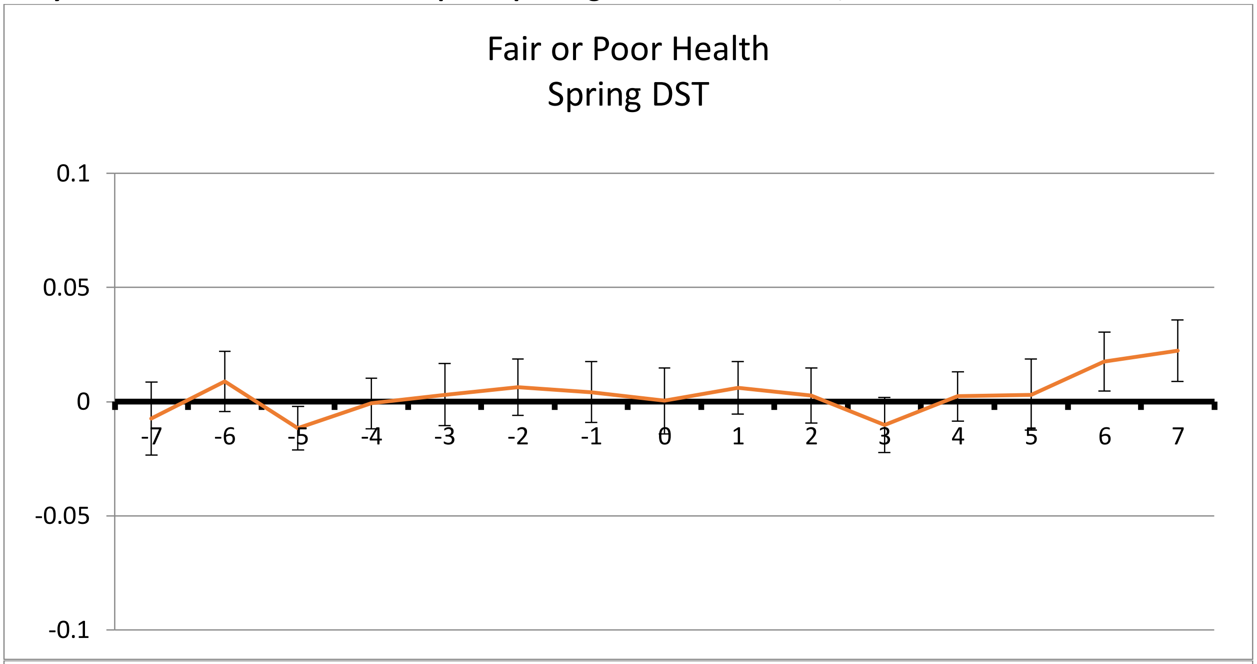
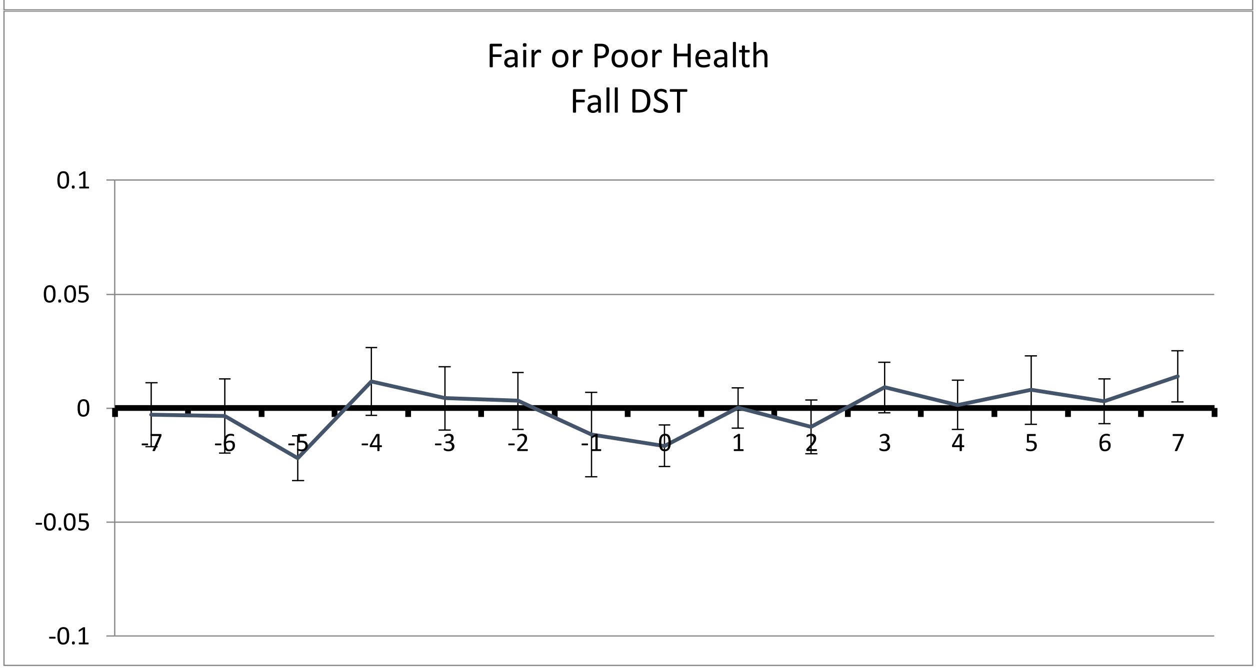


Figure 3a and b: BRFSS Daily Approach

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





<u>Figure 4a and b: BRFSS Daily Approach</u>

People Reporting Unintentionally Falling Asleep on at least 1 Day in the Past 30 Days, 2001-2010

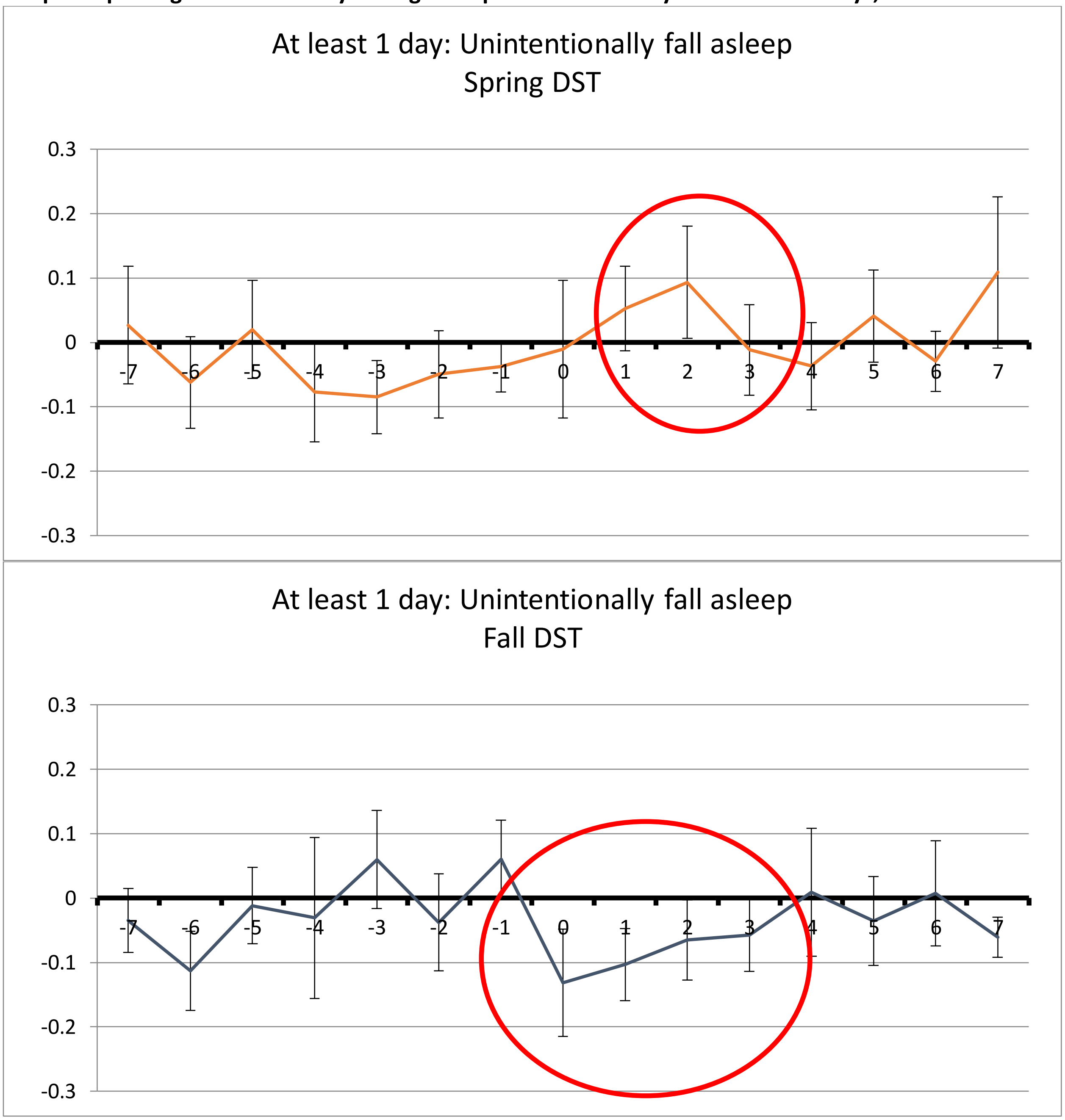
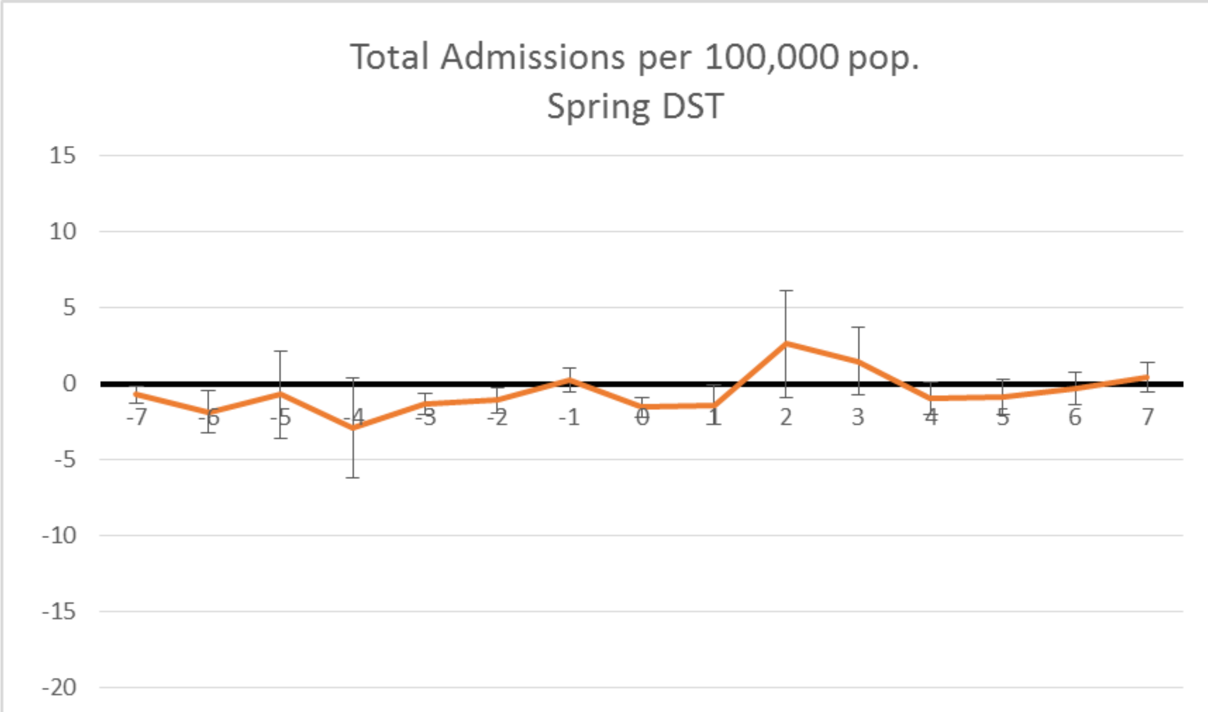


