Spring Forward at Your Own Risk: Daylight Saving Time and Fatal Vehicle Crashes

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Abstract

Despite mounting evidence that Daylight Saving Time (DST) fails in its primary goal of saving energy, some form of DST is still practiced by over 1.5 billion people in over 60 countries. I demonstrate that DST imposes high social costs on Americans, specifically, an increase in fatal automobile crashes. DST alters fatal crash risk in two ways: disrupting sleep schedules and reallocating ambient light from the morning to the evening. First, I take advantage of the discrete nature of the transitions between Standard Time and DST to measure the impact of DST on fatal crashes in a regression discontinuity design. Then, to measure the duration of the effect, I exploit variation in the coverage of DST created primarily by a 2007 policy change, in a day-of-year fixed effects model. Both models reveal a short-run increase in fatal crashes following the spring transition and no aggregate impact in the fall. Employing three tests, I decompose the aggregate effect into ambient light and sleep mechanisms. I find that shifting ambient light reallocates fatalities within a day, while sleep deprivation caused by the spring transition increases risk. The increased risk persists for the first six days of DST, causing a total of 302 deaths at a social cost of $2.75 billion over the 10-year sample period, underscoring the huge costs of even minor disruptions to sleep schedules. *JEL* Codes: R41, I18, Q48

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1 Introduction

Daylight Saving Time (DST) in the US was originally implemented as a wartime measure to save energy and was extended as part of the Energy Policy Act of 2005. However, recent research demonstrates that DST does not save energy and could possibly increase energy use (Kellogg and Wolff, 2008; Kotchen and Grant, 2011). Despite mounting evidence that DST fails in its primary goal, some form of Daylight Saving Time is still practiced by over 1.5 billion people globally. In this paper I demonstrate that DST imposes high social costs on Americans, specifically, an increase in fatal automobile crashes. Employing three tests to differentiate between an ambient light or sleep mechanism, I show that this result is most likely due to sleep deprivation caused by the spring transition and the result implies additional costs of DST in terms of lost productivity nationwide.

The procedure for DST is well characterized by the phrase "spring-forward, fall-back." Each year on the spring transition date, clocks are moved forward by one hour, from 2 a.m. to 3 a.m. The process is then reversed for the fall transition with clocks "falling back" from 2 a.m. to 1 a.m. This alters the relationship between clock time and solar time by an hour, effectively moving sunlight from the morning to the evening (see Figure 1). The procedure was first suggested by George Vernon Hudson, an entomologist who wanted more light in the evenings to pursue his passion of collecting insects (Hudson, 1895). While the policy was first used during World Wars I and II, it has since become a peacetime measure. In all instances, the rationale has been that aligning sunlight more closely with wakeful hours would save energy used for lighting.[1] However, as Hudson's personal motivation for the policy suggests, DST has many impacts on practicing populations.

This paper focuses on a major side-effect of DST, its impact on fatal vehicle crashes. DST alters the risk of a fatal crash in two ways: disrupting sleep schedules and reallocating ambient light from the morning to the evening. With an average of over 39,000 annual fatalities, motor vehicle crashes are the number one cause of accidental death in the US (CDC, 2005-2010). Given the large base level of fatalities, even a small change in fatal crash risk is a potentially large killer. I identify the impact of DST on fatal crashes by taking advantage of (i) detailed records of every fatal crash occurring in the United

[1] DST is often mistakenly believed to be an agricultural policy. In reality, farmers are generally against the practice of DST because it requires them to work for an extra hour in the morning, partially in darkness, to coordinate with the timing of markets (Prerau, 2005).

States from 2002-2011; (ii) the discrete nature of the switch between Standard Time and Daylight Saving Time; and (iii) variation in the dates covered by Daylight Saving Time, created primarily by a 2007 policy change. I employ two different identification strategies. First, I use a regression discontinuity (RD) design that examines changes in daily crash counts immediately before and after DST transitions. Second, to measure the duration of impact, I use a day-of-year fixed effects (FE) model that is identified by dates that are covered by DST in some years but Standard Time in other years. In both specifications I find a 5.4-7.6% increase in fatal crashes immediately following the spring transition. Conversely, I find no impact following the fall transition when no shock to sleep quantity occurs.[2] To address the possibility that some other unobserved factor related to the transition dates is driving this result, I impose the pre-2007 transition dates on data from 2007-2011 and the current transition dates on data from 2002-2006 and find no impact of these dates when not associated with a policy change. I then examine the relative contribution of each DST mechanism.

Daylight Saving Time impacts practicing populations through two central channels. First, it creates a short-term disruption in sleeping patterns following the spring transition. Harrison (2013) surveys the sleep literature and finds that "increased sleep fragmentation and sleep latency" caused by the 23-hour spring transition date "present a cumulative effect of sleep loss, at least across the following week." Second, DST alters the relationship between clock time and solar time by an hour, creating darker mornings and lighter evenings than would be observed under Standard Time (see Figure 1).[3] Even this one hour shift in light can have major consequences; Doleac and Sanders (2013) find that increased ambient light in evenings reduces crime while Wolff and Makino (2013) suggest that it increases time devoted to exercise.

To parse out these mechanisms and determine what portion of the increase in fatal crashes is due to sleep loss versus reallocating light, I run three primary tests. These tests exploit differential timing in when each mechanism is active, both within and across days. First, I isolate the light mechanism by examining only the fall transition.[4] Then, I look at the difference between aggregate estimates in the fall (only the light mechanism) and spring (light and sleep mechanism) to determine the net impact of the

[2] Barnes and Wagner (2009) find that Americans sleep 40 minutes less on the night of the spring transition, but experience no significant change in sleep quantity on the fall transition.

[3] Since fatal crashes are more prevalent in the evening (Figure A1), it is possible that transferring light from a lower risk morning period to a higher risk evening period could lead to a net reduction in fatal crashes.

[4] Americans do not sleep a significant amount more on the fall transition date despite receiving an extra hour in the middle of the night (Barnes and Wagner, 2009).

sleep mechanism. Second, I isolate the sleep mechanism in the spring by examining a subsample of hours furthest from sunrise and sunset. These hours are least impacted by the light mechanism and a drowsy driver is presumably more at risk throughout the entire day, even in hours of full light or full darkness. Third, I compare the sleep impacted days of DST (up to the first two weeks) to the remainder of DST with common support.[5] All three tests suggest that the sleep deprivation is driving the increase in fatal crashes.

My preferred specification reveals a 6.3% increase in fatal crashes, persisting for six days following the spring transition. Over the 10-year sample period, this suggests the spring transition is responsible for a total of 302 deaths at a social cost of $1.2 to $3 billion, underscoring the huge costs of even minor disruptions to sleep schedules given the current sleep-deprived culture in the US.[6,7] The total costs of DST due to sleep deprivation could be orders of magnitude larger when worker productivity is considered (Wagner et al., 2012; Kamstra, Kramer, and Levi, 2000).[8]

This finding is timely, given the recent empirical research suggesting that DST does not reduce energy demand. Kellogg and Wolff (2008) use a natural experiment in Australia where DST was extended in some states to accommodate the Sydney Olympics. They find that while DST reduces energy demand in the evening, it increases demand in the morning with no significant net effect. Kotchen and Grant (2011) make use of quasi-experiment in Indiana where some Southern Indiana counties did not practice DST until 2006. Their work suggests that DST could actually increase residential energy use, as increased heating and cooling use more than offset the savings from reduced lighting use. For a failed energy policy to be justified from a welfare standpoint, the social benefits must outweigh the social costs. In this paper, I find a significant mortality cost that must be weighed against any perceived benefits of DST.

The remainder of the paper is organized as follows. The next section provides a brief background of DST in the US. Section 3 details the mechanisms through which DST influences crash risk, including reviewing existing evidence of the impact of DST on vehicle crashes. Section 4 introduces the data, highlighting the visual discontinuity in raw

[5]Common support refers to dates that are DST in some years and Standard Time in others.

[6]Social cost is based on Kniesner et al. (2012) value of a statistical life range of $4 to $10 million.

[7]Nearly 30% of American adults reported sleeping less than 6 hours per day in 2005-2007 according to a National Center for Health Statistics survey.

[8]There has been surprisingly little empirical research on the effects of sleep on worker productivity. Although fatal crashes are an extreme measure of productivity, driving is a behavior engaged in by over 90% of American workers (Winston, 2013) and the increase in fatal crashes suggests that sleep loss likely reduces productivity.

crash counts at the spring transition. Section 5 describes the RD and FE identification strategies, outlining the requirements for causal estimates. Section 6 presents results, including those that differentiate between the sleep and light mechanisms, and explores alternative explanations. Section 7 concludes with a brief summary and further remarks about the implications for DST as a policy.

2 Daylight Saving Time in the US

Daylight Saving Time has been a consistent feature in most US states since the Uniform Time Act of 1966.[9] This legislation allowed states to determine whether they practiced DST, but set uniform start and stop dates for any practicing states. Since 1966, Congress has twice made lasting changes to the DST transition dates, most recently as part of the Energy Policy Act of 2005. Starting in 2007, DST begins on the second Sunday of March and continues until the first Sunday of November, a 3-4 week extension in the spring and a 1 week extension in the fall.

Figure 1 illustrates the impact of DST on sunrise and sunset times throughout the year and highlights the 2007 extension. On the spring transition date, clocks skip forward from 2 to 3 a.m. pushing sunrise and sunset times back by one hour. In the fall, the process is reversed as clocks are adjusted back by an hour to facilitate the return to Standard Time. The 2007 extension to DST altered these transition dates and created an additional range of dates that are DST in some years and Standard Time in others.[10] In the next section, I discuss the primary mechanisms through which DST could influence fatal crash risk and how I disentangle the relative contributions of each.

3 Mechanisms

There are two mechanisms through which Daylight Saving Time could impact fatal crash risk. First, there is sleep loss associated with the spring transition when one hour in the middle of the night is skipped. Since sleep is a key factor in alertness and control (Smith, McEvoy, and Gevins, 2002), this sleep deprivation likely reduces driving safety. In a study of 400 U.S. Army soldiers, Legree et al. (2003) find a correlation of 0.20 between driver at fault accidents and self reported insufficient sleep. Second, DST shifts

[9] Among the contiguous US, all states but Arizona and parts of Indiana have practiced DST since 1973.

[10] Since transition rules are based on moving dates (e.g. the second Sunday of March ranges from 3/8 to 3/14) there is variation in start and end dates even within a particular transition rule.

the mapping of solar time to clock time by an hour, reallocating sunlight between the morning and the evening. Ambient light reduces fatal crash risk (Fridstrom et al., 1995; Sullivan and Flannagan, 2002), and this reallocation of light within a day creates riskier morning driving conditions and less risky evening driving conditions during DST.[11] I next discuss each mechanism individually, outlining its likely effect on fatal crashes and reviewing existing evidence of its impact through DST.