Figure 5a and b: Hospital Census Daily Approach

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



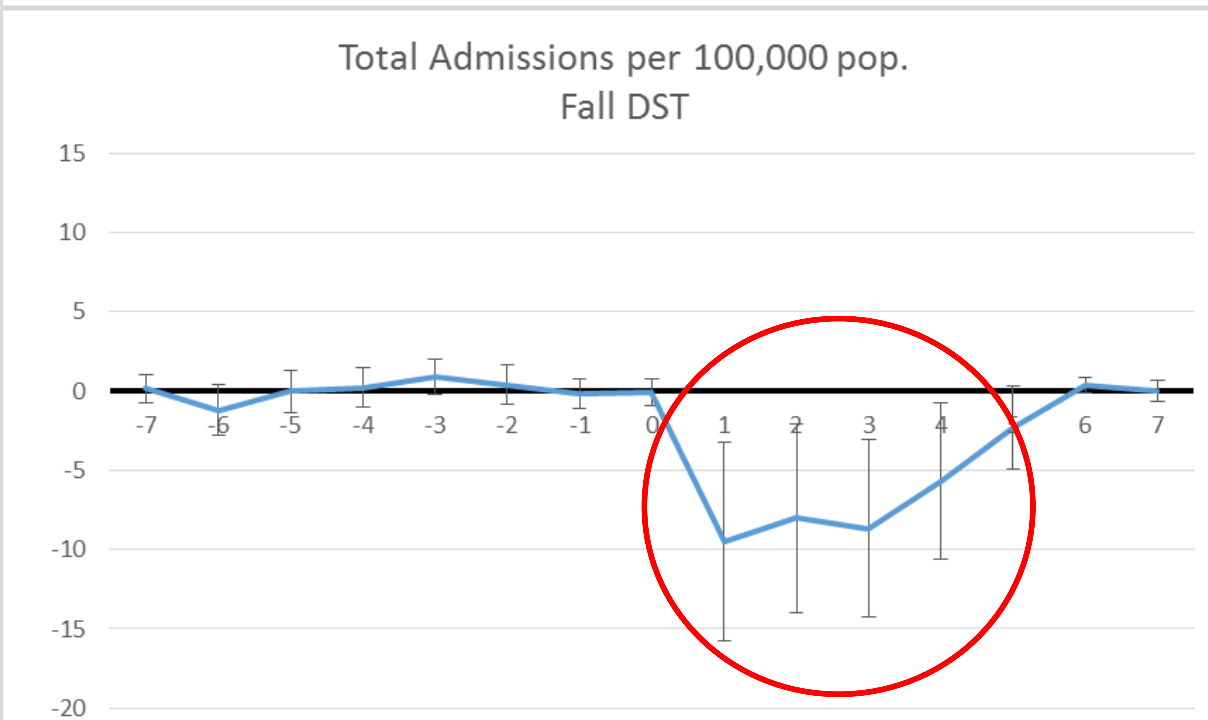


Figure 6a and b: Hospital Census Daily Approach

Daily Effects of DST on Cardiovascular Admissions, 2000-2008

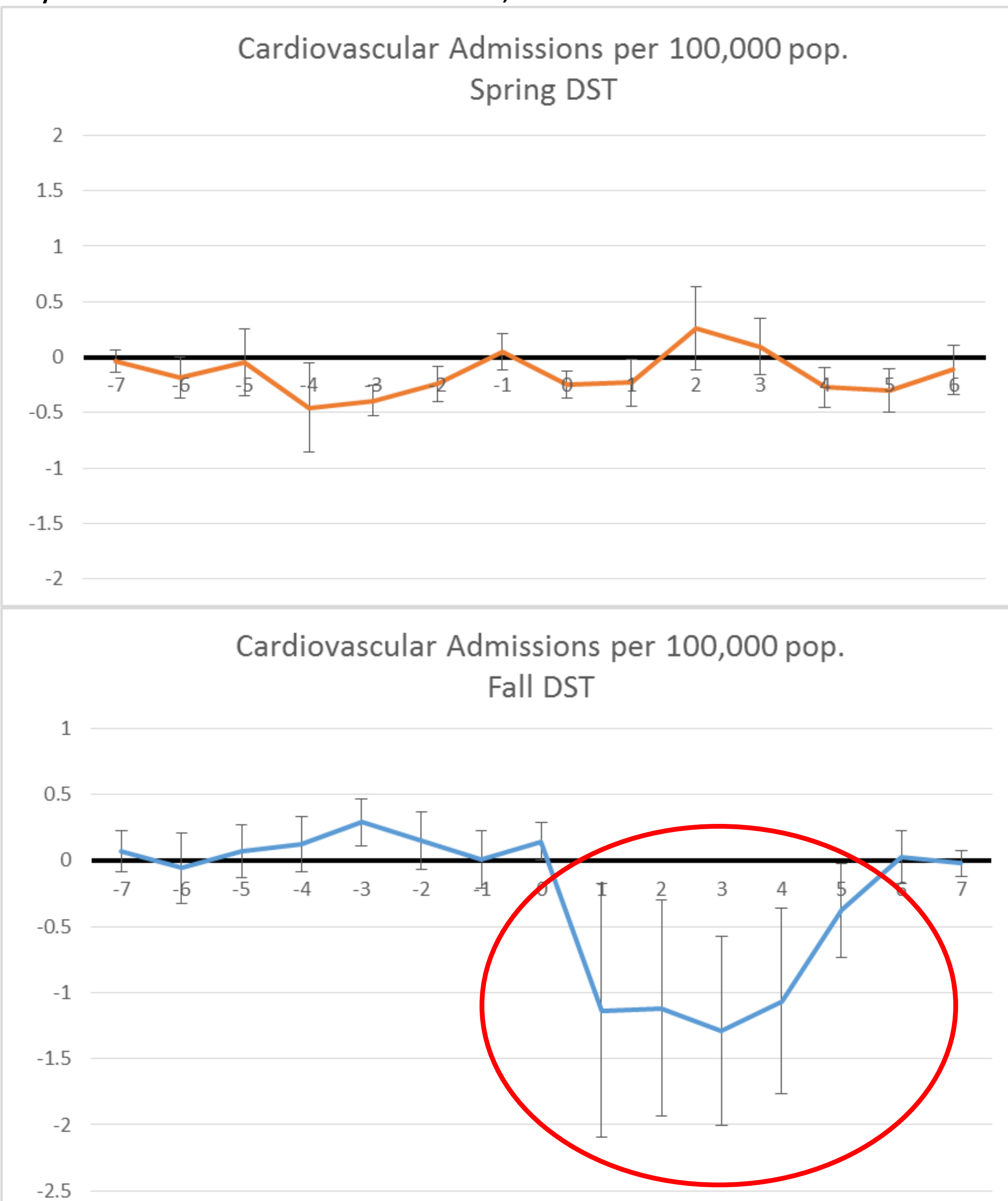


Figure 7: Google Searches

"DST" and "Heart Attack" Google Search Volume Over the Year

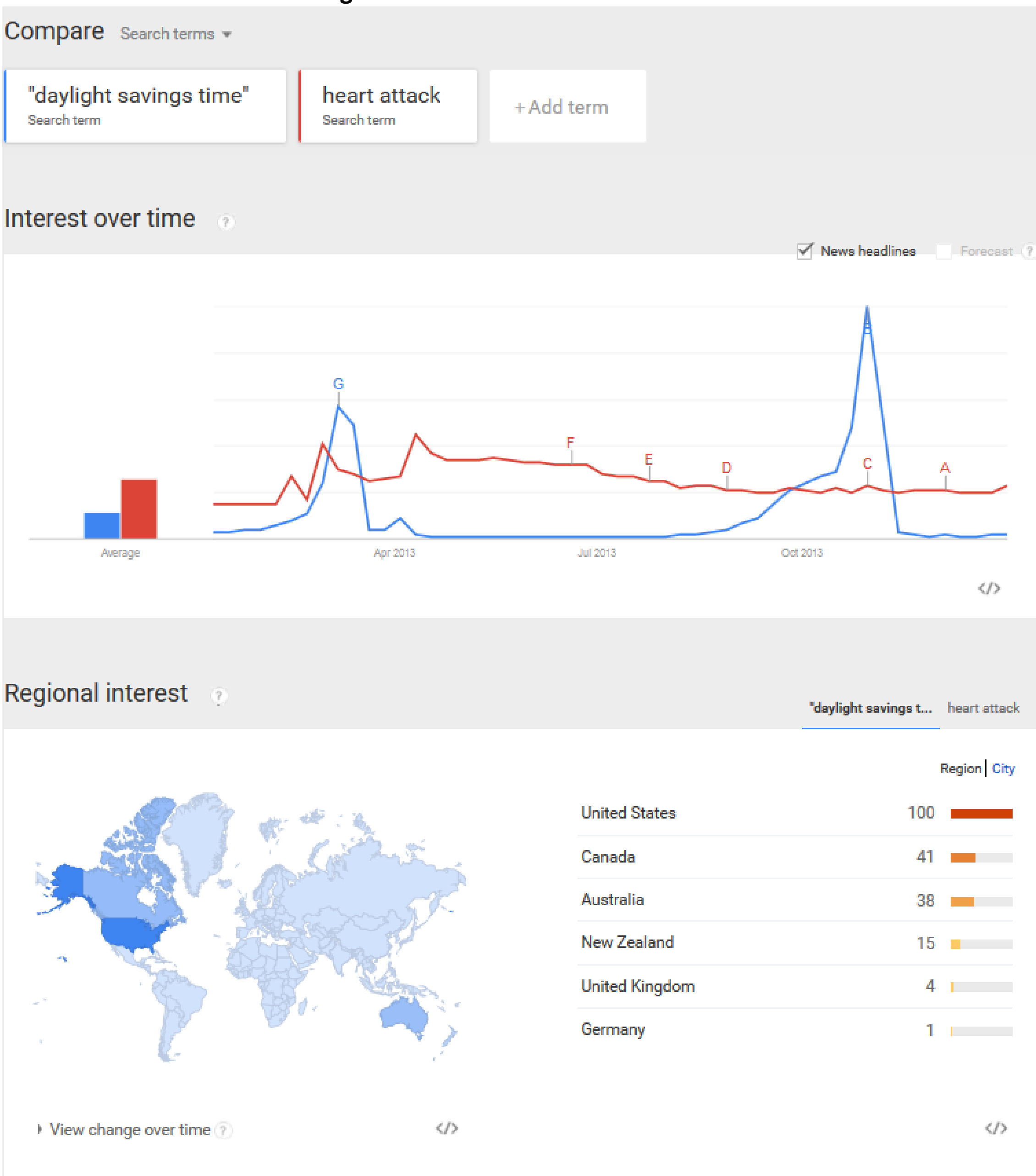


Table 1: DST US

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 2: 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 3: BRFSS Weekly Approach Alternative Outcomes

Weekly Effects of DST 2001-2010: Alternative Outcome Variables

	(1) Hours of sleep in a 24-hour period	(2) At least 1 in past 30 days: unintentionally falling asleep during day	(3) 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: Hospital Census Weekly Approach: Specific Diseases

Effects of DST on Universe of Hospital Admissions 2000-2008, by Disease Type

	All cause admission rate (1)	**Cardiovascular admission Rate (2)**	**Heart attack rate (3)**	**Injury admission rate (4)**	**Metabolic adm. rate (5)**	**Neoplastic adm. rate (6)**	**Suicide Attempt rate (7)**	**Drug Overdosing (8)**
Week of Spring DST	0.2123	-0.0664	-0.0104	0.0142	0.0348	0.2204**	0.0230*	0.0099
(2am → 3am at end of March)	*(0.4274)*	*(0.0547)*	*(0.0214)*	*(0.0145)*	*(0.0243)*	*(0.1100)*	*(0.0124)*	*(0.0061)*
Week of Fall DST	-4.9556***	-0.7195***	-0.0882***	-2.7121***	-0.1874***	-0.7357***	-0.0276**	-0.0044
(3am → 2am at end of Oct)	*(1.1139)*	*(0.1589)*	*(0.02611)*	*(0.6869)*	*(0.0385)*	*(0.1884)*	*(0.0128)*	*(0.0055)*
Controls								
County FE	X	X	X	X	X	X	X	X
Easter & 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 Fixed Effects	X	X	X	X	X	X	X	X
Linear & quadr. time trend	X	X	X	X	X	X	X	X
Socioeconomic covariates	X	X	X	X	X	X	X	X
Mean of dep. variable	59.77	9.53	1.59	57.56	1.73	6.59	0.32	0.09
R^2	0.8469	*0.5675*	*0.1510*	*0.2067*	*0.3095*	*0.6986*	*0.0179*	*0.0008*
Observations	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***	***336,604***

Note: * p<0.1, ** p<0.05, *** p<0.01; standard errors are in parentheses and two-way clustered at the county and date level. The *Week of Begin/End DST* variables are indicator variables that equal 1 if the interview date is on the DST Sunday or one of the following 6 days. Table B1 lists the dependent variables for as displayed in the column header. Each column is one model as in equation (2).