3.1 Sleep Mechanism

The spring transition into DST is facilitated by clocks jumping forward from 2 a.m. to 3 a.m. on the transition date. This creates a 23-hour transition day, rather than the standard 24-hour days people are accustomed to. While this "missing" hour could be cut from work or leisure time, Barnes and Wagner (2009) find that Americans make up the majority of the missing time by sleeping less. Using the American Time Use Survey, they find Americans sleep an average of 40 minutes less on the night of the spring transition. Depending on the individual, this transition could impact sleep patterns for anywhere from two days to two weeks (Valdez et al., 1997) with an average of about one week (Harrison, 2013).

In the fall, the opposite scenario occurs with a 25-hour transition day. However, in this case, Americans use very little of the extra hour for sleep, sleeping a statistically insignificant extra 12 minutes (Barnes and Wagner, 2009). This creates variation in treatment status for the sleep mechanism. The spring transition is treated (sleep loss), while the fall transition is untreated (insignificant change to sleep quantity).[12]

Previous research on the sleep impact of DST on vehicle crashes has been mixed. Coren (1996) and Varughese and Allen (2001) find an increase in crashes on the Monday following the spring transition into DST, while Sood and Ghosh (2007) and Lahti et al. (2010) suggest no effect. By focusing on one day, these tests can lack power and often cannot rule out a wide range impacts. In contrast to these studies, I gain statistical power by testing for a longer term sleep impact consistent with recent literature on sleep disruptions.

Additionally, these previous studies use data centered in 1992, 1985, 1987 and 1994 respectively. Average sleep quantity has been on the decline in the US, a phenomenon also seen in the lower tail of the distribution. According to the National Sleep Foun-

[11] When switching out of DST in the fall, the mornings become less risky and evenings more risky than under DST.

[12] Sexton and Beatty (2014) also find significant sleep loss associated with the spring transition but no significant change in the fall.

dation, the percentage of Americans averaging less than 6 hours of sleep has risen from 12% in 1998, to 20% in 2009. My data spans 2002-2011 and should generate a more up to date measure of the impact of sleep loss given the current sleep patterns in the US.

3.2 Light Mechanism

Despite strong evidence suggesting the importance of ambient light in fatal crash risk, the implication for net crashes due to Daylight Saving Time remains unclear. DST does not alter the amount of light in a day, it simply reallocates it between the morning and the evening. Since fatal crashes are more prevalent in the evening (Figure A1), it is possible that transferring light from a lower risk morning period to a higher risk evening period could lead to a net reduction in fatal crashes.

Previous studies by Ferguson et al. (1995) and Broughton, Hazelton, and Stone (1999) examine the light mechanism by estimating the impact of ambient light on fatal crash risk directly, and then simulating the impact of imposing DST light levels on the rest of the year. Both studies suggest a reduction in fatal crashes through this mechanism.[13] However, the simulation in Ferguson et al. (1995) uses a single measure of the impact of light on crash risk. This generates a biased estimate of the life saving potential of DST if ambient light interacts with other risk factors such as driver alertness, or type of trip (work versus leisure) both of which are likely to vary from morning to evening driving. Further, simulation requires assumptions about driver behavior under counterfactual hours of light.

As an alternative to these simulation methods, I use empirical techniques to estimate the effect directly. First, I focus on the fall transition as a clean estimate of the light mechanism because it is not afflicted by any significant shock to sleep. Then, I examine the spring following the first two weeks of DST, when the sleep mechanism should no longer be active.

4 Data

4.1 FARS

For vehicle fatality data, I use the Fatality Analysis Reporting System (FARS), compiled by the National Highway Traffic and Safety Administration. These data contain

[13] Sood and Ghosh (2007) also find a reduction in crashes which they attribute to the light mechanism. However, they analyze only the spring transition and results are sensitive to the time frame analyzed and choice of control group.

a record of every fatal crash occurring in the United States since 1975, including exact time and location of the accident. I focus on recent crashes, from 2002-2011, allowing for five years on either side of the 2007 DST extension. Consistent with other DST literature, my sample is the continental US excluding Arizona and Indiana because at least part of those states did not practice DST consistently over the entire sample time frame.[14] Since the initial Sunday of DST is 23 hours long, whereas other days are 24 hours long, I adjust the crash count by counting the 3-4 a.m. hour twice, using it as a proxy for the missing 2-3 a.m. hour. For the 25-hour fall transition date, I divide the fatalities occurring from 2-3 a.m. by two, because this hour occurred twice.[15]

My dependent variable in all specifications is the natural log of the number of fatal crashes occurring on a given day at the national level. I aggregate to the national level due to the relative rarity of fatal crashes. There are roughly 100 fatal crashes per day across the entire US and the mode for daily crashes at the state level is zero. Aggregating allows me to gain statistical power and smooths out potential confounders such as weather which could drive results in some states or even regions, but likely not the entire US.

Figure 2 plots the total number of fatal crashes occurring in the weeks surrounding the spring transition into DST. There is a clear break in the seasonal trend of fatal crashes, occurring right at the spring transition.[16] This provides suggestive evidence that the spring transition is associated with a short term increase in fatal crashes. My initial estimation strategy (RD) formally tests for this discontinuity.

If complete data were available for less severe crashes, it could be analyzed in the same identification framework I propose. However, many states do not maintain a uniform database of these less severe crashes and the potential for reporting bias and less rigorous redundancy checks for non-fatal crashes make these data less reliable. Considering only fatal crashes is likely a lower bound on the impact of DST on all automobile crashes.

4.2 Other Data Sources

Fridstrom et al. (1995) find "exposure to risk" or Vehicle Miles Traveled (VMT) to be the most important predictor of fatal crash counts. Unfortunately, daily VMT data does not exist at the national level. As such, I use VMT data from Caltrans' Performance

[14] Less than 1% of the remaining observations are dropped due to missing or inaccurate time of day.

[15] I also use two alternative corrections, multiplying crashes on the spring transition date by 24/23rds and those on the fall transition date by 24/25ths, or simply dropping the transition dates from the sample. Results are robust to both methods.

[16] The seasonal trend is largely due to a similar seasonal increase in vehicle miles traveled.

Measurement System (PeMS) to examine whether adjustments to VMT are driving my results. To the extent that VMT on this subset of roads is representative of US driving patterns, this provides a useful test. In the national sample, I use weekly gasoline prices from the U.S. Energy Information Administration and the value of the S&P 500 index to help control for fuel prices and driving patterns.

5 Empirical Strategy

5.1 Regression Discontinuity (RD) Methods

The goal of the empirical analysis is to identify the impact of DST on fatal motor vehicle crashes. To perform this analysis, I use a regression discontinuity design that exploits the discrete change from Standard Time to DST. Every year on the spring cutoff date, clock time is altered by one hour. If there is a significant impact of DST on fatal crashes, there should be a shock to the number of fatal crashes from just before to just after the transition. Measuring the discontinuity occurring at the policy transition provides an estimate of the policies immediate impact.

My preferred specification uses local linear regression, as it has been shown to perform better in RD settings than high order polynomials of the running variable (Gelman and Imbens, 2014).[17] To eliminate persistent day-of-week effects (e.g. crashes are higher on weekends than weekdays) and long-term time trends, I first demean the logged crash counts by day-of-week and year. Then, I use the standard RD specification with the demeaned crash data. The estimation equation is seen below:

$$\ln Fatals_{dy} = \beta_0 + \beta_1 DST_{dy} + \beta_2 DaysToTran_{dy} + \beta_3 DST_{dy} * DaysToTran_{dy} + \varepsilon_{dy} \quad (1)$$

DST_{dy} is an indicator equal to one if day d in year y falls under Daylight Saving Time and $DaysToTran_{dy}$ is the running variable, measuring time in days before and after the DST transition. $DaysToTran_{dy}$ is centered at the transition date in each year, the first Sunday of April in 2002-2006 and the second Sunday of March in 2007-2011. The coefficient of interest, β_1, is the aggregate effect of DST on vehicle fatalities at the cutoff date.[18]

My baseline specification uses Calonico, Cattaneo, and Titiunik's (2012) optimal

[17] Results using a global polynomial are qualitatively identical and are available in appendix Table A2.

[18] I refer to this as the aggregate impact, because it does not yet disentangle the DST mechanisms.

bandwidth selector to determine how many days to use on either side of the DST transition and a uniform kernel. As Imbens and Lemieux (2008) argue, there is little practical benefit to other weighting schemes as they are primarily indicative of sensitivity to the bandwidth choice. For robustness I include results using alternative bandwidth selectors and Epanechnikov and triangular kernels.

In this context, a consistent estimate requires that conditional on day of the week and year, the treated and untreated number of fatal car crashes must vary continuously with date around the transition. Stated differently, if all other factors affecting fatal crash risk, besides DST, are continuous at the transition date, the RD design will provide consistent estimates of the effect of DST. Figures 4 and 5 begin to speak to this assumption, providing visual evidence that after demeaning the data, fatal crashes vary smoothly across a year. In Section 6.5, I directly test for discontinuities in other factors that impact crash risk.

The Energy Policy Act of 2005 allows me to further probe the robustness of my RD estimates in a difference in discontinuities placebo test. The new March transition date went into effect in 2007 and should have no impact in previous years. Likewise, the old April transition date should not impact crashes in 2007-2011. By looking for a discontinuity using these placebo transition dates, I can test whether these dates are typically associated with a change in fatal crashes, unrelated to DST. I apply the analogous procedure to the fall transition.

5.2 Day-of-Year Fixed Effects

While the RD design provides a measure of the causal impact of DST on fatal crashes at the transition date, it is more limited in estimating longer term impacts. To empirically estimate these longer lasting effects, I leverage variation in the coverage of Daylight Saving Time created by both the 2007 extension and the DST cutoff rules. From 2002-2006 the time period between the second Sunday of March and the first Sunday of April was part of Standard Time. The Energy Policy Act of 2005 extended DST to cover this 3-4 week period in 2007-2011. This creates a range of dates that are DST in some years and Standard Time in other years. The cutoff rule further expands the number of "switching days". Consider the current decision rule where DST begins on the second Sunday in March. The start date has varied from the 8th to the 14th of March depending on the year.[19] Figure 3 shows days of the year that fall under both DST and Standard

[19] For example, March 11th is Standard Time in 2002-2006, 2010 and 2011 but is DST in the years 2007-09.

Time during the spring and their frequency under each regime. During the fall there is a similar, but smaller, region of switching dates because the fall transition date was only pushed back by one week.

Moving to a fixed effects framework, I run the following specification to take advantage of this variation in DST assignment:

$$\begin{aligned} \ln Fatals_{dy} \quad = \quad & \beta_0 + \beta_1 SpDST_{dy} + \beta_2 FaDST_{dy} + DayofYear_d \\ & + DayofWeek_{dy} + Year_y + V_{dy} + \varepsilon_{dy} \end{aligned} \tag{2}$$

$DayofYear_d$ is a separate dummy for each day of the year, flexibly controlling for the impact of seasonality on fatal crashes.[20] $DayofWeek_{dy}$ and $Year_y$ are day-of-week and year dummies respectively. V_{dy} is a vector of controls used in some specifications, including gasoline prices, the value of the S&P 500 index and non-stationary holidays. $SpDST_{dy}$ is an indicator equal to one if the date falls under DST and is covered by the range of spring switching dates (March 8th - April 7th). Analogously, $FaDST_{dy}$ is an indicator equal to one if the date falls under DST and is covered by the range of switching dates in the fall (Oct 25th - Nov 7th). These are the coefficients of interest and are interpreted as the average effect of DST on fatal crashes over the "switching" dates in that season.