Table 5: BRFSS Weekly Approach: Heterogeneity

Weekly Effects of Begin and End of DST on Self Assessed Health (SAH) 2001-2010: Testing Effect Heterogeneity

	(1) Excellent health	(2) Excellent health	(3) Excellent health	(4) Excellent health	(5) Fair or Poor health	(6) Fair or Poor health	(7) Fair or Poor health	(8) Fair or Poor health
Variable	***Male***	***Age < 50***	***Retired***	***Married***	***Male***	***Age < 50***	***Retired***	***Married***
Begin DST * [column header]	-0.00014 (0.00640)	0.00132 (0.00555)	-0.00361 (0.00598)	0.00108 (0.00556)	0.01471*** (0.00520)	-0.00674 (0.00481)	0.00142 (0.00758)	-0.00574 (0.00489)
End DST * [column header]	0.00529 (0.00661)	0.01319** (0.00543)	-0.00457 (0.00514)	-0.00035 (0.00590)	-0.00773 (0.00583)	0.00381 (0.00461)	-0.00966 (0.00641)	-0.00639 (0.00522)
Week of Begin DST (2am → 3am in spring)	0.00318 (0.00431)	0.00229 (0.00403)	0.00371 (0.00371)	0.00248 (0.00506)	-0.00634* (0.00341)	0.00468 (0.00407)	0.00042 (0.00296)	0.00410 (0.00474)
Week of End DST (2am → 1am in fall)	0.00353 (0.00430)	-0.00147 (0.00394)	0.00685 (0.00464)	0.00623 (0.00528)	0.00316 (0.00391)	-0.00262 (0.00369)	0.00130 (0.00321)	0.00337 (0.00499)
Controls								
State FE	X	X	X	X	X	X	X	X
Easter and Halloween	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
Mean of dep. Var.	*0.193*	*0.193*	*0.193*	*0.193*	*0.184*	*0.184*	*0.184*	*0.184*
R^2	0.0643	0.0644	0.0643	0.0643	0.2041	0.2041	0.2040	0.2041
Observations	799,171	799,171	799,171	799,171	799,171	799,171	799,171	799,171

Notes: Standard errors in parentheses are clustered at the date level. *** Significant at 1% level, ** 5%, * 10%. Regressions are probability-weighted. *Begin/End DST* are indicator variables equal to 1 if the interview is on the DST Sunday or one of the following 6 days. Table A1 lists the dependent and the stratifying variables for the interaction terms as displayed in or below the column header. Each column is one model as in equation (2).

FOR ONLINE PUBLICATION

Appendix A: BRFSS

Figure A1: **BRFSS Observations by Years**

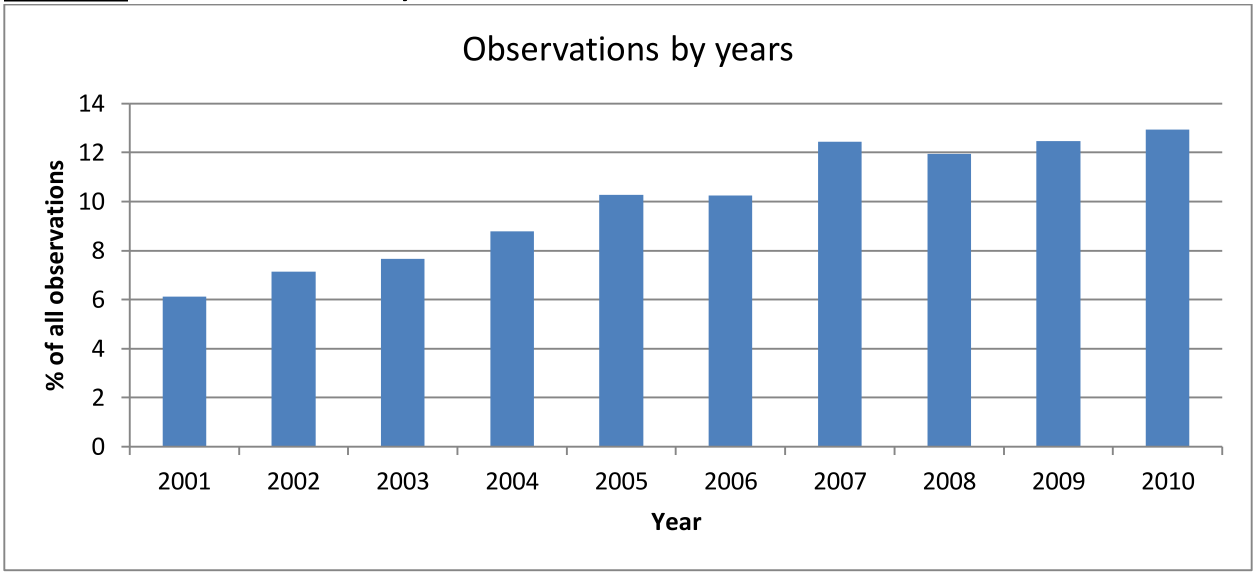
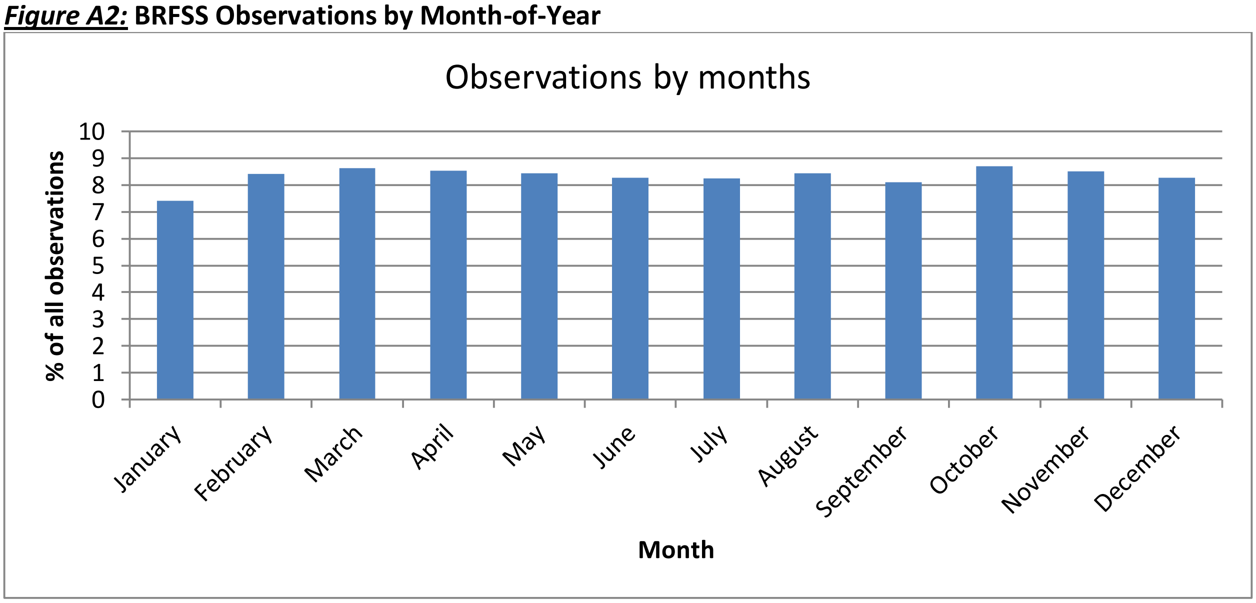


Figure A2: **BRFSS Observations by Month-of-Year**



<u>Figure A3a and b: BRFSS Daily Approach with Unweighted Full Sample</u>

Daily Effects of DST on Share of People Reporting Excellent or Very Good Health, 2001-2010

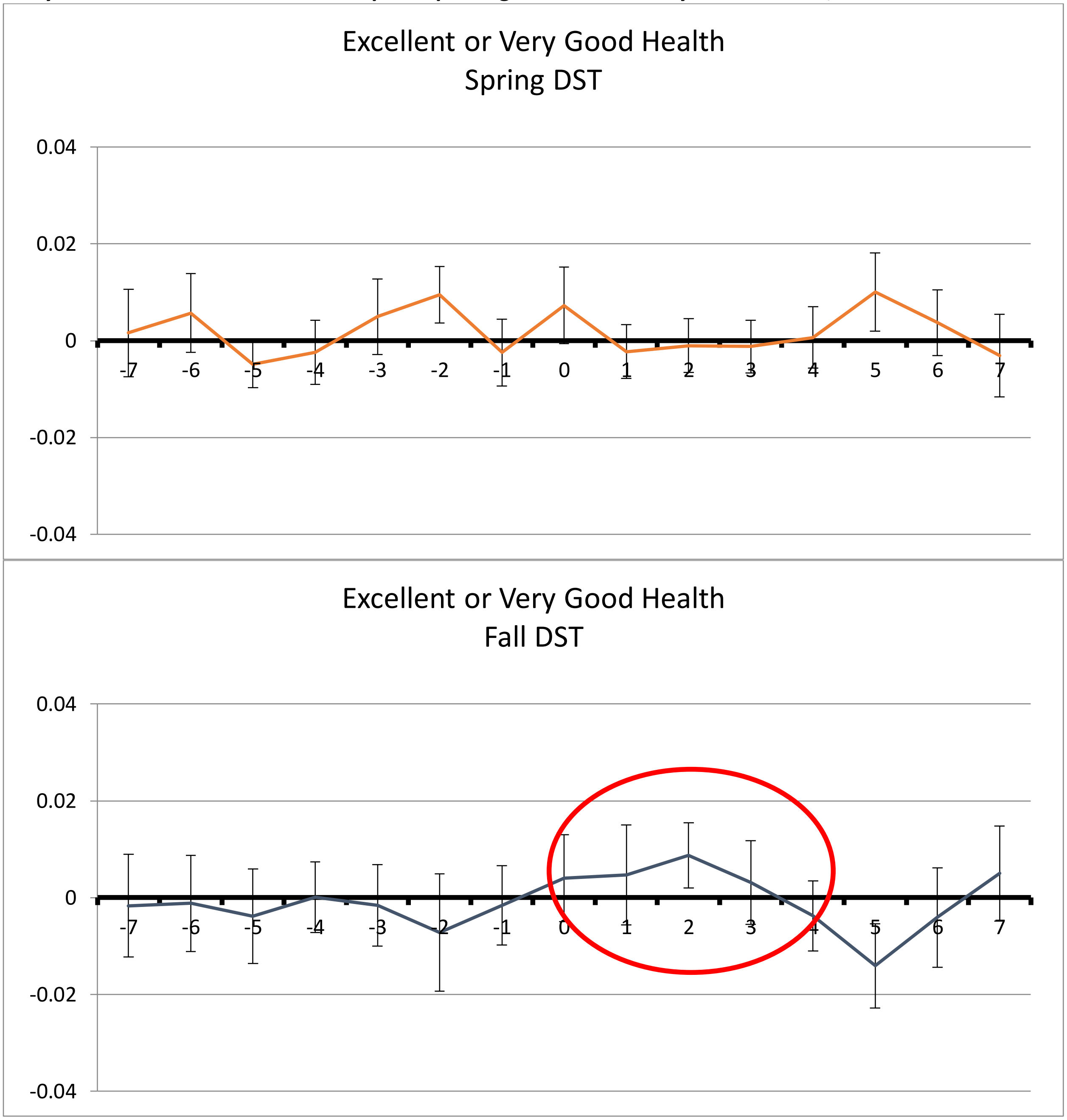
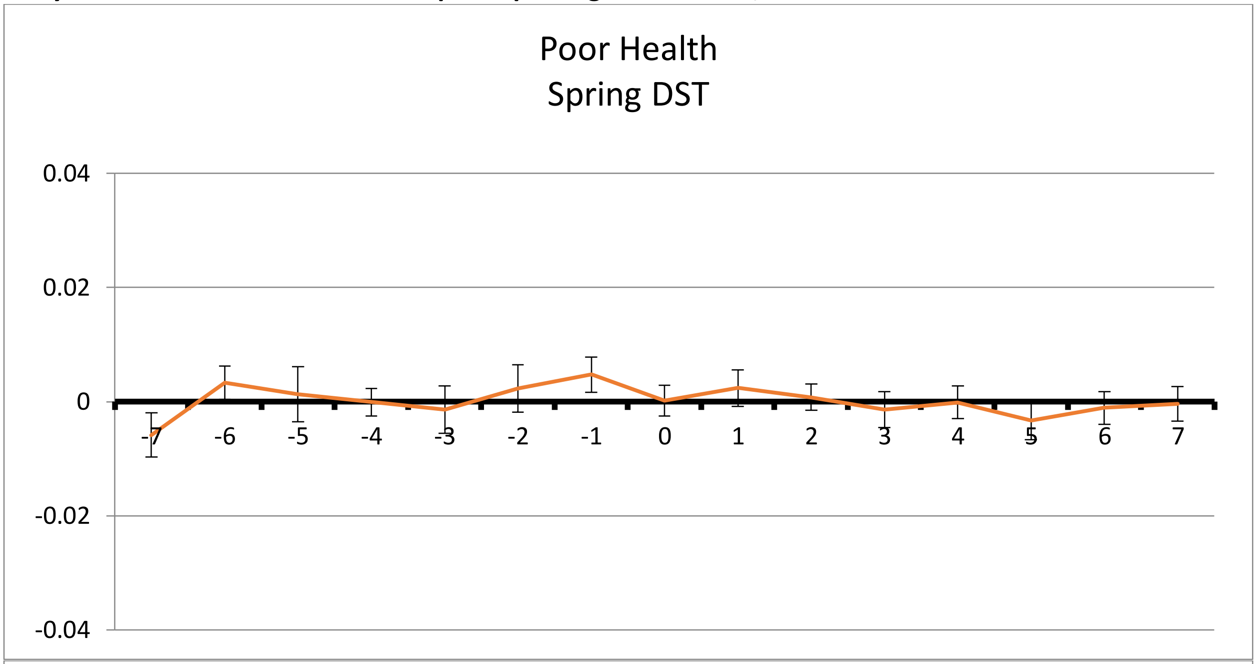


__Figure A4a and b: BRFSS Daily Approach with Unweighted Full Sample__

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



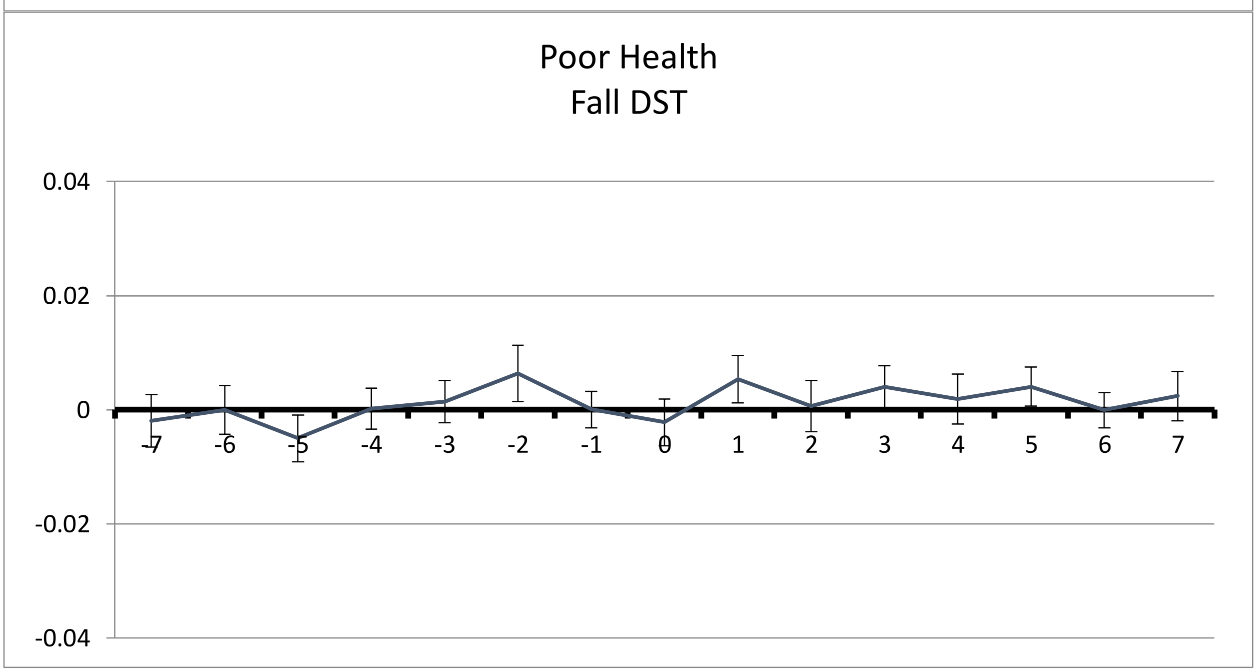


Table A1: **BRFSS Descriptive Statistics**

	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 A2: **BRFSS Distribution of Self-Assessed Health (SAH), 2001-2010**

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

Table A3: **BRFSS Balancing Properties between Treatment and Control Weeks, 2001-2010**

Demographic Characteristics	***Week of DST (treatment group) Mean***	***Neighboring weeks (control group) Mean***	***Normalized Difference***
Age	2.570	2.521	0.031
Female	0.186	0.195	-0.016
White	0.196	0.180	0.029
African American	53.500	51.649	0.075
Married	0.626	0.609	0.025
Never married	0.838	0.825	0.025
Number of Children in Household	0.082	0.088	-0.015
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 Δs = $(\bar{s}_1 - \bar{s}_0)/\sqrt{\sigma_1^2 + \sigma_0^2}$, with $\bar{s}_1$ and $\bar{s}_0$ denoting average covariate values for treatment and control group, respectively. σ denotes the variance. As a rule of thumb, normalized differences exceeding 0.25 indicate non-balanced observables that might lead to sensitive results (Imbens and Wooldridge, 2009).

Table A4: BRFSS Weekly Approach Alternative Outcomes

Weekly Effects of DST 2001-2010: Alternative Outcome Variables

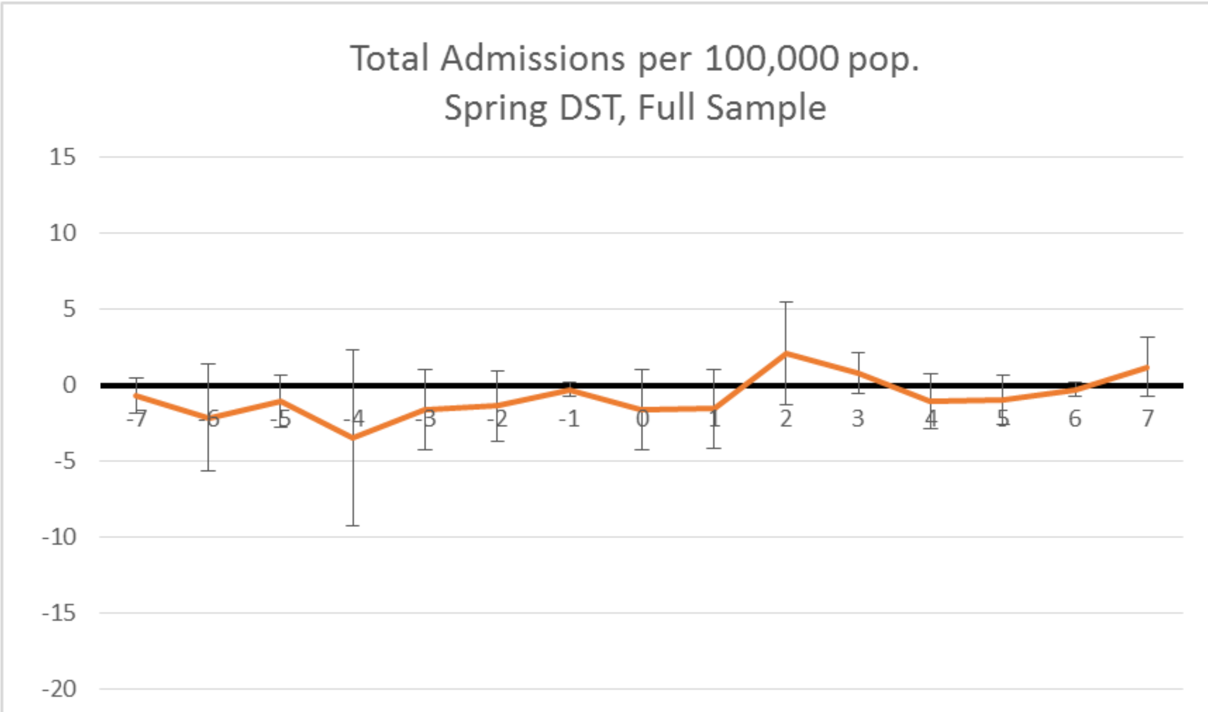
	(1) # Days in past 30 days w/ poor physical health	(2) At least 1 day in past 30 days w/ poor physical health	(3) # Days in past 30 days w/ poor mental health	(4) At least 1 day in past 30 days w/ poor mental health	(5) # Days in past 30 days w/ insufficient rest	(6) At least 1 day in past 30 days w/ insufficient rest
Week of Begin DST	0.01808	0.00530	0.04315	0.00169	0.18489	0.00618
(2am → 3am in spring)	(0.05544)	(0.00425)	(0.05932)	(0.00387)	(0.12580)	(0.00607)
Week of End DST	0.00238	-0.00300	0.11521	0.00236	-0.07988	-0.00230
(2am → 1am in fall)	(0.06277)	(0.00460)	(0.07557)	(0.00449)	(0.13069)	(0.00596)
Controls						
State FE	X	X	X	X	X	X
Easter and Halloween	X	X	X	X	X	X
Day of Week * Month FE	X	X	X	X	X	X
Month * Year FE	X	X	X	X	X	X
Linear &quadr. time trend	X	X	X	X	X	X
Socioeconomic covariates	X	X	X	X	X	X
Mean of dep. Var.	*4.119*	*0.371*	*3.396*	*0.320*	*7.812*	*0.638*
R^2	0.1722	0.0559	0.0880	0.0804	0.0696	0.1008
Observations	743,686	743,686	743,686	743,686	335,930	335,930

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. The column headers describe the dependent variables used in each column; columns (2), (4), and (6) use binary measures, and columns (1), (3), and (5) have values between 0 and 30. The summary statistics of the dependent variables are in Table A1. Each column is one model as in equation (2).