Note, that β_1 here is a different parameter from what is found using the RD design. Regression discontinuity estimates the effect of DST right at the spring transition, whereas the fixed effects specification measures the average effect of DST over all dates that are sometimes DST and sometimes Standard Time during the spring. If DST only creates a short-run effect through sleep deprivation, this should be picked up in the RD, but would be averaged out across the full range of switching dates when using the fixed effects model. Likewise, β_2 is the average effect of DST across the roughly two weeks of fall switching dates, rather than the effect of leaving DST in the fall.

Beyond identifying the average effect of DST across the range of switching dates, this specification can aid in disentangling the mechanisms. I isolate the light mechanism in the spring, by focusing only on dates at least two weeks following the transition, at which time any sleep impact should have dissipated. Comparing this light impact to the initial impact from light and sleep provides another measure for just the sleep impact.

[20] I create dummies for each month/day combination (e.g. an August 25th dummy). This is slightly different than creating a dummy for the 100th day of the year, because leap day would cause August 25th for most years to be matched with August 24th for 2004 and 2008. I use the month/day method as it better aligns with holidays and generates more conservative estimates.

6 Results

6.1 Spring RD Design

Figure 4 illustrates the regression discontinuity strategy for estimating the impact of DST on fatal crashes. The average residuals from a regression of log(daily fatal crash count) on day-of-week and year dummies are plotted, centered by the spring transition date. The plot follows a gradual arc demonstrating the seasonal pattern in fatal crashes, where crashes rise from winter lows, peaking in late summer before dropping again through the fall. If DST has an impact on fatal crashes, this should be evident in a trend break right at the transition date. Visually, there is a short-term spike in fatal crashes before the residuals resume the seasonal trajectory.

Table 1 shows the corresponding regression estimates.[21] The spring transition into DST is associated with a 6.3% increase in fatal crashes. This result persists using the bandwidth selectors of Imbens and Kalyanaraman (2012) and the cross-validation method of Ludwig and Miller (2007) seen in columns 2 and 3 respectively. To test whether the increase is due to one particular transition rule, I split the data into an early subsample (2002-06) that was subject to the April transition, and a late subsample (2007-2011) that is subject to the current March transition. While cutting the sample in half reduces precision, both time periods experience similar increases in fatal crashes at the transition.[22]

To address the possibility that both transition dates are associated with an increase in fatal crashes, unrelated to DST, I run the following placebo test in column 6. I assign the current transition date to 2002-2006 data and the old transition date to the 2007-2011 data. Running the same RD strategy measures the impact of these transition dates in years where there was no actual shift between Standard Time and DST on these dates. If these dates, rather than DST are responsible for the increased crash counts, this test should reveal a similar increase in crashes to those seen in columns 1-5. The zero result in column 6 suggests that the increase in crashes is not simply due to the

[21] Clustering by week or year tends to *decrease* standard errors as the shocks are negatively correlated, so I report the more conservative uncorrected standard errors.

[22] Due to small sample size (pedestrian and pedacycle accidents account for only 15% of my sample), I am unable to address the question of whether pedestrians, or school-children in particular, would experience an even larger increase in the risk of being hit by a vehicle due to the darkened mornings of DST. Using the same RD design on this limited sample yields imprecise point estimates of similar magnitude to those using the full sample.

transition dates, but due to the actual policy.[23]

To address the concern that my results are driven by how I adjust the crash count for the transition date, I run two additional specifications. First, I follow the method used by Janszky et al. (2012) and multiply the the crash count on the transition date by 24/23rds to calibrate for the shorter time period. Alternatively, I throw out the transition date altogether. In both cases, results are qualitatively identical to my main specification (see Table A1). The remainder of Table A1 shows that results are robust to alternative kernel choice, while Table A2 shows they are robust to using a global polynomial RD design. Overall, these results demonstrate that spring transition into DST is associated with a significant increase in fatal crashes. Now I turn to the fall transition to test whether there is an analogous reduction in crashes when leaving DST.

6.2 Fall RD Design

Figure 5 illustrates the regression discontinuity strategy for the fall. In contrast to the spring, the residual plot looks quite smooth as it crosses the fall transition date. Table 2 presents the corresponding regression results. Just as the residual plot suggests, the preferred specification in column 1 indicates no significant change in fatal crashes associated with leaving DST. This result is robust to alternative bandwidths (columns 2-3) and splitting the sample into just the old October or current November transition date (columns 4-5). Using an analogous placebo test to that used in the spring suggests that these transition dates do not systematically alter crash risk independent of a policy. Taken as a whole, the transition from DST back to Standard Time does not reduce fatal crash risk in the same way entering DST increases risk. I now turn back to the mechanisms through which DST could impact crash risk to explain this asymmetric effect.

6.3 Mechanisms

The spring transition is subject to both the light and sleep mechanism. Hence, the 6.3% increase in fatal crashes could be partially due to each mechanism. The most parsimonious method for decomposing this result into each mechanism uses only aggregate results from the spring and fall. Given the fall transition is not subject to any change in sleep quantity, it isolates the light mechanism. The aggregate effect of zero when leaving DST in the fall suggests no net impact of DST through the light mechanism. Differenc-

[23] The negative point estimate would suggest that, if anything, my results understate the true impact of the spring transition into DST.

ing the spring estimate of 6.3% (light and sleep mechanism active) and the fall estimate of zero (only light mechanism active) provides suggestive evidence that the impact of the spring transition should be attributed solely to the sleep loss mechanism.[24] However, differences in sunrise and sunset times and the potential for differences in driver behavior between the spring and fall transitions prevent this from being an ideal comparison. To further disentangle the mechanisms, I use the initial RD framework with sub-samples of hours selected to isolate the impact of one mechanism or the other.

6.3.1 Light

Since only the light mechanism is active during the fall, the aggregate fall effect of zero suggests no net impact through this channel. To determine if light has become altogether unimportant as a fatal crash risk factor, perhaps through improved vehicle lights, I further explore the light mechanism by examining sub-samples of hours closest to sunrise and sunset.[25] Upon leaving DST in the fall, an hour of light is removed from the evening and returned to the morning. If light remains an important fatal crash risk factor, additional morning light should create a safer atmosphere for driving during morning hours. Likewise, removing light from the evening should create a more dangerous driving atmosphere during this time. To test this hypothesis, I break the sample into a set of morning hours (4-9 a.m.) and evening hours (3-8 p.m.). Then I run the initial RD analysis on these subsamples for the fall transition. If light remains an important factor in fatal crash risk, leaving DST should lead to fewer morning crashes (more light) and additional evening crashes (less light). If no change in crashes is seen, it is likely that light no longer plays an important role in fatal crashes. Table 3 details the results.

Across different bandwidths, leaving DST is associated with a significant reduction in fatal crashes during the morning (more ambient light). Conversely, evening hours (less ambient light) are always associated with a significant increase in fatal crashes. These results suggest that light still plays an important role in fatal crash risk. However, the aggregate zero effect (Column 1) suggests these impacts balance out and light has no

[24] The aggregate estimates for leaving DST tend to be positive (though insignificant). By symmetry, if leaving DST increases fatal crash risk this implies that entering DST reduces fatal crash risk. Hence, if anything, the light mechanism reduces crashes during DST (as suggested by Broughton, Hazelton, and Stone (1999) and Ferguson et al. (1995)). As such, the 6.3% increase in crashes in the spring is, if anything, a downwardly biased estimate of the sleep mechanism.

[25] Since 2003 BMW, Toyota and others have released vehicles with Adaptive Front-Lighting Systems (AFS). AFS are designed to optimize headlight direction and volume in response to steering, ambient weather, visibility conditions and speed.

net impact through DST. Crashes are simply reallocated between the morning and the evening. This reallocation can be seen more clearly in the kernel density function in Figure 6.

6.3.2 Sleep

The spring transition is subject to both the sleep and light mechanisms. However, my estimates for the fall transition suggest that the net impact of the light mechanism is zero. Taking a closer look at the spring residual plot in Figure 7 provides a clearer picture of what is occurring right at the spring transition. There is a discontinuous jump in fatal crashes that seems to persist for the first six days of DST, before jumping back down to essentially the same seasonal path seen during Standard Time. Since the light mechanism is in effect for the entire period of DST, this data pattern is inconsistent with a light impact – we would not expect the crash count to jump back down. However, a shock to sleep should only be felt in the initial period following the transition, before dissipating – exactly the phenomena seen here.

To pry further at the sleep mechanism, I focus on a sub-sample of hours furthest away from sunset and sunrise to mitigate the light impact.[26] Figure 8 illustrates the discontinuity while Table 4 provides the regression results. The point estimates are quite similar to the full day impacts and are significant using two of the three bandwidth selectors. This suggests that it is the sleep mechanism, not light, that causes the short-run increase in fatal crashes following the spring transition. To further investigate the mechanisms and to determine the length of this sleep impact, I turn to the fixed effects model.

6.4 Fixed Effects Model

Table 5 presents the results from the FE model. The point estimates represent the average impact of DST over the full range of switching dates (dates that are DST in some years and Standard Time in others), rather than just at the threshold. While the initial columns examine the spring DST period as a whole, columns 3-7 break spring DST down into three components (i) the first six days of DST, where the sleep effect should be felt most strongly;[27] (ii) the next eight days of DST, the longest any sleep

[26] I say "mitigate" not "eliminate" because the angle of the sun and moon are still altered even in these hours of full light and full darkness.

[27] I choose six days based on the appearance of the residual plot seen in Figure 7. This covers the Sunday-Friday following the spring transition and is consistent with the literature on how long DST impacts sleeping patterns.

study suggests a sleep impact could persist; and (iii) the remainder of spring DST with common support, days in which only the light mechanism should remain present.

Beginning with the entire spring period, column 1 shows that spring DST is associated with a significant 3.4% increase in fatal crashes over the roughly one month of switching dates. The fall estimate is insignificant from zero, again suggesting no impact of DST in the fall.[28] In addition to day-of-year fixed effects, column 1 uses just day-of-week and year dummies, the same controls used in the RD design. Column 2 includes additional covariates for nonstationary holidays, gasoline prices and the value of the S&P 500 index.[29] Results are quite stable across columns and continue to suggest that DST causes a significant increase in crashes during the spring and has no effect during the fall.

Turning to columns 3-4, the results are broadly consistent with a sleep impact that diminishes further from the spring transition and no net impact from reallocating light. The first six days of DST experience a significant 5.6% increase in fatal crashes, quite similar to the 6.3% increase found in the RD design. The point estimate shrinks to an insignificant 2.9% during the next eight days and diminishes further to 1.8% for the remainder of the spring. During both time periods in which only the light mechanism is active, the fall and the spring following the first two weeks, there is no significant change in crash counts. Including additional controls in column 4 to help proxy for the character and amount of vehicle miles traveled leaves results qualitatively identical.