Appendix B: German Hospital Census

Figure B1a and b: Hospital Census Daily Approach: Full Sample

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



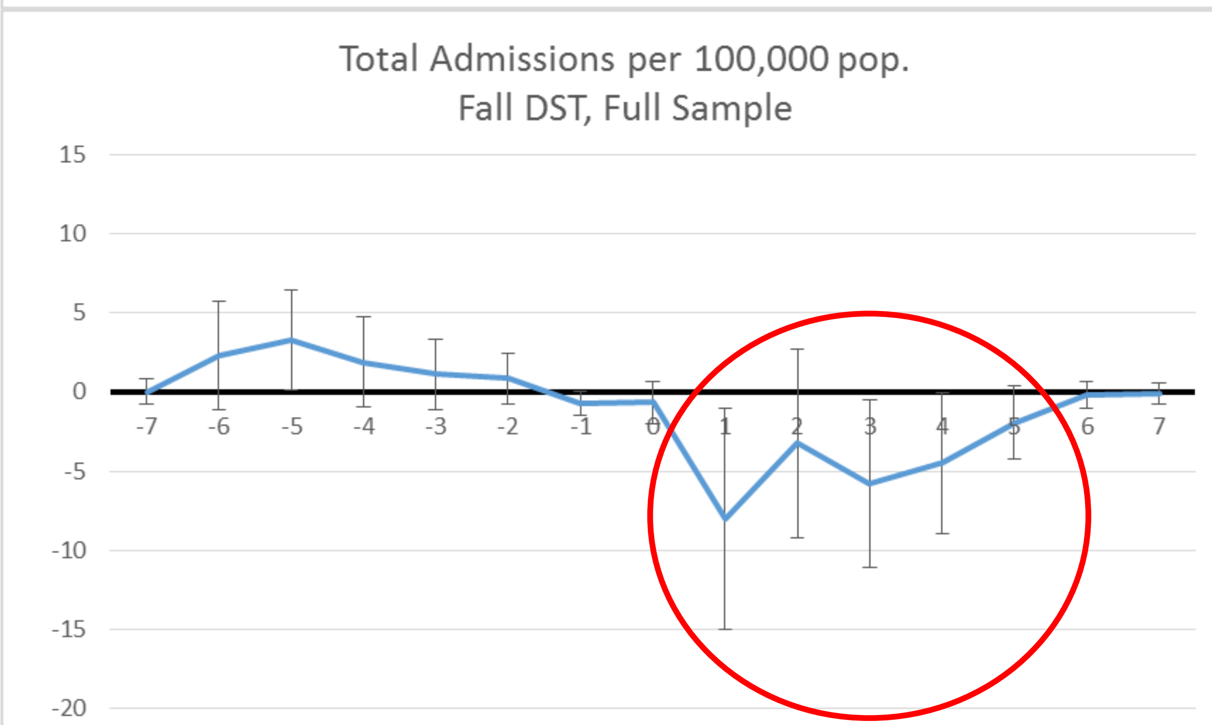
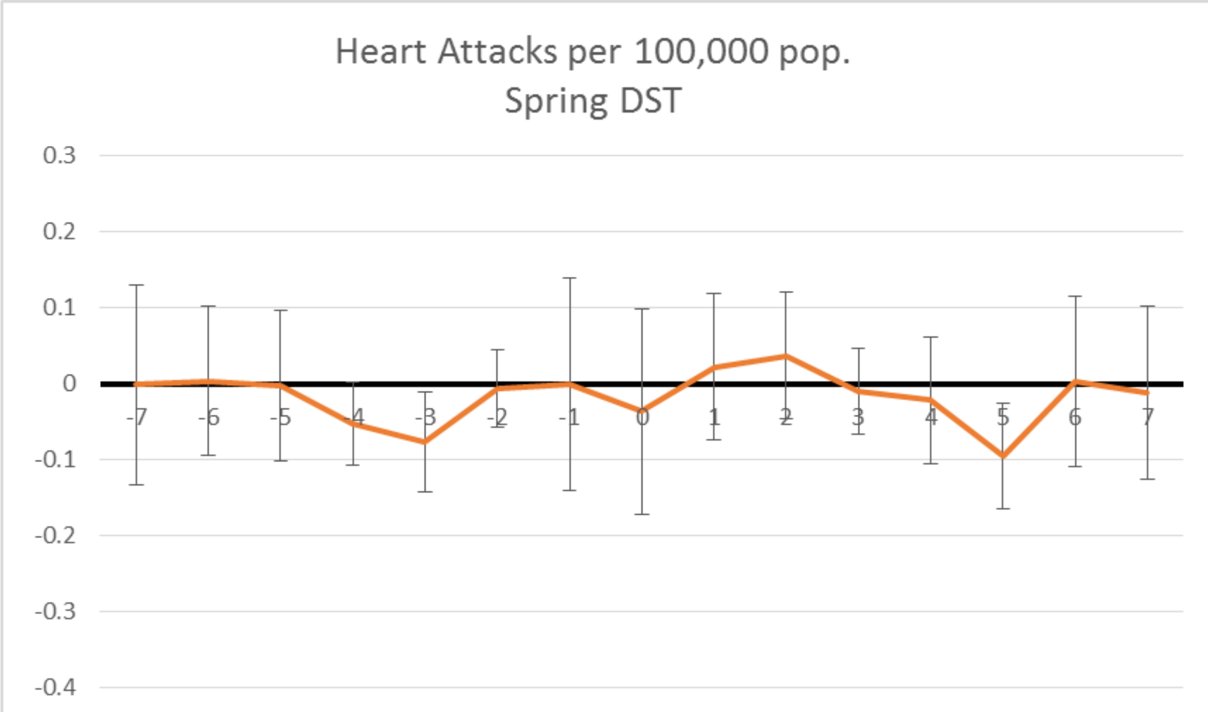


Figure B2a and b: Hospital Census Daily Approach
Daily Effects of DST on Heart Attack, 2000-2008



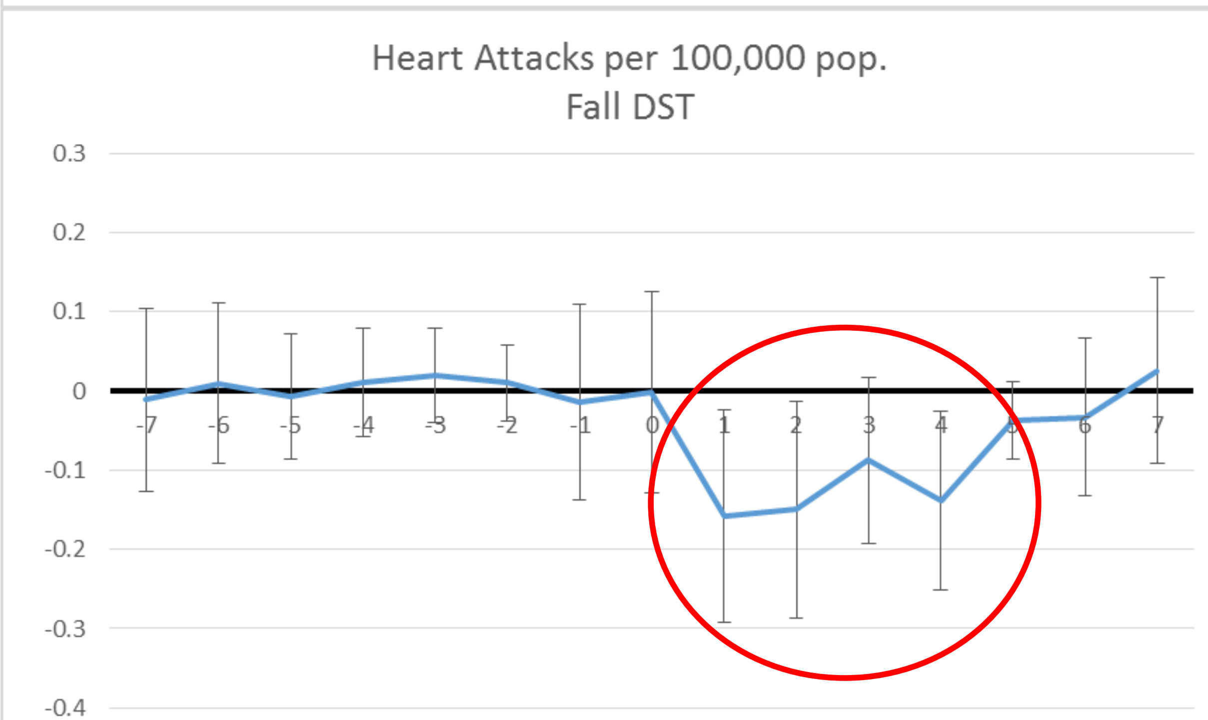


Figure B3a and b: Hospital Census Daily Approach

Daily Effects of DST on Injuries, 2000-2008

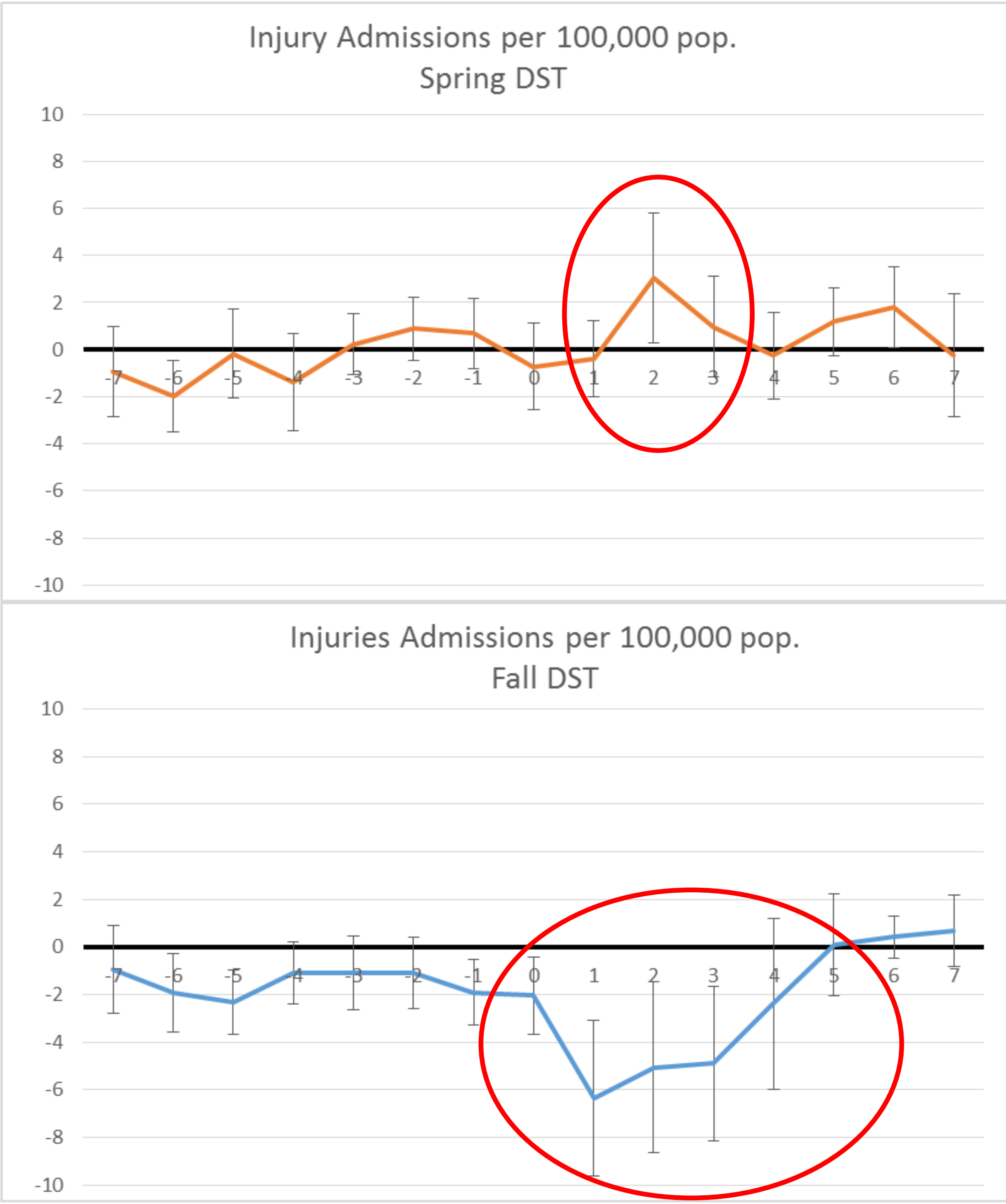


Figure B4a and b: Hospital Census Daily Approach
Daily Effects of DST on Respiratory Admissions, 2000-2008

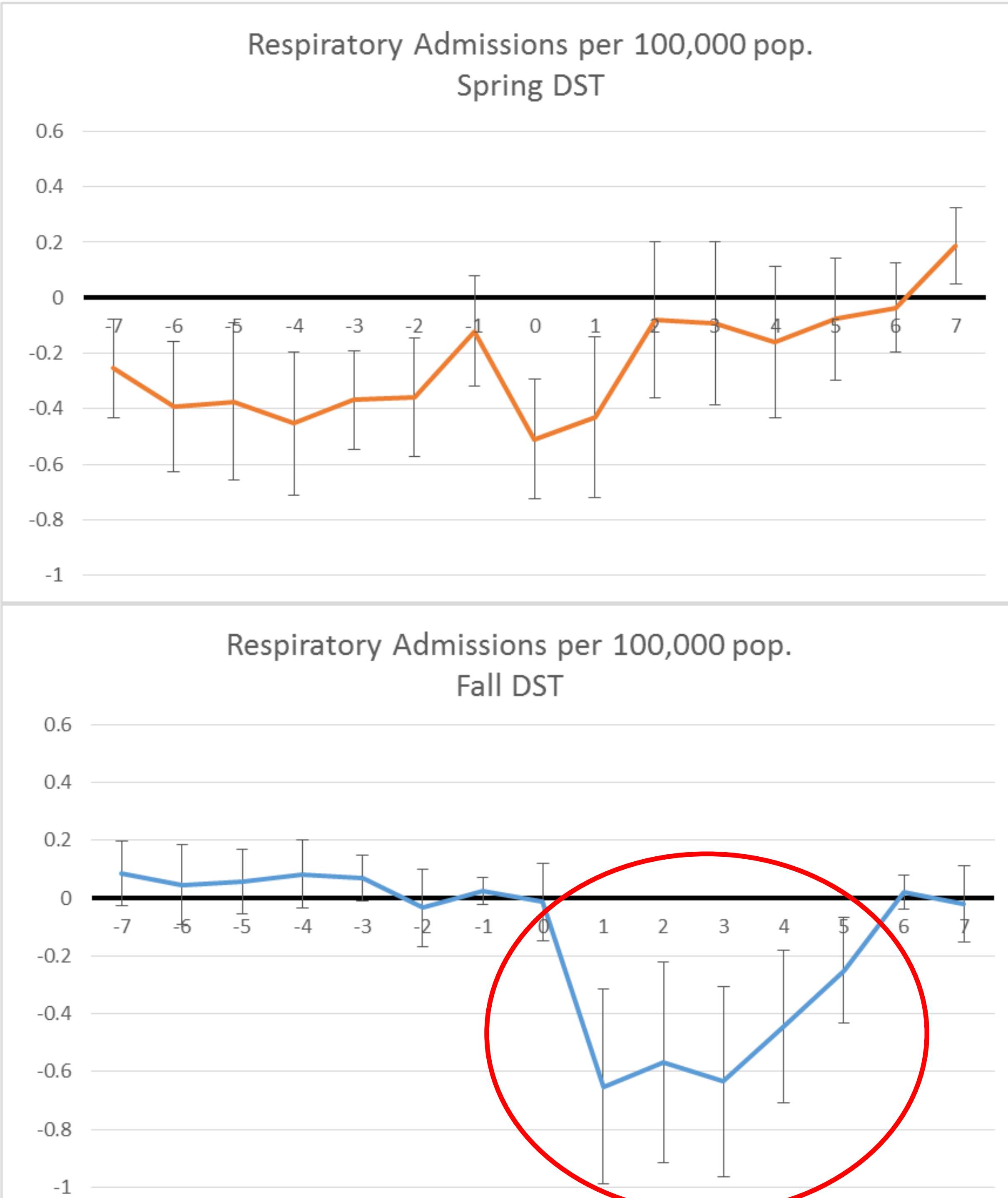
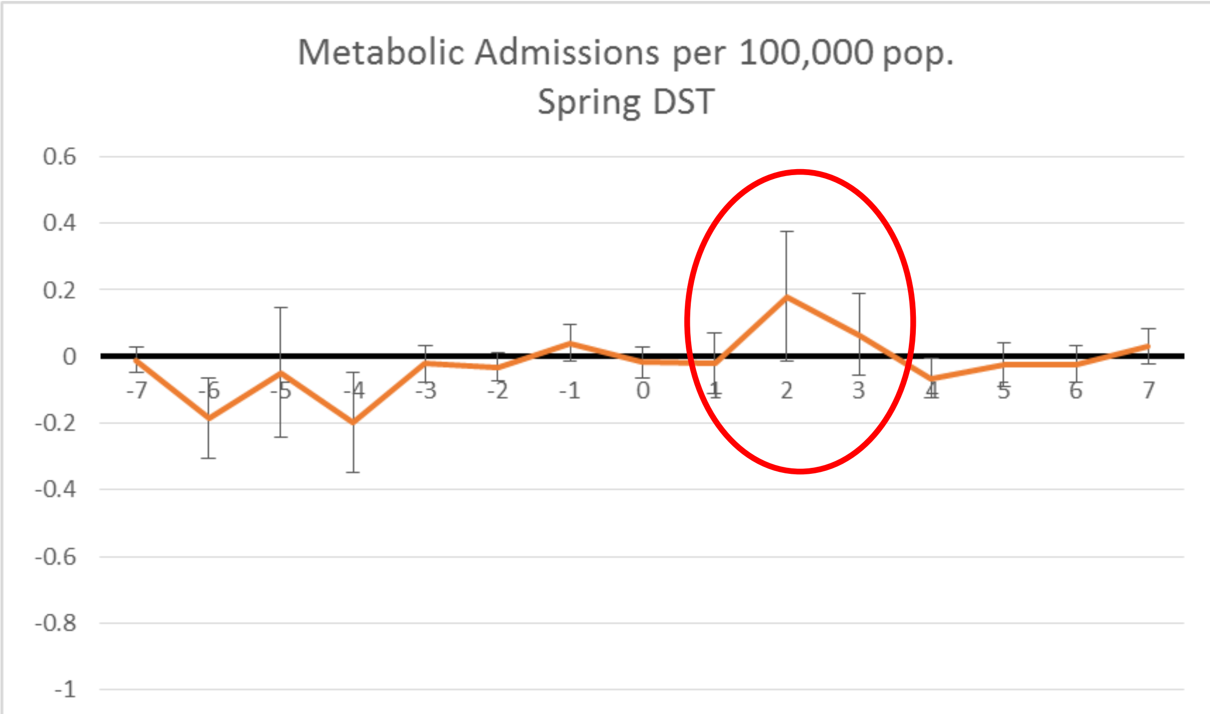