Columns 5-7 explore these impacts across different times of day, reinforcing previous findings regarding the sleep mechanism. Column 5, uses just the subsample of hours least effected by the light mechanism, effectively isolating the sleep mechanism. The 4.8% increase in crashes during the first six days of DST provides a measure of the impact of just the sleep deprivation mechanism on crashes during these hours. Across each subsample of hours, the point estimates drop from the first six days of DST to beyond the first two weeks of DST in the spring. This suggests that across all hours, mitigating the sleep mechanism reduces fatal crash risk. Overall, the body of evidence from the FE model aligns with that found from the RD model. There is a significant short-term increase in fatal crashes following the spring transition, consistent with a detrimental impact of sleep loss. Now I turn to plausible alternative explanations for this short-term spike in fatal crashes.

[28]The fall estimates are less precise because there was only a 1-week extension to DST in the fall, providing fewer switching dates than in the spring.

[29]Adding each additional covariate individually leaves results qualitatively identical.

6.5 Alternative Explanations

A key omitted variable in this analysis and previous studies is Vehicle Miles Traveled (VMT). If VMT increases at the DST transition date, this behavioral change could be driving results rather than sleep loss. While national VMT data is not available, the Performance Measurement System (PeMS) in California tracks VMT on many major highways within the state. Using the same regression discontinuity model from equation 1 with log(VMT) as the dependent variable yields an insignificant 0.016% increase in VMT. To the extent that driving habits on these California roadways are representative of national driving patterns, this suggests VMT is not the cause of increased crashes.

Adverse weather conditions increase the risk of fatal crashes (Fridstrom et al., 1995). Although weather is a pseudo-random phenomena, if adverse weather occurred just following the spring transition, this could lead to the short-term increase in fatal crashes. Using a FARS variable that indicates weather conditions at each fatal crash, I create a variable for the ratio of crashes within a day that are impacted by weather. Using the regression discontinuity model from equation 1 with weather-ratio as the dependent variable I find an insignificant 1.2 percentage point decrease in weather related crashes.[30]

This analysis suggests that some of the most likely alternative pathways cannot explain the increase in fatal crashes. Further, if the increase is due to adjusting to a new schedule, the same increase should occur immediately following the fall transition, a phenomena that we do not see. While this is not an exhaustive list of competing explanations, the balance of evidence points strongly towards DST increasing fatal crash risk, through the mechanism of sleep deprivation. In the next section, I explore whether this result varies by region.

6.6 Geographical Heterogeneity

At the national level, the spring transition into DST leads to a significant increase in fatal crashes. However, this could be due to a constant treatment effect where all regions experience the same 6% increase in crashes, or a heterogeneous treatment effect where some regions experience a larger increase and others experience little or no effect. In this section, I explore two pathways through which geography could lead to heterogeneous impacts of DST, one through the sleep mechanism and the other through the light mechanism.

[30] The residual plots and regression output for both of these "alternative explanations" are available in the appendix.

Sleep deprivation could be more detrimental when driving in already dangerous area. If there are more situations where a delayed response can lead to a crash, the sleep mechanism has more scope to operate. To test this hypothesis, I split my sample in two based on the median number of fatal crashes per capita in each county.[31] The counties with a higher per capita fatal crash rate, I refer to as high risk counties. Running the RD analysis with these subsamples (Table A4) provides weak evidence that high risk counties are subject to a larger initial increase in fatal crashes (in percentage terms) than their low risk counterparts. While the estimates may not be statistically different at conventional levels, in all cases the point estimate for high risk counties is above that of low risk counties. This provides suggestive evidence that sleep loss is more detrimental when performing a more difficult task.

If ambient light is more important in certain hours than others, heterogeneity in sunrise and sunset times within a time zone could lead to differential impacts of DST. Sunrise occurs earliest in the Eastern portion of any time zone; in Boston, sunrise the day before DST occurs at 6:07 a.m. whereas in Louisville, Kentucky, it occurs at 7:04 a.m. In Boston, the onset of DST moves sunrise back an hour to roughly 7 a.m. while in Louisville sunrise is moved to roughly 8 a.m. If light is more important for fatal crashes (perhaps due to more driving) during the 7-8 a.m. hour relative to the 6-7 a.m. hour, Louisville should experience a bigger morning increase in fatal crashes (in percentage terms) than Boston.[32] To test this mechanism, I split the sample into an Eastern, Western, and Central third of each timezone.[33]

Table A5 shows the RD results. In contrast to what might be expected based on common commute times, results are quite similar for both areas. Figure A1 helps to elucidate this finding. While the darkened hour in the Eastern portion of time zones has fewer fatal crashes and the brightened hour has more fatal crashes, it is a very minor difference. Further, the average sunset and sunrise times in the Eastern and Western portion of a timezone is closer than the full hour seen in the Boston - Louisville example. This geographic heterogeneity could be explored further in other applications where higher frequency events would increase the power of the test and allow for more narrow geographic areas than one third of a timezone.

[31] 2010 census counts used for county population.

[32] In the evening, sunset shifts from 17:45 to 18:45 in Boston and 18:45 to 19:45 in Louisville. Again, it would appear that Boston is helped more, as 17:45 to 18:45 is more of a peak travel time than 18:45-19:45.

[33] I split each timezone into East, West, and Central thirds based on number of fatal crashes in each portion (rather than by population or landmass).

7 Conclusion

Daylight Saving Time is one of the most practiced policies across the globe, impacting over 1.5 billion people. Despite this worldwide coverage, many of the impacts of DST remain empirical questions. I exploit the discrete nature of transitions between Standard Time and DST, and variation in the coverage of DST created primarily by a 2007 policy change, to estimate the impact of DST on fatal vehicle crashes. My main finding is that the spring transition into DST increases fatal crash risk by 5.4-7.6%.

I employ three tests to determine whether this result is due to shifting of ambient light or sleep deprivation caused by the 23-hour transition date. These tests reveal that while ambient light reallocates risk within a day, it does not contribute to the increase in crashes. All three tests suggest that the sleep deprivation is driving the increase in fatal crashes. Consistent with literature investigating the impact of DST transitions on sleep, the impact persists for the first six days of DST. Back of the envelope calculations suggest that over the ten year study period, DST caused 302 deaths at a social cost of $2.75 billion.[34]

In terms of DST, this result should be viewed as one piece of the puzzle, to be examined in conjunction with research on other impacts of DST. In previous research, when a benefit of DST is found it tends to be through the light mechanism. More light in the evening has benefits at reducing crime (Doleac and Sanders, 2013) and encouraging exercise (Wolff and Makino, 2013).[35] When costs are found, similar to my study, it tends to be due to sleep loss or disruptions associated with transitions (Janszky et al., 2012). Taking these points in combination, an ideal policy solution would leave the benefits of DST intact while eliminating the damage caused by the spring transition. Before a significant policy change is made, further research should be conducted on the welfare effects of the policy.

Finally, this paper fits into the small but growing literature examining the impact of sleep on worker productivity (Kamstra, Kramer, and Levi, 2000; Lockley et al., 2007; Barnes and Wagner, 2009; Wagner et al., 2012). Although fatal vehicle crashes are an extreme measure of productivity, driving is an activity that over 90% of American work-

[34]Social cost is calculated as follows: Multiplying the 5.6% increase found in the FE model by the 489.3 fatal crashes averaged on Sundays-Fridays in March and April yields 27.4 additional fatal crashes per year. Multiplying this by the 1.104 fatalities per crash observed over my sample and the 10 year study period yields and extra 302 deaths over 10 years. Applying the Department of Transportation's $9.1 million value of a statistical life, this a $2.75 billion social cost.

[35]One concern about DST is that morning rise time relative to sunrise time is an important factor in clinical depression (Olders, 2003).

ers engage in (Winston, 2013) and DST provides an exogenous shock to sleep quantity. The increased risk of a fatal vehicle crash suggests significant costs of sleep deprivation, even when undertaking a routine task. Given the ongoing trend towards less sleep, particularly among full-time workers (Knutson et al., 2010), it is important that researchers continue to investigate the relationship between sleep and productivity. My results represent a lower bound for the overall cost of DST through sleep deprivation, since reductions in workplace productivity are unaccounted for.