Figure B5a and b: Hospital Census Daily Approach

Daily Effects of DST on Metabolic Admissions, 2000-2008



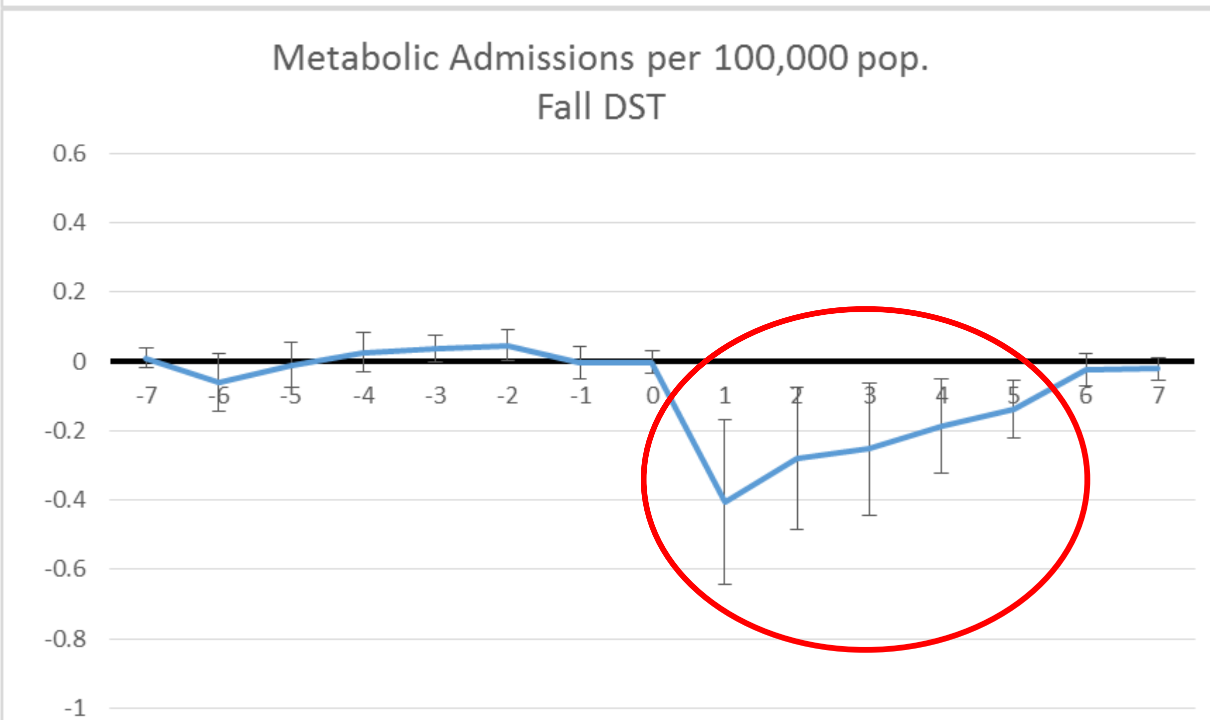


Figure B6a and b: Hospital Census Daily Approach

Daily Effects of DST on Neoplastic Admissions, 2000-2008

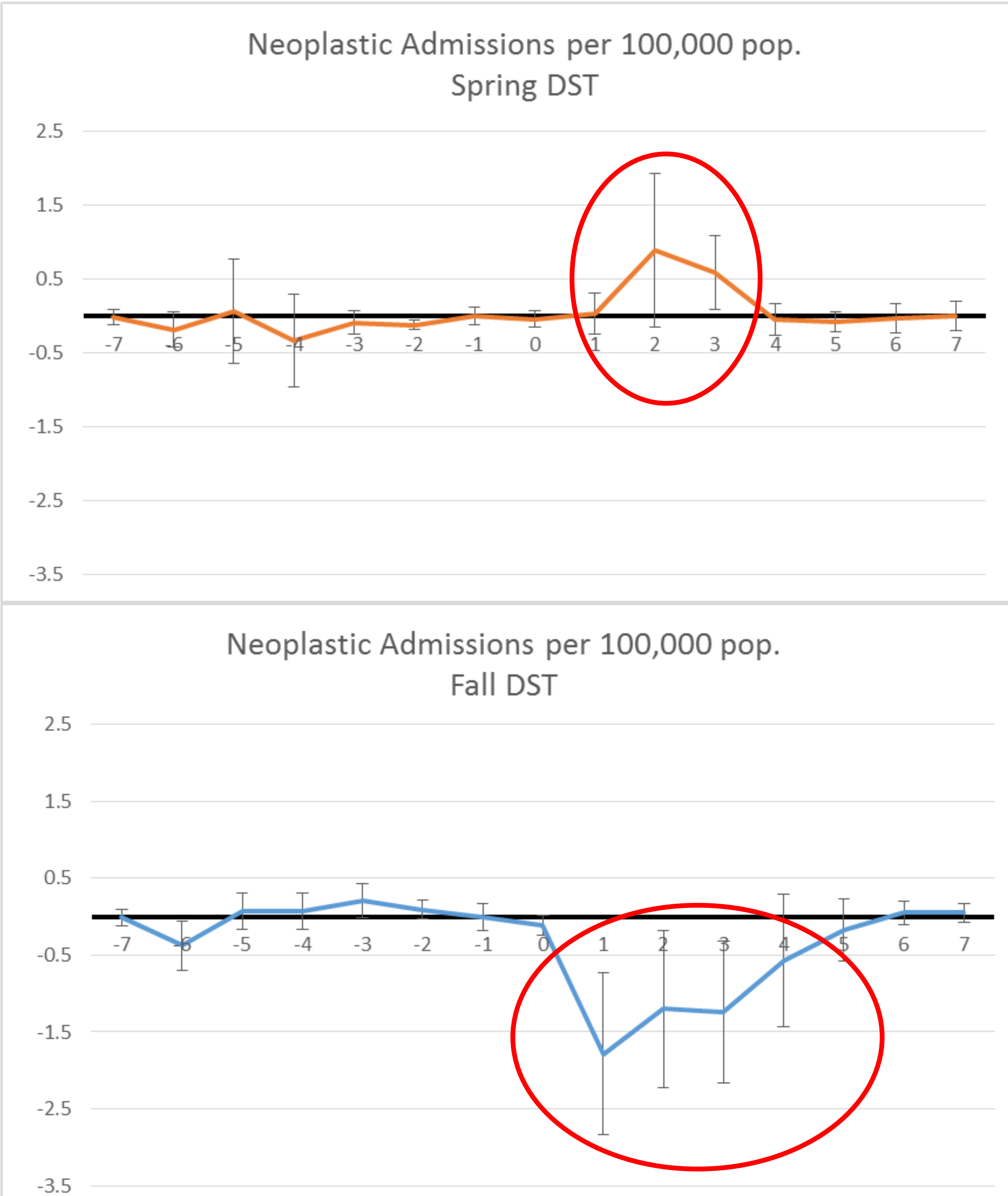
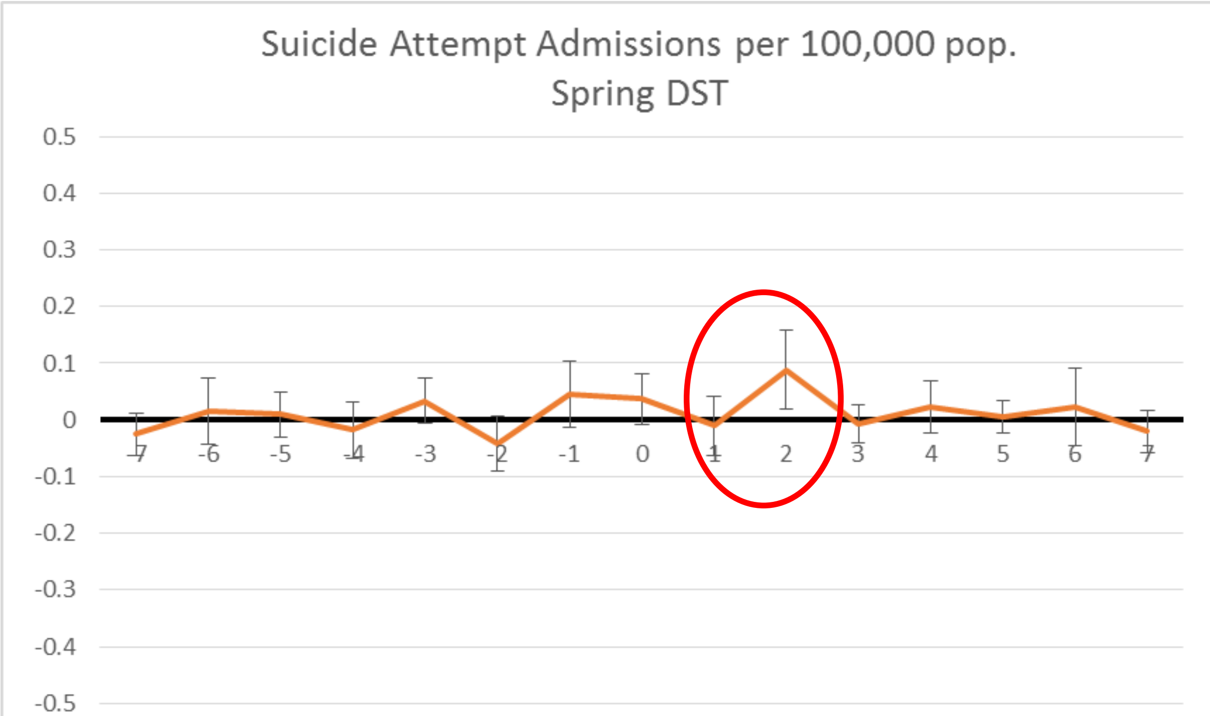


Figure B7a and b: Hospital Census Daily Approach

Daily Effects of DST on Suicide Attempts, 2000-2008



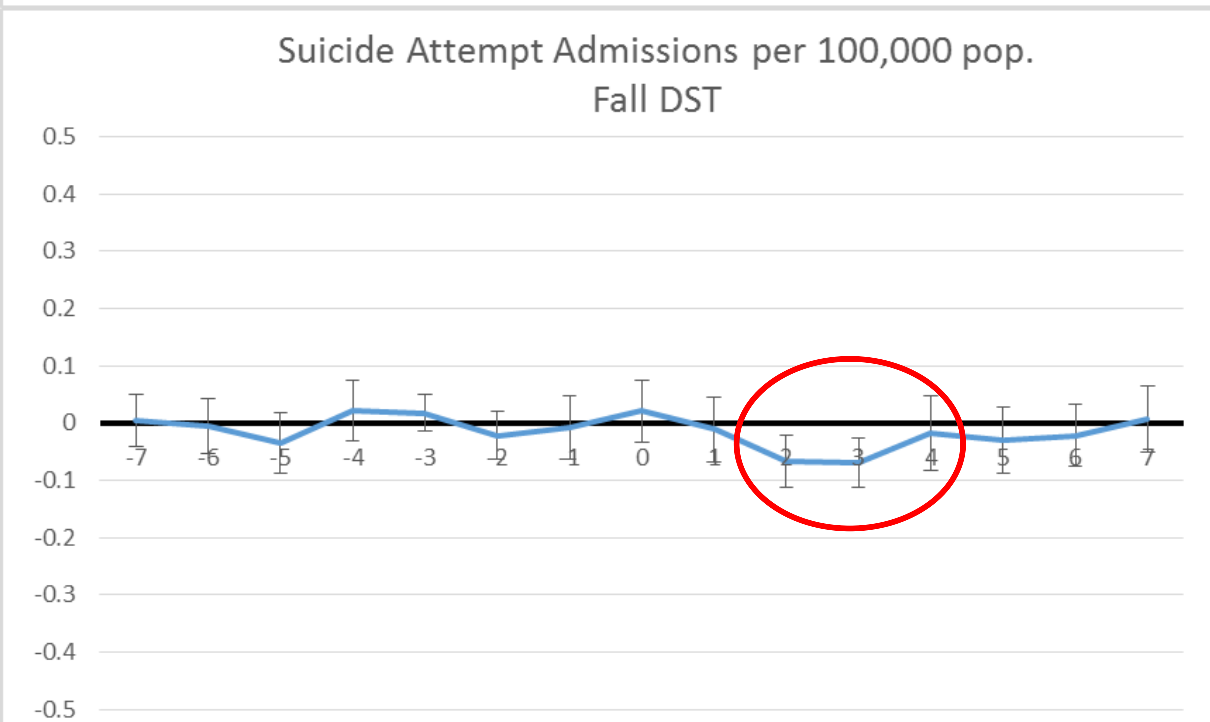
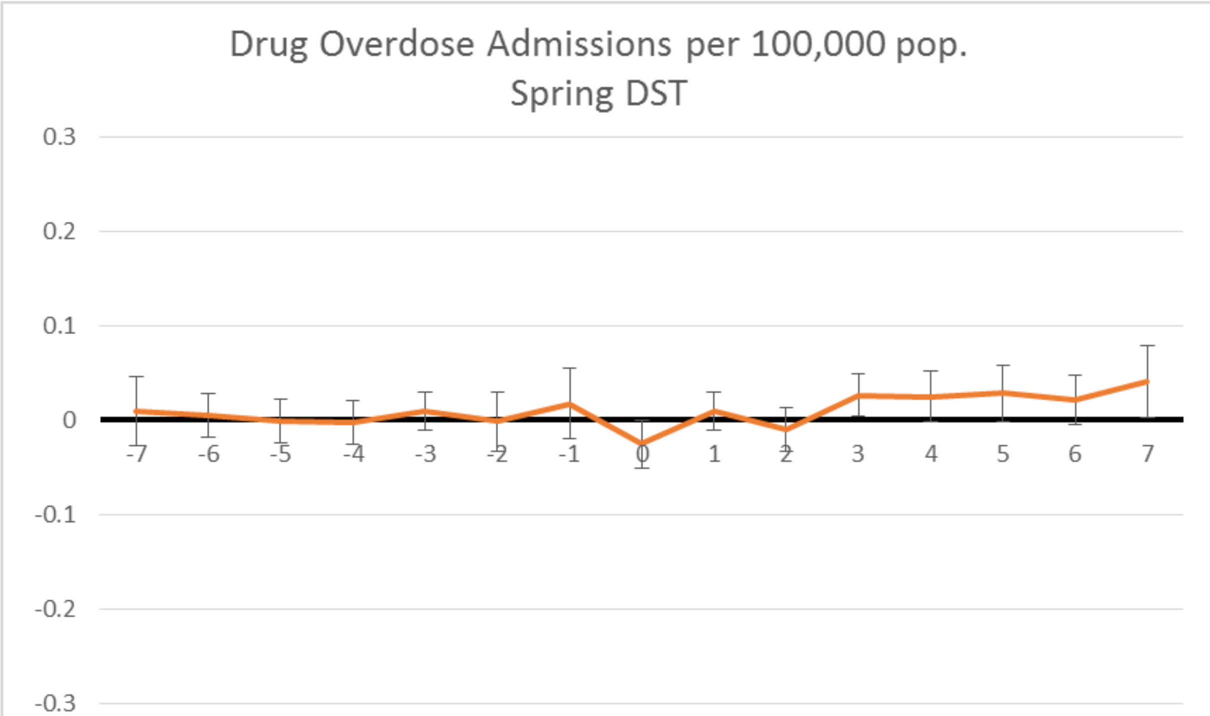


Figure B8a and b: Hospital Census Daily Approach
Daily Effects of DST on Drug Overdosing, 2000-2008



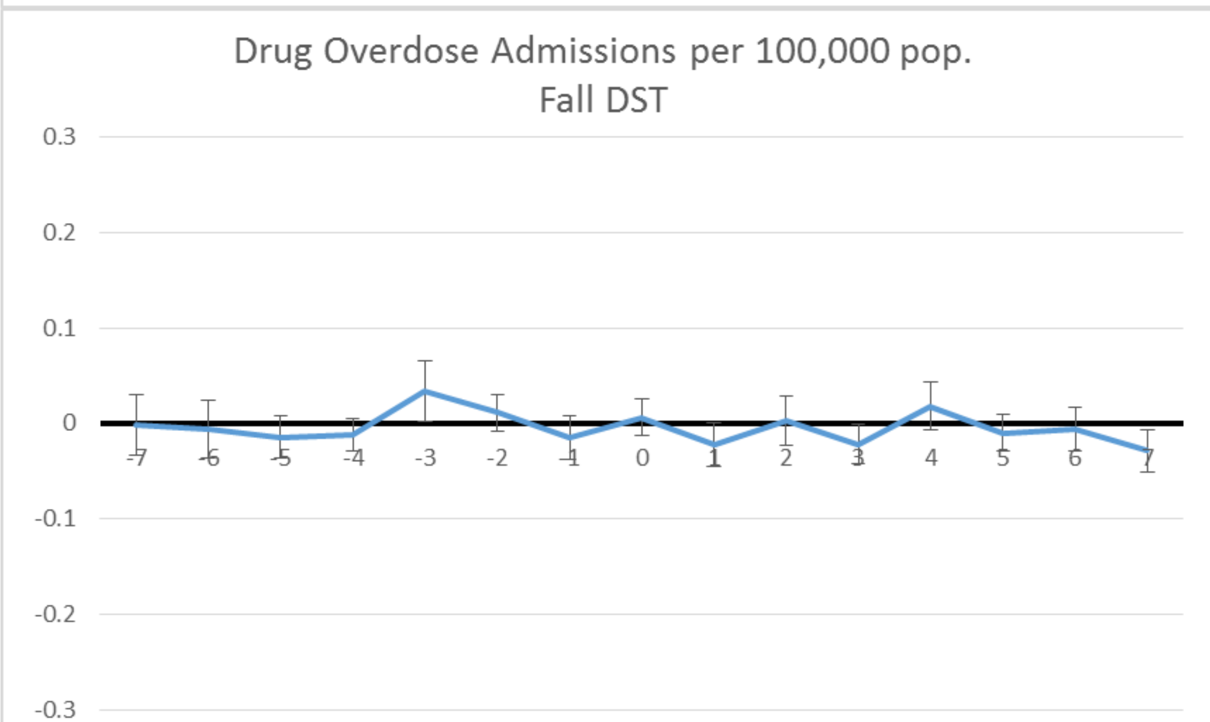


Figure B9a and b: Hospital Census Daily Approach

Daily Effects of DST on Infectious Admissions, 2000-2008

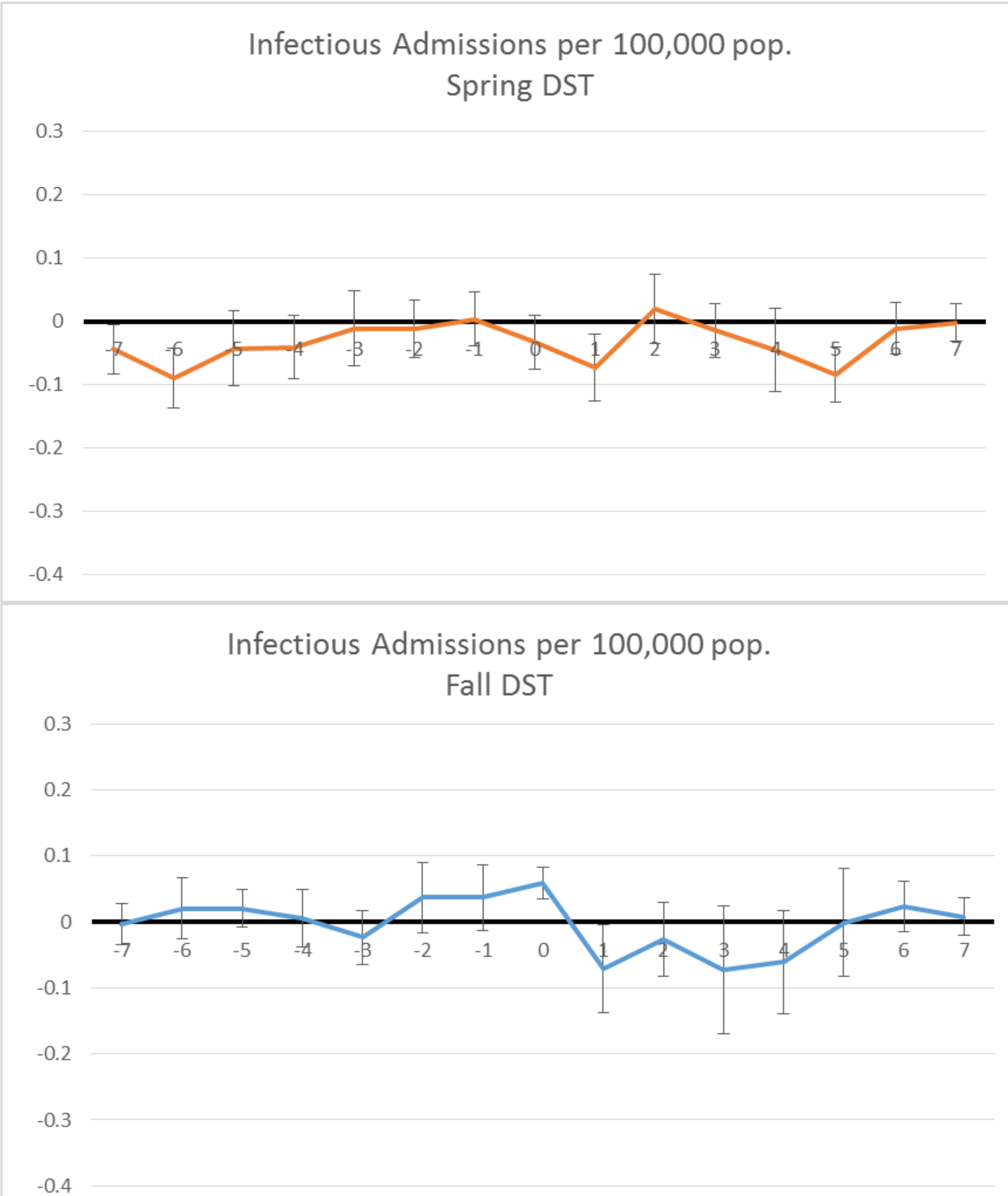
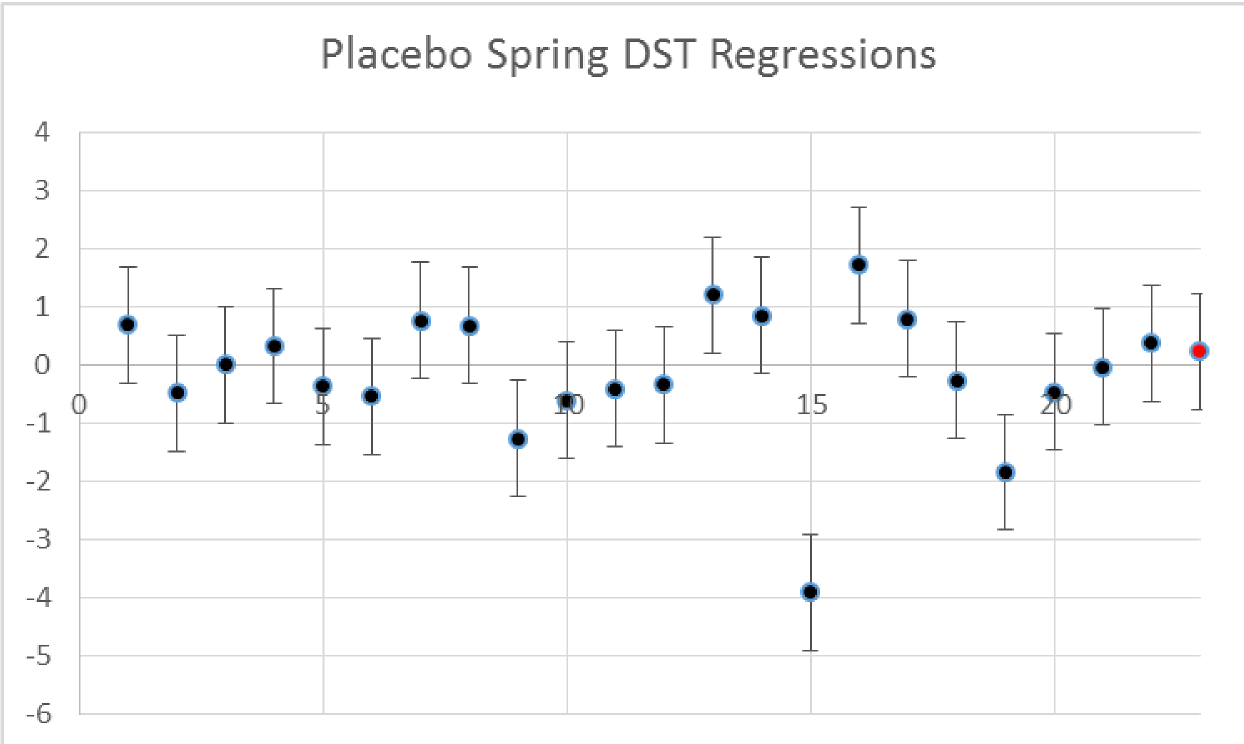
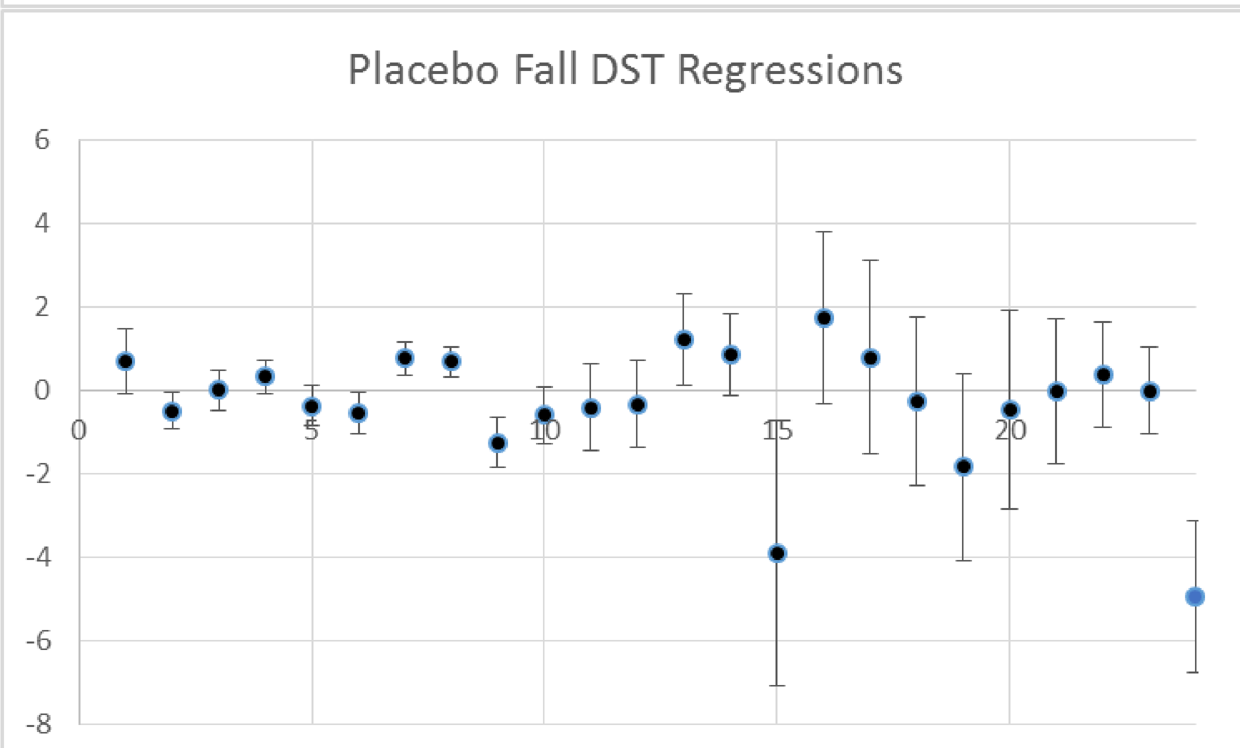


Figure B10a and b: Hospital Census Daily Approach

Daily Effects of DST in 22 Summer and 23 Winter Placebo Weeks, 2000-2008





Note: True DST week rightmost estimate.

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

	Mean	***Std.Dev***	***Min.***	***Max.***	***Obs.***
Dependent Variables					
Total admission rate per 100,000	59.7681	25.7333	N/A	N/A	336,604
Cardiovascular admission rate per 100,000	9.5339	4.9525	N/A	N/A	336,604
Heart attack admission rate per 100,000	1.5909	1.4035	N/A	N/A	336,604
Injury admission rate per 1 million	56.5571	26.6603	N/A	N/A	336,604
Respiratory admission rate per 100,000	3.9595	2.5850	N/A	N/A	336,604
Metabolic admission rate per 100,000	1.7351	1.5909	N/A	N/A	336,604
Neoplastic admission rate per 100,000	6.5951	5.0857	N/A	N/A	336,604
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 Admission 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 Weekly Approach:

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
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).