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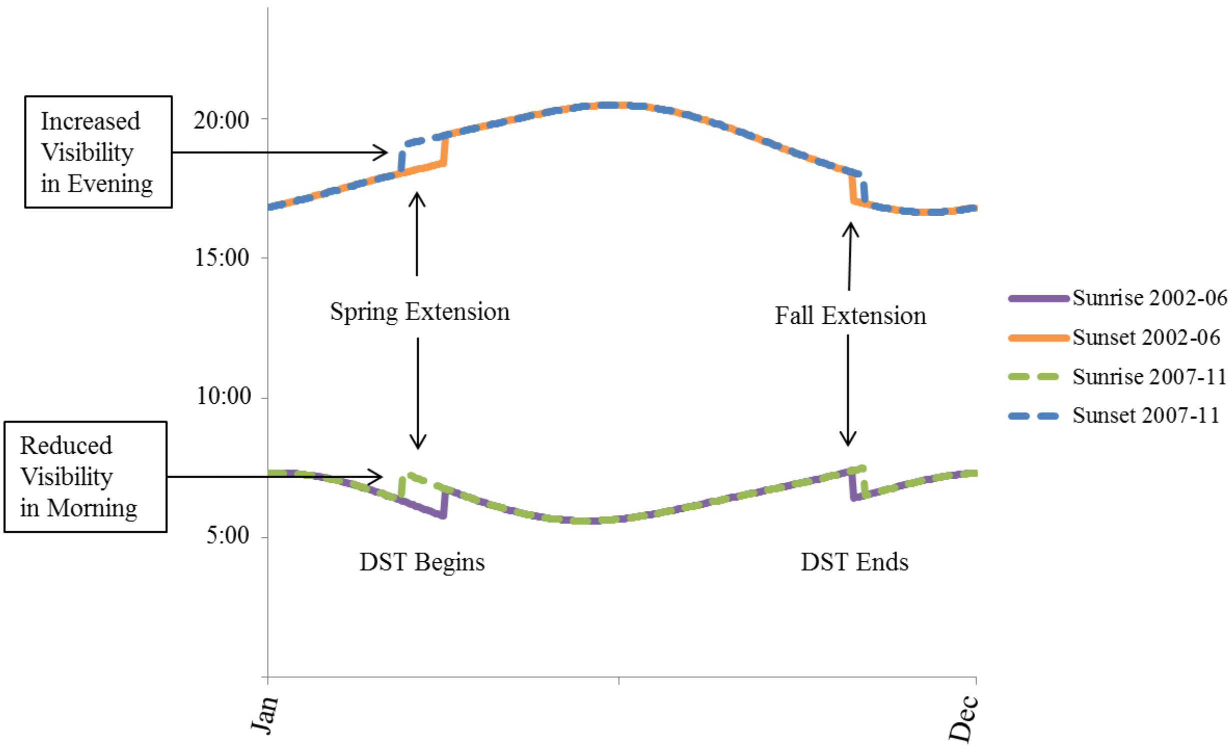
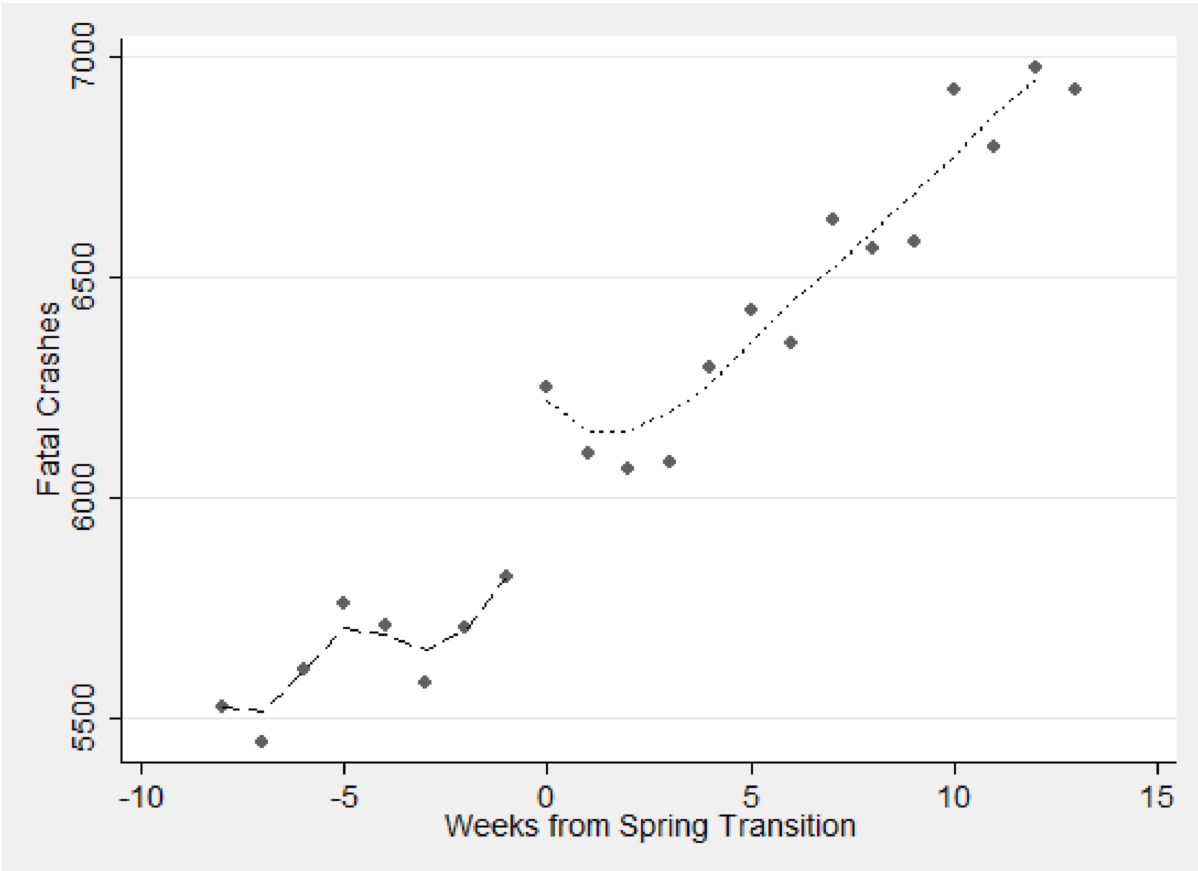
Figure 1: The Influence of Daylight Saving Time on Ambient Light



Note: The sunset and sunrise times are for St. Louis Missouri, the nearest major city to the population center of the US.

Figure 2: Fatal Crashes Around the Spring Transition



Notes: Each point represents the total number of fatal crashes occurring during that week from 2002-2011. Smoothed lines are results of locally weighted regression.

Figure 3: Variation in DST Coverage - Spring

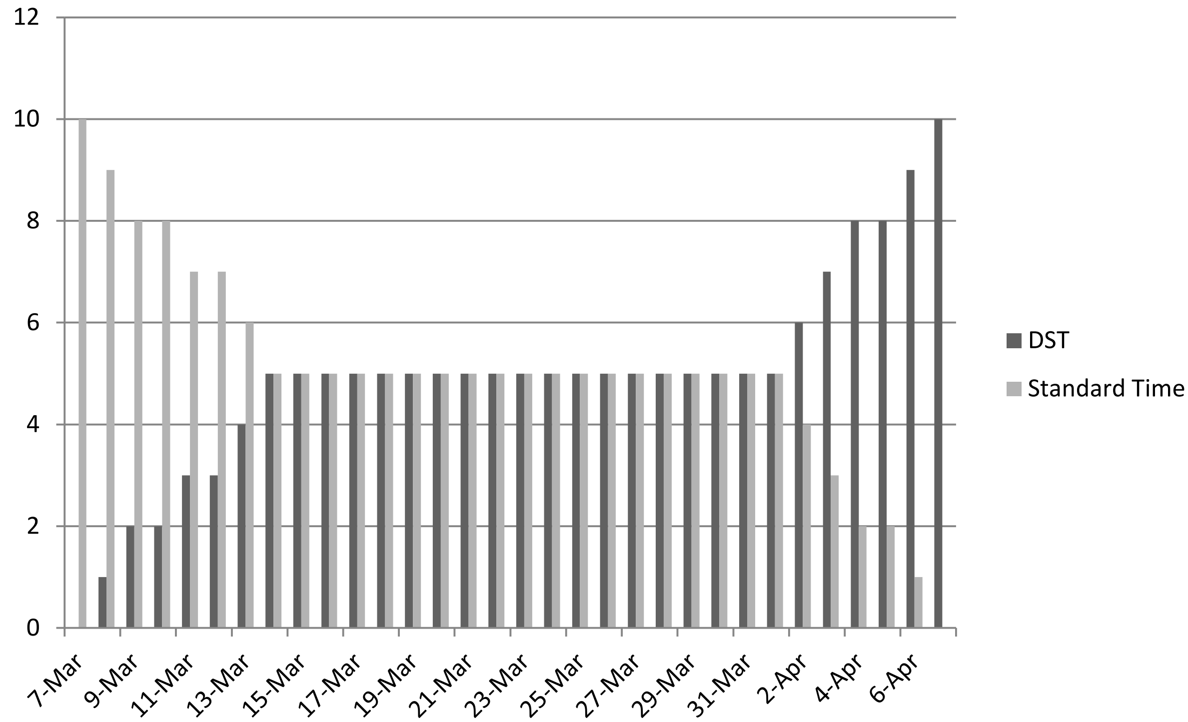
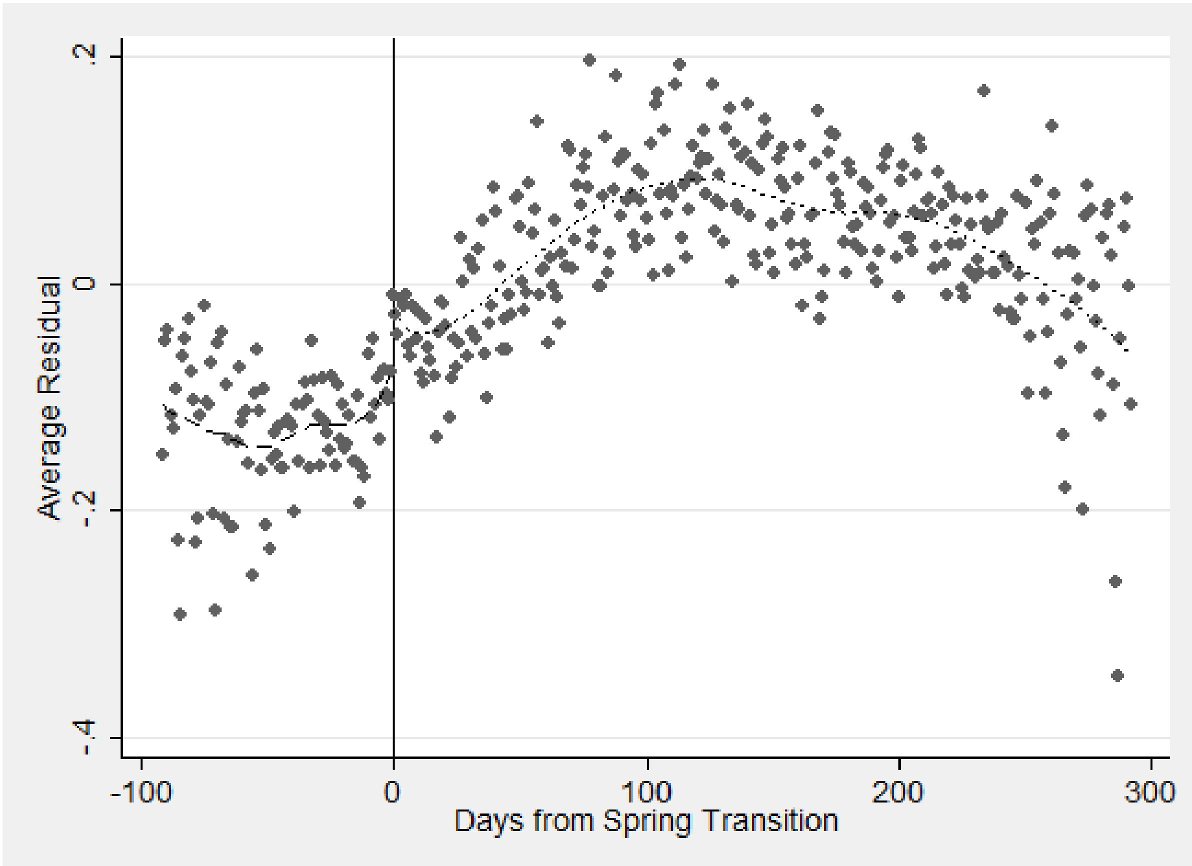
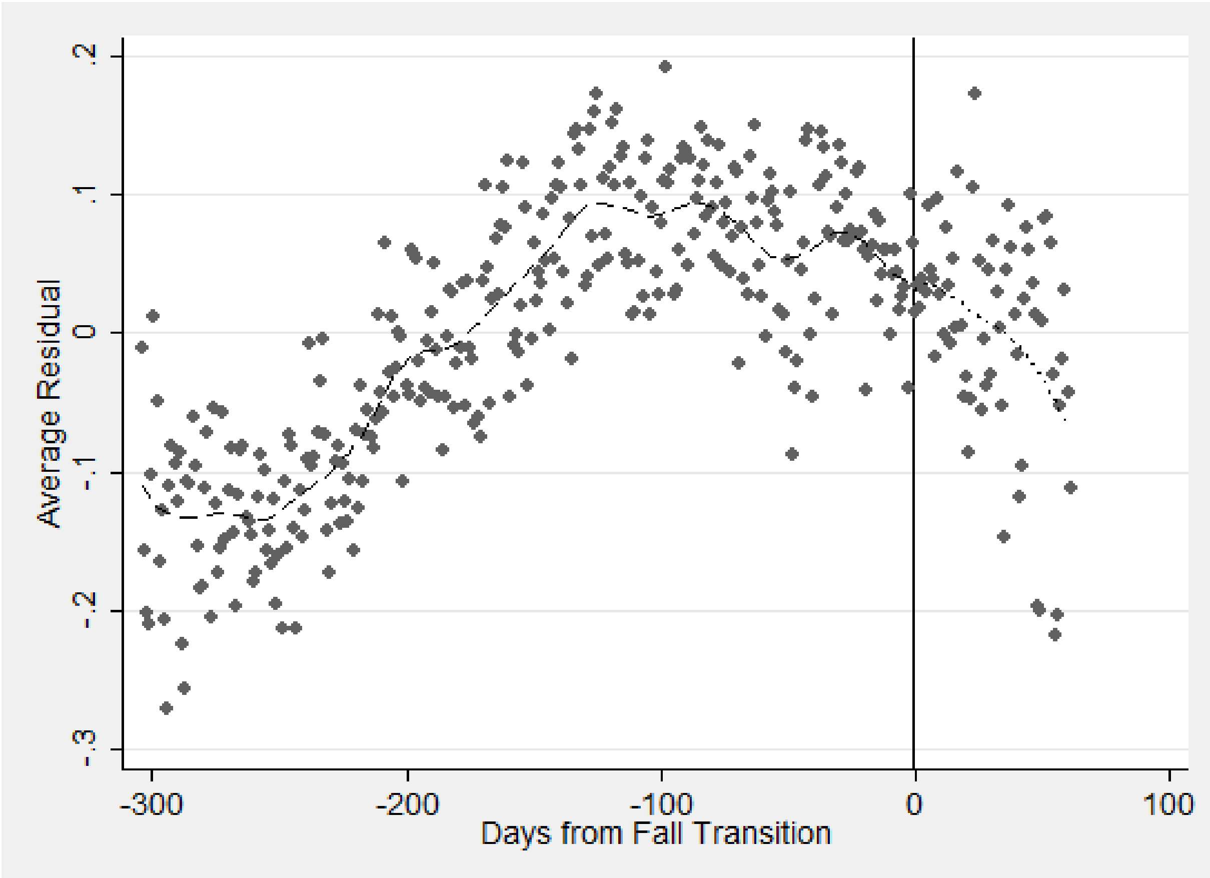


Figure 4: Spring Residual Plot



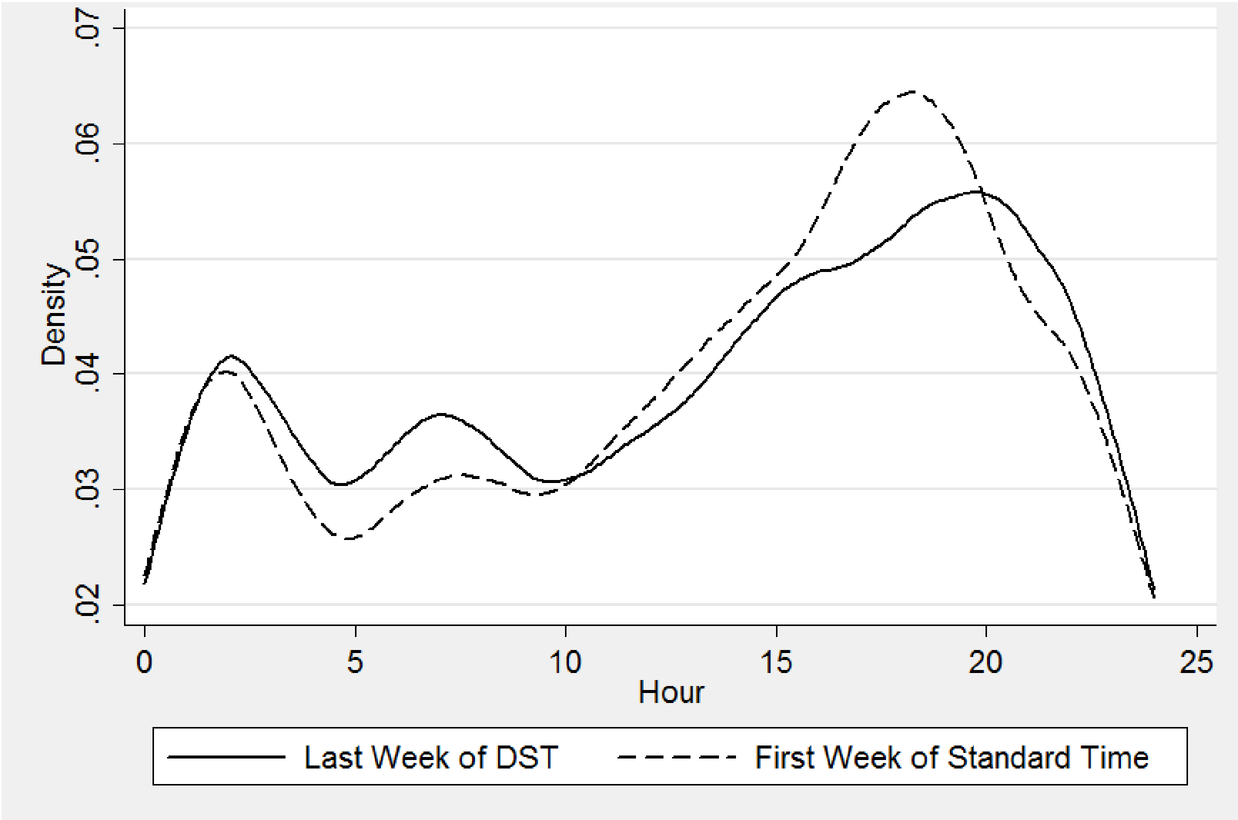
Notes: The residuals are generated from a regression of ln(fatal crash count) on day-of-week and year dummies. Each point is the average of all residuals for that date relative to the spring transition. Fitted lines are results of locally weighted regression. Greater variability on the ends is largely due to these average residuals being formed by only 5 observations rather than 10 towards the middle. This is a product of the 2007 DST extension; in 2002-2006 there are about 14 weeks before the spring transition but in 2007-2011 about 11.

Figure 5: Fall Residual Plot



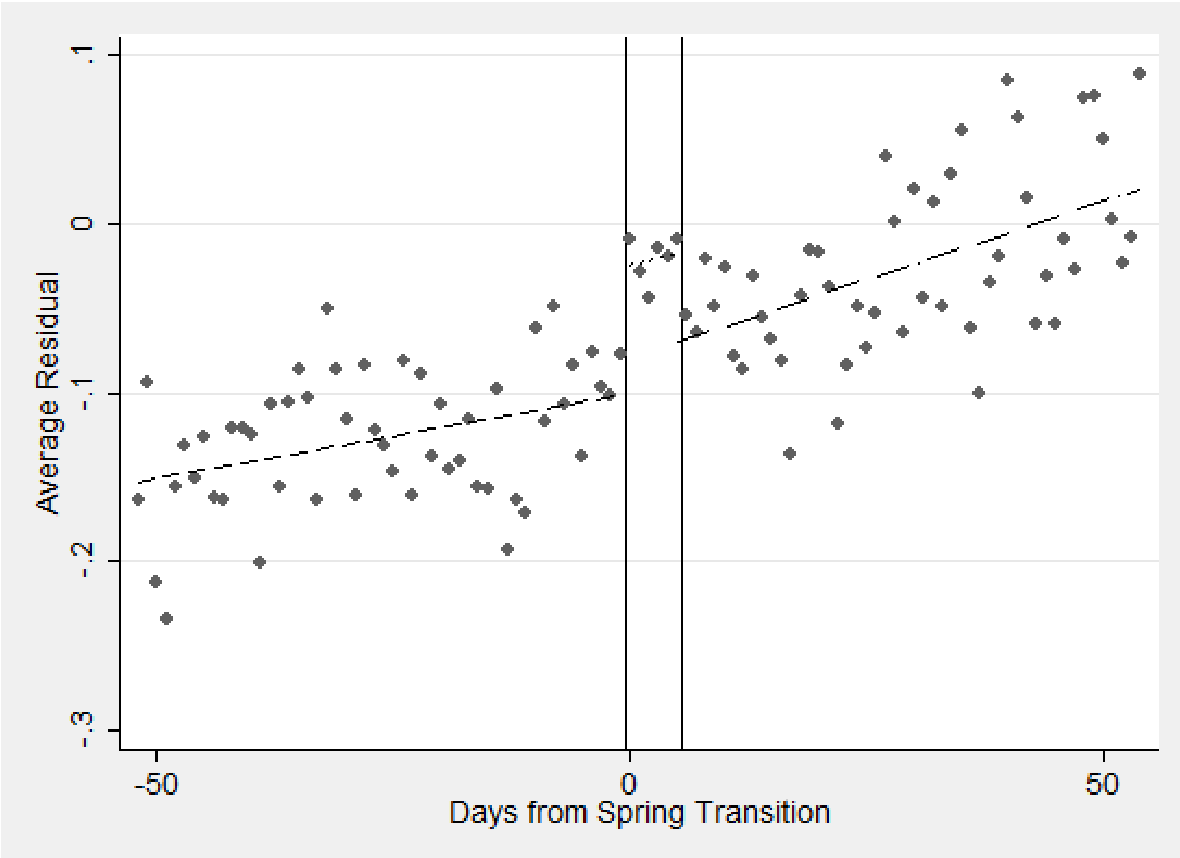
Notes: The residuals are generated from a regression of ln(fatal crash count) on day-of-week and year dummies. Each point is the average of all residuals for that date relative to the fall transition. Fitted lines are results of locally weighted regression. Greater variability on the ends is largely due to these average residuals being formed by only 5 observations rather than 10 towards the middle. This is a product of the 2007 DST extension; in 2002-2006 there are about 9 weeks following the fall transition but in 2007-2011 about 8.

Figure 6: Reallocation of Fatal Crashes (Fall Transition)



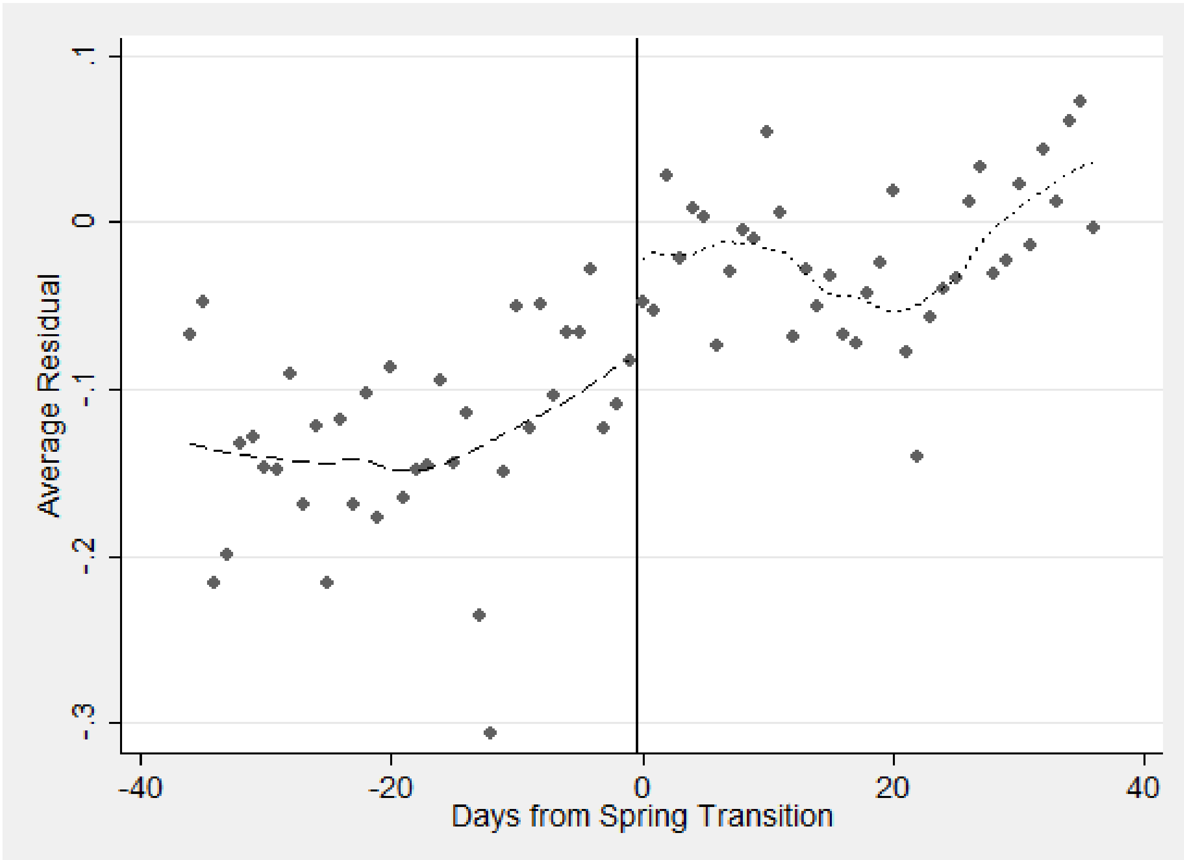
Notes: The kernel density functions use an Epanechnikov kernel. First week of standard time begins on the 25-hour transition date (Sunday).

Figure 7: Spring Residual Plot – Six Day Sleep Impact



Notes: The residuals are generated from a regression of ln(fatal crash count) on day-of-week and year dummies. Each point is the average of all residuals for that date relative to the spring transition. Fitted lines impose linear seasonal trend on residuals.

Figure 8: Spring Residual Plot – Least Light Impacted Hours



Notes: The residuals are generated from a regression of ln(fatal crash count) on day-of-week and year dummies. Each point is the average of all residuals for that date relative to the spring transition. Fitted lines are results of locally weighted regression. Least light impacted hours are 9am-3pm and 8pm-4am.

Table 1: RD estimates of the impact of entering DST on fatal crashes

	(1)	(2)	(3)	2002-2006 (4)	2007-2011 (5)	Placebo (6)
DST	0.0631**	0.0536**	0.0756***	0.0682**	0.0949	-0.0174
	(.0309)	(.0215)	(.0218)	(.0341)	(.0583)	(.0278)
Bandwidth	CCT	IK	CV	CCT	CCT	CCT
# days left	18	41	57	20	12	20
# days right	19	42	58	21	13	21

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys, a first order polynomial and a uniform kernel. DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the spring transition into DST. Placebo assigns the current March transition date to 2002-2006 data and the old April transition date to the 2007-2011 data. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 2: RD estimates of the impact of leaving DST on fatal crashes

	(1)	(2)	(3)	2002-2006 (4)	2007-2011 (5)	Placebo (6)
Leaving DST	0.0018	0.0226	0.0026	0.0189	-0.0233	0.0231
	(.0247)	(.0207)	(.0175)	(.0331)	(.0542)	(.0236)
Bandwidth	CCT	IK	CV	CCT	CCT	CCT
# days left	18	41	62	13	11	18
# days right	19	42	63	14	12	19

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys, a first order polynomial and a uniform kernel. Leaving DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the fall transition out of DST. Placebo assigns the current November transition date to 2002-2006 data and the old October transition date to the 2007-2011 data. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 3: RD estimates of the influence of ambient light on fatal crashes when leaving DST

	All Hours	Morning			Evening		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Leaving DST	0.0018	-0.1631**	-0.1182**	-0.1482**	0.1208**	0.2093***	0.1614***
	(.0247)	(.0703)	(.0555)	(.0657)	(.0506)	(.0499)	(.0323)
Bandwidth	CCT	CCT	IK	CV	CCT	IK	CV
# days left	18	16	30	57	13	61	16
# days right	19	17	31	58	14	60	17

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys, a first order polynomial and a uniform kernel. Leaving DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the fall transition out of DST. "Morning" refers to 4-9am; "Evening" is 3-8pm. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors in parenthesis *** p<0.01, ** p<0.05, * p<0.1

Table 4: RD estimates of the influence of sleep loss on fatal crashes

	All Hours	Least Light Impacted Hours		
	(1)	(2)	(3)	(4)
DST	0.0631**	0.0484	0.0601**	0.0773***
	(.0309)	(.0360)	(.0250)	(.0258)
Bandwidth	CCT	CCT	IK	CV
# days left	18	17	36	57
# days right	19	18	37	58

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys, a first order polynomial and a uniform kernel. DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the spring transition. Least Light Impacted Hours are 9am-3pm and 8pm-4am. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

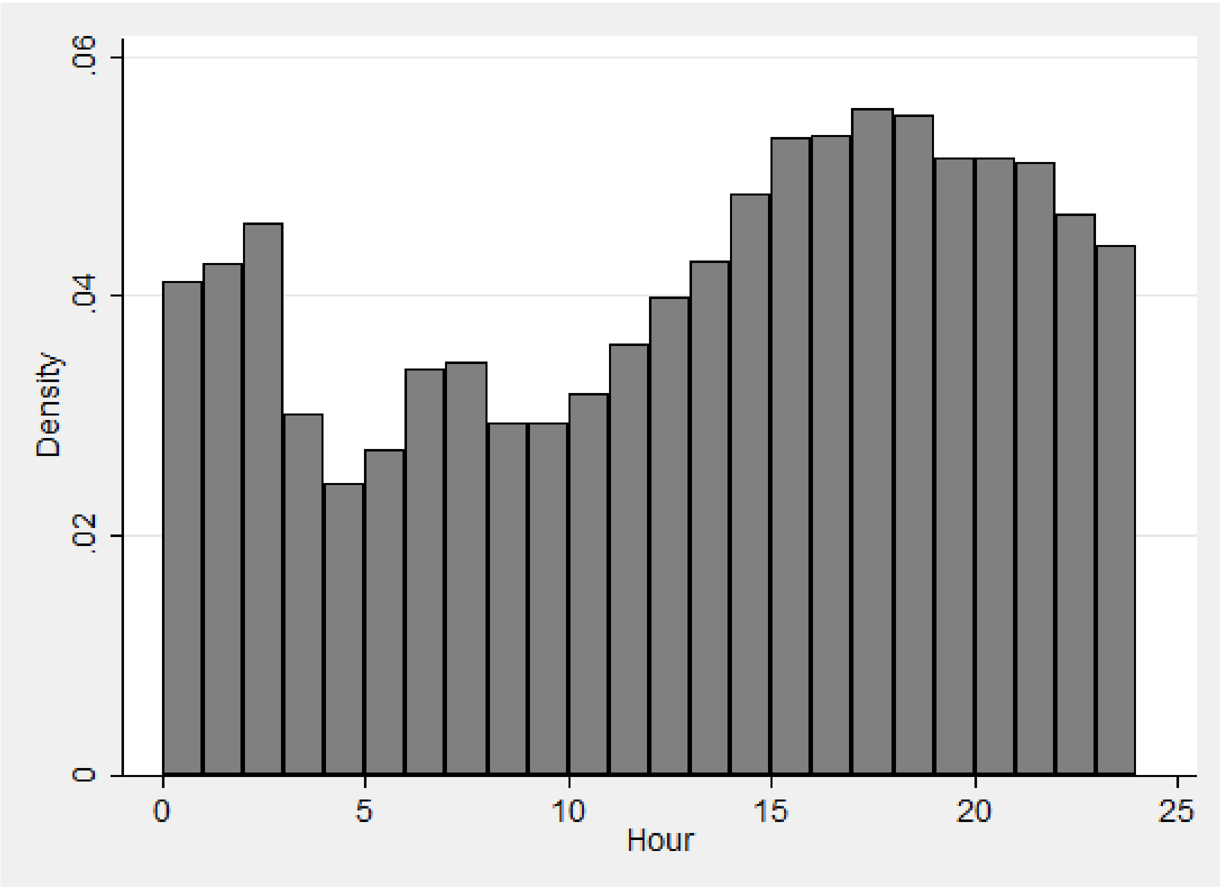
Table 5: FE estimates of the impact of DST on fatal crashes - decomposing spring DST

	All Hours				Least Light Affected	Morning	Evening
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spring DST	0.0340**	0.0335**					
	(0.0164)	(0.0165)					
First 6 Days of DST			0.0559**	0.0570**	0.0481*	0.206***	0.00745
			(0.0231)	(0.0233)	(0.0279)	(0.0525)	(0.0396)
Next 8 days of DST			0.0285	0.0279	0.0385	0.134**	-0.0668
			(0.0200)	(0.0201)	(0.0254)	(0.0539)	(0.0417)
Remainder of Spring DST			0.0181	0.0161	0.00764	0.121**	-0.0260
			(0.0196)	(0.0197)	(0.0231)	(0.0521)	(0.0389)
Fall DST	0.0280	0.0272	0.0272	0.0263	0.0541*	0.225***	-0.120***
	(0.0246)	(0.0245)	(0.0246)	(0.0246)	(0.0320)	(0.0653)	(0.0448)
Additional Controls	No	Yes	No	Yes	Yes	Yes	Yes
Observations	3,652	3,652	3,652	3,652	3,652	3,652	3,652
R-squared	0.755	0.759	0.755	0.760	0.770	0.295	0.421

Dependent Var: Log fatal crashes; all specs use day-of-year, day-of-week and year dummys. Remainder of Spring DST is an indicator variable equal to one if the day occurs after the first two weeks of DST and by April 7th, the final spring switching date. Fall DST is an indicator variable equal to one if the day falls under DST and occurs on Oct 25th or later, the first fall switching date. Additional Controls are ln(gas prices), ln(S&P index) and dummies for nonstationary holidays. Morning refers to 4-9am; Evening refers to 3-8pm; Least Light Affected are the remaining hours. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

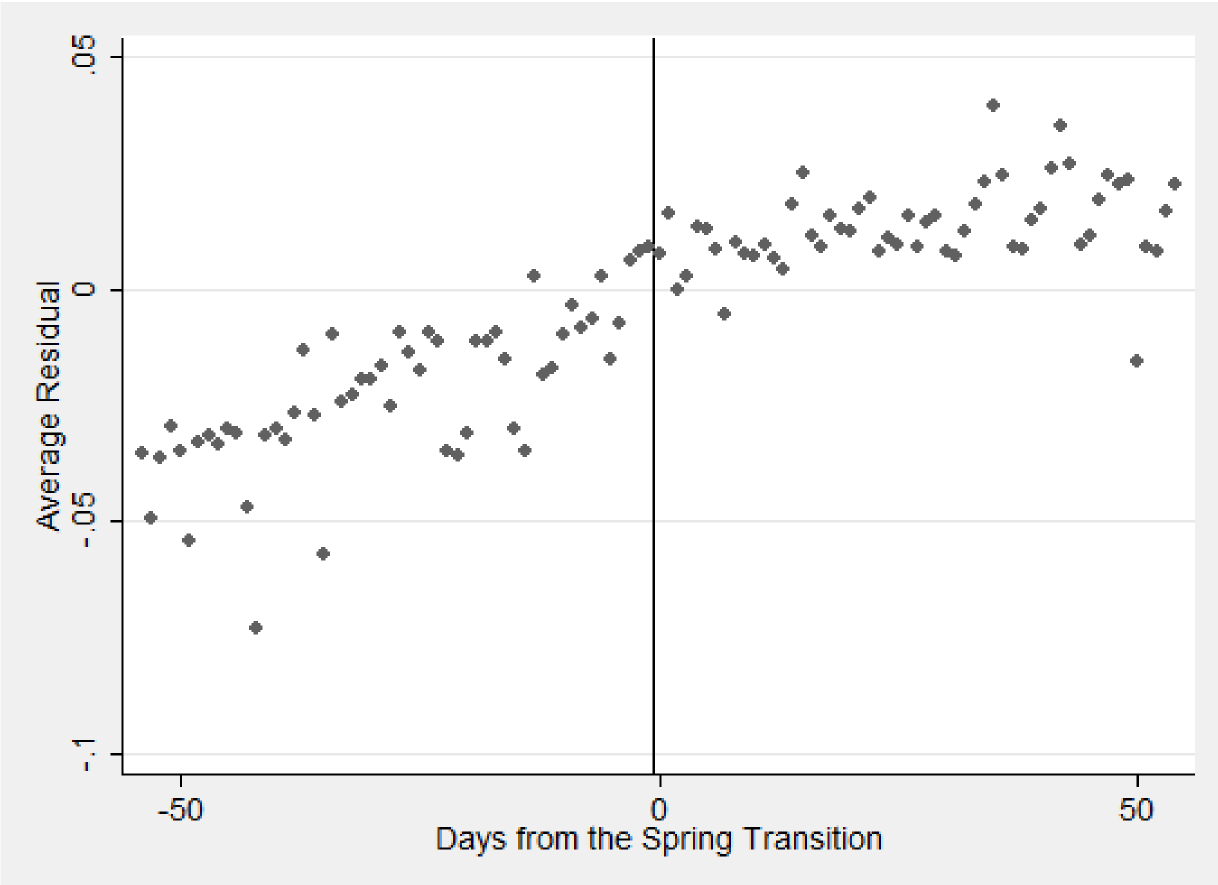
Supplementary Appendix (For Online Publication)

Figure A1: Frequency of Fatal Crashes by Hour



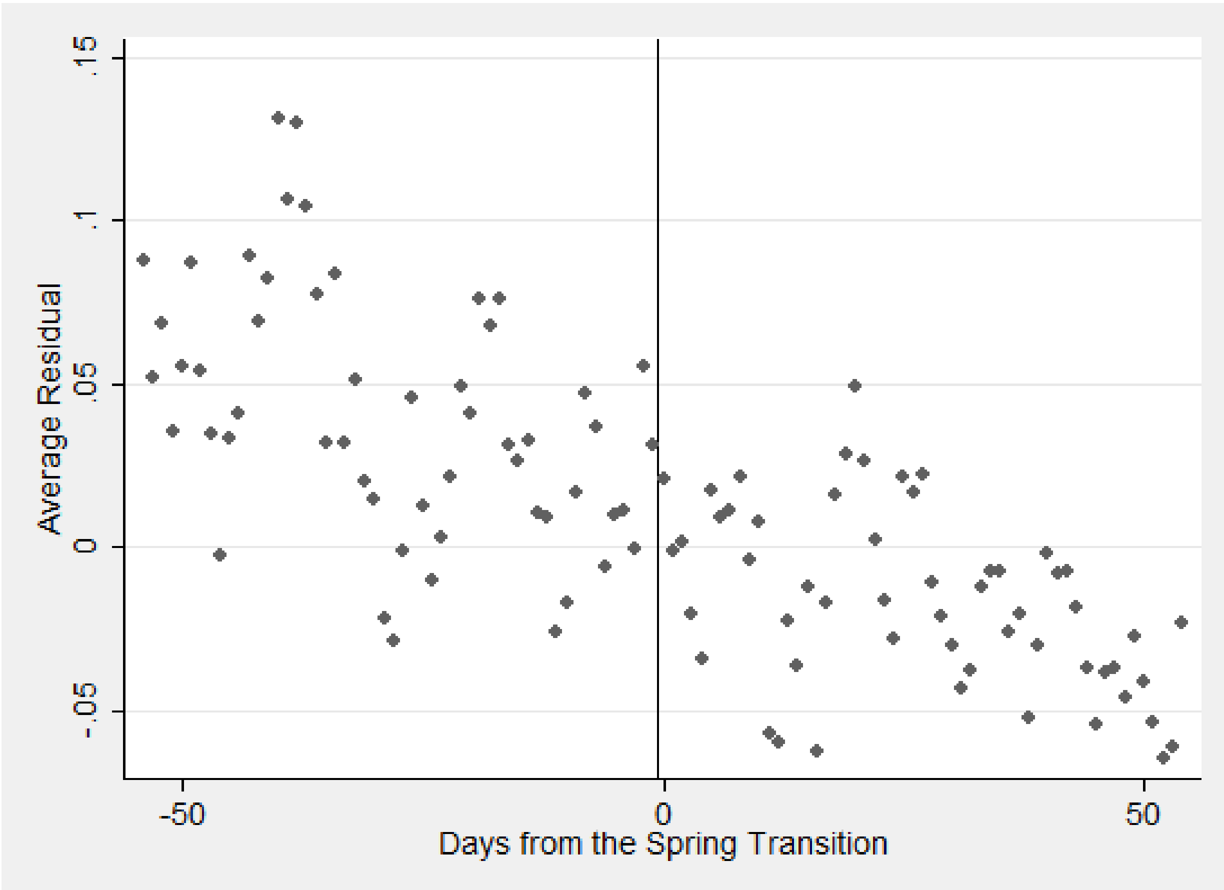
Note: Histogram uses all fatal crashes from 2002-2011 in the contiguous US except Arizona and Indiana.

Figure A2: VMT Residual Plot



Notes: Residuals from a regression of ln(VMT) on day-of-week and year dummies. Aggregate VMT data comes from Caltrans PeMS.

Figure A3: Weather Residual Plot



Notes: Residuals from a regression of Weather Ratio on day-of-week and year dummies. Weather ratio is the proportion of crashes within a day that are impacted by weather.

Table A1: RD estimates of the impact of entering DST on fatal crashes - additional robustness

	Alternative Kernels			24/23rds	No Trans
	(1)	(2)	(3)	(4)	(5)
DST	0.0631**	0.0587*	0.0584*	0.0566*	0.0685**
	(.0309)	(.0314)	(.0312)	(.0307)	(.0340)
Kernel	Uni	Tri	Epa	Uni	Uni
# days left	18	22	20	18	17
# days right	19	23	21	19	17

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys, a first order polynomial and the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the spring transition. Uni refers to a uniform kernel; Tri refers to a triangular kernel; Epa refers to an Epanechnikov kernel. 24/23rds is an alternative correction for the spring transition date where the crash count is weighted as 24/23rds. No Trans drops the spring transition date from the sample. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A2: RD estimates of the impact of entering DST on fatal crashes - global polynomial regressions

	Alt Polynomials				Alt Bandwidth	
	(1)	(2)	(3)	(4)	(5)	(6)
DST	0.0805***	0.0844***	0.0646*	0.0727**	0.0828*	0.0583***
	(0.0299)	(0.0302)	(0.0355)	(0.0299)	(0.0434)	(0.0212)
Bandwidth	30	30	30	30	15	60
Polynomial Order	4	3	5	4	4	4
Additional Controls	No	No	No	Yes	No	No

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys. DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the spring transition. Additional controls consist of national gasoline prices, the S&P 500 index (both in log form) and holiday dummies. Bandwidth is # of days on each side of the transition. Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table A3: RD estimates of the impact of leaving DST on fatal crashes- additional robustness

	Alternative Kernels			24/25ths	No Trans
	(1)	(2)	(3)	(4)	(5)
Leaving DST	0.0018	-0.0099	-0.0062	0.0003	-0.0005
	(.0247)	(.0257)	(.0253)	(.0242)	(.0252)
Kernel	Uni	Tri	Epa	Uni	Uni
# days left	18	21	20	19	19
# days right	19	22	21	20	19

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys, a first order polynomial and the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012). Leaving DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the fall transition out of DST. Uni refers to a uniform kernel; Tri refers to a triangular kernel; Epa refers to an Epanechnikov kernel. 24/25ths is an alternative correction for the fall transition date where the crash count is weighted as 24/25ths. No Trans drops the spring transition date from the sample. Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A4: RD estimates of the impact of entering DST on fatal crashes, by county risk level

	High Risk Counties			Low Risk Counties		
	(1)	(2)	(3)	(4)	(5)	(6)
DST	0.0817	0.0919**	0.1213***	0.0466	0.0352	0.0576**
	(.0530)	(.0417)	(.0421)	(.0346)	(.0222)	(.0237)
Bandwidth	CCT	IK	CV	CCT	IK	CV
# days left	23	50	57	16	42	57
# days right	24	51	58	17	43	58

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys, a first order polynomial and a uniform kernel. DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the spring transition. High and Low Risk Counties are based on a cut at the median county of fatal crashes per capita based on 2010 county population. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A5: RD estimates of the impact of entering DST on fatal crashes-geographical impacts

	Eastern Portion of Time Zone			Western Portion of Timezone		
	(1)	(2)	(3)	(4)	(5)	(6)
DST	0.0737	0.0621	0.784**	0.1066***	0.0525*	0.0726**
	(.0502)	(.0386)	(.0391)	(.0343)	(.0308)	(.0299)
Bandwidth	CCT	IK	CV	CCT	IK	CV
# days left	21	42	57	23	48	57
# days right	22	43	58	24	49	58

Dependent Var: Log fatal crashes; all specs use day-of-week and year dummys, a first order polynomial and a uniform kernel. DST is the estimate of the discontinuity in fatal crashes that occurs immediately following the spring transition. The Eastern Portion of a TZ are the roughly 1/3 of crashes most Eastern based on latitude within a timezone, the Western Portion the same for the West. CCT refers to the bandwidth selector of Calonico, Cattaneo, and Titiunik (2012); IK is Imbens and Kalyanaraman (2012); CV is the cross-validation method of Ludwig and Miller (2007). Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1